

Dear Nonlin. Processes Geophys. Editors,

This is a letter accompanying our submission of a revised version of Nonlin. Processes Geophys. Discuss., 1, C633–C637, 2014; www.nonlin-processes-geophys-discuss.net/1/C633/2014/ © Author(s) 2014.

“Precision variational approximations in statistical data assimilation” by J. Ye, N. Kadakia, P. J. Rozdeba, H. Abarbanel and J. Quinn

First, we wish to express our appreciation to the referees for their attention and commentary about this paper. Their suggestions, have improved the manuscript, we believe, and for that we thank them.

We have already posted commentary on each of the reviews we received, and this is a summary of the actions we have taken in our revised manuscript to address the questions and concerns of the referees.

A. The main issue both referees had was a request for a much better explanation of the “annealing” process which lies at the core of the paper. We have significantly expanded the discussion of this procedure in Sections 3 and 3.1 of the revised manuscript. We have given detail how each step is performed, and we are hopeful that this expansion of the discussion now allows the reader to better follow the work reported in this paper.

In this expansion, we have clarified the connection between this method and 4DVar which is well known in geophysical applications of data assimilation. Our procedure is a specific sequence of operations, each one of which is equivalent to 4DVar, and this is explained. The advantage of our approach is that we recognize the probability that there are many solutions to the problem the saddle points of the cost function or action, and in standard 4DVar it is hard to know if one has found the lowest level of the cost function or the action. Indeed, we show that when the number of measurements at each observation time is too few—discussed in the paper and the references—then there are many cost function levels close to one another. These split apart as the precision of the model is systematically increased. Further by casting these calculations in the context of integrations over the path of the model variables, we can show when the path with the smallest action level dominates the required integral (Eq. (2)) because the split in action level of that path compared to the action level of other paths is large enough.

The results shown in Figure 1 are a graphic example of this last statement shown within the Lorenz96 model with $D = 20$.

B. The point raised by Referee 1 about the “global” minimum of the action was right to the point. As we have no proof that we have found the global minimum of the action, we have carefully removed all text which might give that impression. As ever, then, though we do have a consistency condition which hints at properties of the minimum action

level we find through annealing in R_f , we test the quality of the assimilation procedure through prediction as in Figure 2.

C. Referee 1 asked what happens when we do not estimate the forcing parameter f in the Lorenz96 equations, Eq. (7). We did this calculation choosing $f = 8.17$ in generating the 'data' for a twin experiment, and $f = 18$ in the model. In this calculation we keep the model f fixed. We include a paragraph indicating that the results are, as one might expect, not very good. However, we chose not to add another Figure for this, hoping the point has been made with a written paragraph and, in the same vein, in Figure (3).

D. Further to comments of Referee 1, we have added explicit noise to the Lorenz96 equations Eq. (7), noting we use that noise to be associated with model errors. Further, we note in the text, in the twin experiment, the data is generated without noise or fluctuations (except for round-off error), then noise is added to those data and used in the assimilation method with R_f finite. At $R_f \rightarrow \infty$ there is no noise at all.

E. With regard to Referee 1's point (5) about why starting with large R_f makes the minimum so hard to find, we thought the comments in Section 4 would suffice along with the reference to Section 4.6 of Quinn's dissertation. That dissertation is now on line and available to all at the University of California's open access website: www.escholarship.edu. If Referee 1 wishes a longer explanation, we would be pleased to provide it.

E. One more comment with regard to Referee 1 (whose overall comments we found very helpful, indeed), that has to do with comment (6). We have made explicit that the annealing procedure is 4DVar at each step, so we felt no further explanation is required about that. Also the two other main points of the paper: corrections to "4DVar" seen as Laplace's method can be evaluated, and the need to have enough measurements (for Lorenz96 $D = 20$, we need L about 8) in order to have the 4DVar path give an accurate approximation to the integral (2): these points are the core of this paper.

F. There was a request to compare this method to EnKF. We could do that, but this paper is totally about properties of the variational method within the Laplace method for data assimilation. It is totally 4DVar and extensions of that. To introduce calculations using other methods of data assimilation, such as EnKF or others, seemed to us to take attention away from the new ideas introduced about 4DVar approaches. In any case there must be many other comparisons between 4DVar and EnKF in the literature, and we did not see that introducing that here would add substantially to the paper.

However, we do plan to analyze the use of the annealing method and the other issues with the extension of variational methods to larger, geophysically more realistic problems. In that context it may be appropriate to bring out a comparison between this

form of 4DVar (annealing, corrections, dependence on number of measurements) as the idea itself will have now been established with the publication of this paper. We also anticipate others will explore this comparison in various models of interest to them.

G. We remain puzzled by comments of Referee 2 about the methods appearing in our earlier work, in particular, Reference 1. We tried to address that in our posted reply in the discussion period. To repeat, however, there is no work whatsoever in Reference 1 to the annealing method, except as there are references to the Quinn dissertation, actually focusing on other matters. We suggested a reason for this perception in our posted comments; hopefully, that will clear up this matter.

H. There were a number of typos, confusions between the path: capital \mathbf{X} , and the model state: lower case \mathbf{x} , and some desire to explain the appearance of the Chi-squared distribution. We have attempted to clarify and correct these matters. In the case of the Chi-squared distribution with both a reference to a standard handbook of statistical methods and with a reference, which we were embarrassed to have forgotten, to work of Bennett and Chua, reported in Bennett's book. Hopefully, these issues are now clarified.

I. We considered the suggestion to replace Precision \rightarrow Precise in the paper's title. After some discussion, we concluded that one of the points of the paper is to evaluate the precision of various variational/Laplace method/4DVar approximations, and that we did not, in a strict sense, have a method that, for all R_f , for example, was precise. Eventually, we have decided to stick with our original choice, though we see no downside to the alternative.

All these comments are embodied in the revised version of our paper submitted to Nonlin. Processes Geophys. Through the website and characterized in the Latex file using latexdiff to make the comparisons easier.

Many thanks,

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