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Interactive comment on "Seeking for the rational basis of the median model: the optimal combination of multi-model ensemble results" *by* A. Riccio et al.

Anonymous Referee #2

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This is a very belated review. I apologize to the authors and the editor for the delay in sending it.

Recommendation : acceptance, subject to a number of improvements concerning the presentation of the evaluation method used in the paper.

General comments

The paper presents a method for evaluation of multi-model ensemble simulation and prediction. The method is based on Bayes? rule, and determines a conditional probability for each of the ensemble models to be true, given the verifying observations. It

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is implemented on a set of 25 simulations of the dispersion of tracers released on the occasion of the ETEX-1 experiment. A number of conclusions are drawn, concerning in particular the relative performance of the various models. The paper is original and instructive. However, if the general conclusions concerning the ETEX-1 simulations are clear, the description of the evaluation method is somewhat confusing, and could I think the paper could be substantially improved. Also, a number of hypotheses are made a priori (for instance, log-normality of the modeling and observational errors, space-time independence of the observational errors), but are not discussed in any way.

Specific comments

1. The presentation of the estimation procedure (section 4) is obscure. It requires random draws from probability distributions (p. 5717, ll. 15-17) while the basic computations (5-7) of a posteriori probability distributions do not include any random component. Clearly, a form of Monte-Carlo method is used there, but no explanation is given of why such a method is considered as appropriate for the problem at hand, nor of what is exactly achieves. These points must be clarified.

2. Concerning the ETEX-1 simulations, important information is missing. How many verifying observations were used ? Which error was assumed on those observations ? (if the evaluation method does not require quantitative specification of observational errors, say it clearly).

3. Uncertainties on the various estimated parameters are given, in Table 1. It is not clear how these uncertainties have been obtained. Are they an ouput of the Monte-Carlo simulations ?

4. The impact of some of the a priori hypotheses made on the errors is not discussed. The assumed independence of models is discussed (I understand that what matters here are not the models per se, but the errors affecting those models). But, as said, the lognormality of errors and the space-time independence of observational errors are not discussed. A full discussion may of course be very difficult, but still the question must

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be explicitly stated and commented that the results obtained may significantly depend on those a priori hypotheses.

5. The authors claim that their paper provides an a posteriori justification for the ?median model? approach introduced and discussed in previous papers. It would be useful to explain, even if succinctly, what the median model approach exactly is. The reference (p. 5721, l. 15), to the APL50 index used in Galmarini et al. (2004a) is unsufficient.

Technical corrections

The presentation of the paper needs in my opinion a number of significant improvements. I am under the impression that the paper consists of parts that have been written more or less independently, and have not been properly harmonized. Here are some specific points to be modified.

6. The paper is inconsistent as to what the reader is expected to know. While some developments describe material that belongs to elementary probability theory (Section 4, Independence and correlation, for instance) and can be expected to be known by the reader of a scientific paper, other parts are technically much more difficult. For instance, the first equation after eqs (3) (that is before Section 4) very succinctly states, without real explanation that the information entropy (the expression is not used) decreases in the estimation (3. Reference is also made, without explanation, to the Kullback-Leibler information divergence theorem. These parts may be difficult to understand, even by readers who are reasonably familiar with probability theory.

Another example is ?Gibbs sampling?, which is mentioned (p. 5717, l. 12) without explanation nor reference. I am not sure many readers will know what Gibbs sampling is.

7. There are inconsistencies of notations. For instance, the notation zetaik is used in section 5 (top of p. 5718) for denoting a quantity that is said to take the values 0 and 1 only (incidentally, it is not said that this quantity, which is called an ?unobserved

latent variable?, and is linked to ?observations drawn from models?, is itself obtained by a random draw). Then, the same notation zetaik is used a few lines further down (eq. 21) to denote a quantity which can take all integer values between 0 and some undefined upper limit N. From what I understand, that new zetaik must be the sum of the previously defined zetaik over N independent draws. I must say all that is totally confusing for the reader.

8. P. 5706, l. 17. The reference to the ?least squares method? (?least variance? would here be more appropriate here anyway), following considerations on bayesian estimation, is ill-placed, It would be preferable to speak of gaussianity first, and to say that bayesian estimation amounts to least variance estimation in the linear and gaussian case.

9. It could be useful to sate explicitly that eq. (18) is the same as eq. (6). And there should no index k on theta in the right-hand-side of eq. (18).

10. Contrary to what is implied in the paper (last line of p. 5710), gaussianity is not necessary for eqs (8) to be valid. Actually, eqs (8) are only a very slightly modified reformulation of eqs (7). Either one of those two sets of equations is sufficient.

11. Figures 3 and 4 should preferably be merged into one figure.

12. P. 5703, l. 3. To be frank, the sentence ?ensemble estimates average out non-predictable components? seems more like wishful thinking thatn like the statement of an objectively established fact.

13. Eq. (24). L is not defined (it is said, on the first line after eq. 26, to be the number of McMC iterations).

Finally, I take this opportunity to respond to the comment submitted by Ivan Kovalets in the open discussion of the paper. It is true that any estimate intended at statistically minimizing the estimation error will in one sense or another smooth extremes. If one wants to know the range of the uncertainty, and the associated extremes, one must

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then use the ensemble an dits spread as a whole, and not (at least not only) as a tool for obtaining a kind of error minimizing estimate.

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