Atmos. Chem. Phys. Discuss., 9, C7041–C7043, 2009
www.atmos-chem-phys-discuss.net/9/C7041/2009/

Atmospheric Chemistry and Physics Discussions

ACPD

9, C7041–C7043, 2009

Interactive Comment

Interactive comment on "*Est modus in rebus*: analytical properties of multi-model ensembles" *by* S. Potempski and S. Galmarini

Anonymous Referee #1

Received and published: 11 November 2009

The article presents a mathematical exhaustive insight into what can be expected from ensembles in terms of reduction of uncertainty for prediction. Although I did not find major breakthroughs in the results, it is a rigorous and detailed analytical description of the properties of ensembles, treated from the highest level of assumptions of interdependance between models to the lowest (correlated models, multidimensional). I tried to verify the strangest formulae but really could not find any error. However I did not verify all calculations.

The style may be surprising as there are many formulae and calculations, for which the presentation could be simplified (all "proofs" could be put in then appendix), and simply the results in the main text body, but this is interesting to have the analytical formulations.

I was excited by the analytical formulation given for the ensemble approach but after a few pages disappointed that the paper treated only one aspect of ensembles: how ensemble averages or best linear combination reduces the uncertainty in a prediction. The big story, however, is to see how the variance itself can represent the uncertainty, which actually is the problem of the "blind leading the blinds" stated only in the end. Quite rapidly in the paper the problem is simplified by removing the bias, i.e. one assumes that the mean of the ensemble PDF is the observation. However the question is how, on average, the expectation of the bias is linked to the variance of the ensemble itself. In the first days of ensemble weather forecasting it was hoped that predictability itself could be predicted (see the works of Tim Palmer in the 1990's and R Buizza) on a daily basis. It was hoped that on days when the ensemble spread (the variance) was small the forecast skill was high. NThe spread-skill relation and the use of ensemble in the weather forecasting context should really be presented in the introduction, this is missing.

Apart from that general comment I have only more or less minor comments:

p 14266 l 10: The "political consensus" is really not a good wording. More scientific and accurate terms should be used. What do you refer to? In the IPCC the "political consensus" is not part of the scientific assessment. It is only in the wording of the interpretations that it is used, not in the model definition themselves. More generally most of the discussion included in this paragraph should be reworded in accurate terms to meet the style of the journal, and sentences like "we are not against this process of natural aggregation of scientist ..." have nothing to do in ACP.

p 14267, bullet points 1-4. I was a bit confused by the wording: "Is the ensemble result..." should be "Is the ensemble average result". The term "ensemble" is often used for "ensemble mean", which confuses reading. In bullet 2, authors should specify their goal if "removing" a model (to optimize what?)

p 14268: please define accurately what the "Talagrand diagrams" are

ACPD

9, C7041–C7043, 2009

p 14270 l15-20: "While this condition..." This sentence is not at all obvious. The whole paragraphs in this area are not clear. There should be clear wording and explanations.

p 14271 Eq 2: the variable "y" is not defined (it is probably the observation).

p 14277 l8-12: These are quite trivial statements. What does it bring?

p 14278 Section 4 title and in many places. I am not very comfortable with the expression of "correlated models". In what is described, the "model errors" are correlated, but no assumption is made about the models themselves. If all models of the ensembleare all very close to observation, they should be very correlated while the error may not be.

p 14282 last paragraph. I have been trying to understand why there is a discontinuity between the bounds found for uncorrelated case and correlated case, but could not find any. I verified the calculations which seemed correct. The authors should better explain why for a very slighty correlated models one cannot have a better bound than m+1 (we probably can actually. It would be nice to have an analytical expression which is continuous between coorelated and uncorrelated cases.

p 14284 lines 4-13. This is quite obvious as the estimation is not optimal when not taking into account correlation. Why emphasizing it?

p 14290 discussion in lines 15-20: I am really not comfortable with this concept of removing one or several models to make the model ensemble average closer to observation than the best model. The best linear combination solves this problem.

p 14291 point 2 in the bottom of the page: the authors seem to imply that the only source of errors is the meteorology. How about emissions, boundary conditions in air quality models or dispersion models?

p 14293 l 10: I know many atmospheric modellers who are very good statisticians also. This sentence should be rephrased.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 14263, 2009.

ACPD

9, C7041–C7043, 2009

Interactive Comment