

Response to the Reviewer RC2

I much appreciate the careful balanced thoughts on this manuscript by the present Reviewer.

Here, I emphasize that the present Reviewer does not point to any defect in the methodology adopted in this study. Considering the originality of the methodology, thus, the present manuscript should be accepted for the publication. I frankly admit that the results from this methodology are not a total success. However, the results should be made public for the further common investigations for the progress, rather than bringing them back to my personal drawer.

For this goal, I would also appreciate it, if the Reviewer could elaborate on the matters not clear for me in the following, best, in the interactive mode.

Please note that in the following response, the Review texts are quoted by »...«.

I much appreciate a positive evaluation by the present Reviewer stating that »the manuscript provides a careful and well-done analytical analysis of the Liouville equations and the assumed Pdf approach.« Yet, at the same time, the Reviewer expresses a strong reservation, remarking that it »likely of rather limited practical benefit as also noted by the author.« Unfortunately, I do not understand what the Reviewer means by »limited practical benefit«. Although the Reviewer says that I also note it, the phrase itself is not of mine.

In response, I emphasize the fact that a very solid analysis of the performance is performed in the present study by directly comparing the assumed-PDF results with direct numerical results with the Liouville equation by taking some simple dynamical systems. Providing a general, robust formulation that enables this kind of comparisons is a real originality of the present study: this is not possible with the current existing assumed-PDF schemes, because these are formulated only in a case-by-case manner with only specific applications in mind. Thus the performance of those schemes cannot be tested by simple dynamical systems.

The robustness of the proposed formulation is discussed extensively in YLP (submitted to ACP), which is also currently available online. The goal of the present manuscript is, as clearly acknowledged by the present Reviewer, to test this formulation for more advanced cases.

One may judge that the obtained results are not quite promising, and even a failure. However, one must also count on the basic fact that the assumed PDF attempts something almost impossible: to perform an accurate prediction of a distribution only by using a limited number of parameters. For this reason, one should consider the obtained results are important demonstration of the fundamental difficulties with the assumed-PDF methods in general, not only with a particular approach adopted herein..

All these points will be more extensively discussed in the final manuscript.

-Abstract and Conclusion: Here, the Reviewer objects my interpretation that the tendency for vanishing variance with the present approach is “likely a common cause of collapse in variance found in ensemble-based data assimilation”. I do not know in what sense »This is not founded here and partly misleading«:

it is clearly a common feature that we can identify both with the ensemble-based data assimilation and the present study. Here, I am afraid, the Reviewer slightly misunderstands the standard procedures with ensemble-based data assimilation: as the present Reviewer correctly points out, indeed, they typically »introduce spread/variance through perturbations to the observations and the model« (*cf.*, L582–584). However, this procedure is necessary, because otherwise, the variance would collapse (*cf.*, L574–581), as found in the present study. Somehow the present Reviewer misses this latter point. Please refer to L574–584 of the manuscript.

- Analysis: I am glad to know that the present Reviewer finds that »the analysis is accurately done«.

-The present Reviewer clearly acknowledges that the present manuscript is an extension of another manuscript currently available as a »Technical Memorandum« in EGU sphere (submitted to ACP). The Reviewer states explicitly that »I liked the example/solution part«. Nevertheless, also adds »too long without benefit«, however, with no specific reason provided. More specifically, the Reviewer suggest to drop Secs. 5.2 and 5.3: this suggestion is just odd, because these two subsections pursue the alternative possibilities that partially overcome the defects found with the first model considered in Sec. 5.1. Inclusion of these two subsections is crucial for this reason.

Typos:

-112 “as in values themselves”: will be modified to “as frequency distributions of variables at a single macroscopic point”

Further typos at l24, l46, and l224, and l287 will also be corrected, as well as the errors in the labeling of Figures 7 and 8.