A stochastic covariance shrinkage approach to particle rejuvenation in the ensemble transform particle filter – Second review report

25th February 2022

In the revised manuscript, the authors have corrected the manuscript to take into account the remarks of both reviewers. In particular they have added a couple methodological sections about localisation and convergence, as well as a new set of experiments with the 40-variable Lorenz 1996 model. The additional content overall contributes to the improvement of the manuscript. However, I also have the impression that the authors did not address all the reviewers' concerns.

1 General comments

1.1 Lorenz 1963 test series

This first test series with the Lorenz 1963 model is using the exact same setup as one of the experiments described by Acevedo et al. (2017), hereafter A17. However, the results reported in the present manuscript (Figs. 4 and 5) are different from the ones reported by A17 (left panel of Fig. 7.1). In particular, I noticed the following differences in score:

- SIR benchmark: 2 here vs. 1.5 in A17 (dark red line);
- EnKF: diverged here vs. 2.5 in A17 (cyan line);
- ETPF with 15 particles: 12 here vs. 7.5 in A17;
- ETPF with 25 particles: 6 here vs. 4 in A17;
- ETPF2 with 15 particles: 5 here vs. 3.5 in A17;
- ETPF2 with 25 particles: 3.5 here vs. 2.5 in A17.

How can such differences be explained?

In addition, I would like to come back on the choice of the rejuvenation factor. In general, the optimal rejuvenation factor depends on the ensemble size. For a small range of values of the ensemble size, such as [15, 35] as used by A17, using a constant factor for all values of the ensemble size may be a good approximation. For a larger range of values of the ensemble size, such as [5, 100] as used in the present manuscript, this approximation is less justified. Therefore, I think that the present test series should include a tuning of the rejuvenation factor which depends on the ensemble size, or at least test different values as done by A17.

Finally, I would like to mention that I appreciate the efforts that the authors have put into the improvement of their figures. I have one last remark: the dashed lines can not be distinguished from the plain lines in the legend of Figs. 4, 5, 8, and 9.

1.2 Lorenz 1996 test series

With the linear observation operator, this test series uses a standard and well-documented setup, which is commonly used to assess the performance of new data assimilation algorithms. Once again, the results some results are different from what can be found in the literature:

- the LETKF curve in Fig. 8 does not seem to be correct: the RMSE should be lower than 0.3 with 5 members, close to 0.2 with 10 members, and lower than 0.2 (these scores can be found, for example, in the chapter on the EnKF of Ash et al., 2016, already cited in the manuscript);
- the authors mention that the LETPF does not converge, but Farchi and Bocquet (2018) provide an illustration of the convergence of the LETPF in the exact same setup (red curve in Fig. 16 of their article), with lower RMSE scores as those reported in Fig. 8 of the present manuscript for the LFETPF(G) with 8 and 16 particles.

My intuition is that these differences can be largely explained by the (very restrictive) choice of not tuning the localisation radius and the inflation or rejuvenation factor.

With the non-linear observation operator, the author conclude that the setup is "highly Gaussian". My impression while reading the text is that the purpose of this setup is precisely to be non-Gaussian. If we end up with a Gaussian setup, then this does not provide any added value compared to the first setup. In addition, there is a contradiction with the conclusion of the experiments with the linear observation operator: in a highly Gaussian setup the LETKF outperforms the LFETPF (which is expected and which is not what can be seen in the experiments with the non-linear observation operator).

1.3 General comments on the response to referee 1

In many cases, the corrections described in the answers do not match the revised manuscript (answer to comments L. 17, L. 18, L. 26, LL. 45-46, and L.58). A mistake

can happen, but at this point this is unprofessional and a clear waste of time for both authors and reviewers.

2 Specific and technical comments on the revised manuscript

LL. 17-19 "Recent attempts to apply particle filters [...] methods such as the ensemble Kalman filter." I appreciate this additional discussion about localisation in the particle filter which was needed. However, having this discussion right here seems weird. Indeed, at this point, the particle filter has not been introduced, neither has the weight collapse phenomenon. For this reason, I would suggest to move this discussion to the end of the following paragraph (namely after "Like all particle filters, ETPF is susceptible to weight collapse"). Also please correct the citation to Farchi and Bocquet, 2018.

L. 22 "ETPF transports..." (and many other occurences) "ETPF" is an acronym and not a name, so I would suggest to use "the ETPF" instead of just "ETPF".

L. 42 With the new notation, I think that one should read "$\pi_{Y|X^{\mathrm{f}}}$" instead of "$\pi_{Y|X}$" in Eq. (1).

L. 54 Please define "$\hat{X}^{\mathrm{a}}$" right after Eq. (3) and not later (at the moment it is defined L.65).

L. 102 "Specifically, consider the ensembles the l^{th} state space variables:" I think that this formulation is incorrect.

L. 107 "$\mathbf{T}^\top \mathbf{1}_{N^{\mathrm{f}}} = \mathbf{1}_{N^{\mathrm{f}}}$" $\rightarrow$ "$\mathbf{T}^\top \mathbf{1}_{N^{\mathrm{f}}} = \mathbf{1}_{N^{\mathrm{a}}}$". This is the same mistake as in the original manuscript.

L. 187 "form" $\rightarrow$ "from".

L. 213 "In effect attempt to avoid ensemble collapse" I think that this formulation is incorrect.

LL. 307-308 "As the covariance chosen depends on the dynamical ensemble, these results indicate that a more detailed climatological distribution that varies seasonally might induce an even greater decrease in error." I do not understand the meaning of this sentence. Could it be clarified?

L. 317 "RMSEfor" $\rightarrow$ "RMSE for".

L. 332 "significantly much more significantly skewed" Please reformulate.

LL. 353-354 "[...] is not shown due to space limitations" What kind of limitations does prevent the authors from showing $\mathcal{P}$, which is a 40×40 matrix? If they want to, they could easily add a figure showing $\mathcal{P}$, for example in a similar way as Fig. 6.

3 Specific and technical comments on the response to referee 1

Answer to comment 1.4 In this comment, I mentioned that the discussion on the results and the conclusions are too short. This part of the comment has not been answered. The revised manuscript offers some discussion of the results, but in my opinion this is still not enough. Furthermore, the conclusions of the revised manuscript are even shorter than the original one!

Answer to comment L. 97-99 Clearly my remark was incorrect. I would like to apologise for this.

Answer to comment LL. 125-126 Visibly my remark has been misunderstood. In my opinion, there are two possible ways of rigorously treating the additional information:

1. either P in Eqs. (1)-(6) does not include the climatological information but P in Eq. (21) does include this climatological information, in which case it should be clearly mentioned that P does not have the same meaning in Eqs. (1)-(6) and in Eq. (21);

2. or P in Eqs. (1)-(6) does include the climatological information, but then Eqs. (1)-(6) are incorrect because, as mentioned in the text, this climatological information is ignored before Section 4: the conditional on P should be removed.

Answer to comment LL. 175 Once again, I repeat that "$\mathbf{R}$" should be replaced by "$\mathbb{R}$".