Review of "A Stochastic Covariance Shrinkage Approach to Particle Rejuvenation in the Ensemble Transform Particle Filter"

by A. A. Popov et al.

In order to circunvent the collapse of the weights in the ensemble transform particle filter, this manuscript proposes to generate syntetic particles sampled from a normal distribution with the dynamical particle sample mean and a climatological covariance. Synthetic and dynamical particles are combined using weights from a covariance shrinkage estimator. The manuscript is in general well written and the proposed methodology may be an interesting venue for the particle filter limitations. However the methodology may require further fundamentation and a more extensive experimentation is required to evaluate the proposed "particle rejuvenation" in different regimes with a discussion of the limitations and strenghs of the method. An experiment in a higher dimensional system is also required. I consider this manuscript is suitable for publication in NGP however it requires a major revision.

Some specific points:

1. The discussion of convergence of the method is rather short (Ln 235). In principle, for a large number the dynamical particles the estimator will give weigth 0 to the synthetic particles. Is that correct? Is this the reason of the convergence? The authors should discuss how the estimator varies in the experiments as a function of the number of particles. In a regime with small/medium number of particles, there should be two effects that the methodology should lead to a suboptimal filter. The synthetic particles are sampled from a Gaussian distribution which may deteriorate the performance of the filter for non-Gaussian forecast predictions. In several dynamical systems the prediction covariance varies with time, then the use of prior information with a climatological covariance should give a suboptimal filter. The authors should discuss these limitations and evaluate them in the experiments.
2. A plot and/or a discussion in the experiments about the values given by the shrinkage estimator (which is used as weight in the dynamical and synthetic particles) are required.
3. To my understanding of the methodology, the authors are not using the sampled particles as a rejuvenation of the sequential filter but just to improve the inference step in the ETKF. The synthetic particles are not used in the prediction step, is this correct?. I had in mind that in the ETPF, the rejuvenated particles were used in the prediction step? Could the authors discuss this point in the manuscript?
4. Sampling perturbations from climatological covariances in geophysical applications may give physically unrealistic states. The authors should comment how the method could be extended to be applied in realistic applications.
5. Experiments are rather insufficient. An experiment with a nonlinear observational operator or any other configuration that result in a non-Gaussian posterior distribution would also be

illustrative. Experiments with different observational errors (particularly smaller ones) are also required.

6. The authors may compare the performance of the experiment with a standard inflation technique (with optimal inflation factor) as a baseline. They mentioned in passing that the ETKF deteriorates with inflation, a deeper examination of this point could be useful to further motivate the proposed method.

7. The experiments comparing the Laplacian with Gaussian sampling, and the ones comparing two climatological covariances versus one do not appear to have a conclusion.

8. I had in mind covariance shrinkage as a covariance regularization method for small samples. However, the low-dimensional example shown in the manuscript does not appear to evaluate the regularization of the long-distance correlations. The authors should evaluate at least in a 40-dimension Lorenz-96 the performance of the methodology that they are proposing. In principle one expects a larger impact of the covariance shrinkage estimator in higher dimensional systems.