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Hydrology and Earth System Sciences Discussions

Interactive comment on "Ensemble Kalman filter versus ensemble smoother for assessing hydraulic conductivity via tracer test data assimilation" *by* E. Crestani et al.

Anonymous Referee #2

Received and published: 20 December 2012

The paper investigates the ability of the Ensemble Kalman Filter (EnKF) and the Ensemble Smoother (ES) to identify a heterogeneous conductivity field from tracer data. In a synthetic case study, they disclose and discuss the differences, and provide some insight why the EnKF outperforms the ES. They also test the potential of several marginal transforms of the data states to mitigate the non-Gaussianity of the data that causes a problem for both EnKF and ES.

I find the paper extremely well written, a pleasure to read, and very concise. The study reports potentially valuable results and insight. However, the paper can be made substantially stronger by improving the following issues:

(A) the discussion and conclusions could be supported by some more analysis carried out on the test case results,

(B) the test case variance is relatively small (although sufficient to cause a non-linear problem), why not introduce a second test case with some variations in the geostatistics and a larger variance?

(C) many citations to relevant literature are missing,

(D) some informative details are not provided.

With these issues improved, I see a nice and strong paper emerging from this discussion paper! In the following, I will provide a more detailed substantiation of the above comments.

DETAILED COMMENTS

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(1) line 8: please insert "approximate" before "Bayesian"

(2) page 13092, line 23: please add "marginal" before "transformations".

(3) Figure 1: please do not use an interpolated shading for a cell-wise constant field.

(4) Figure 3, caption: please correct "untrasformed" to "untransformed".

(5) figures 6, 7, 9: please enlarge these figures for better visibility and font size!

(A.1) See comments B.1 and B.2 on introducing scenarios with different variance of log-conductivity.

(A.2) Complete results of EnKF and ES always include the estimation variance maps. It will be interesting to see how good the two methods THINK they are, and check that versus reality. Is the reality within the 95% confidence interval of the conditional mean?

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(A.3) I agree with your statement that the EnKF wins against the ES, because the residual variance decreases from EnKF update to update, so that in the end the remaining fluctuations of concentration and conductivity are small enough to allow the linear and multi-Gaussian approximation. Why do you not analyse and visualize this? For example, you could plot bivariate scatter plots between some concentration value at the expected value of the plume centroid and some conductivity cell, and see how it evolved over time. Initially, you should see a highly non-linear dependence, and an almost linear/bivariateGaussian one in the end.

(A.4) page 13095, last paragraph: please note that the marginal data distributions fix only the marginal distributions, but not the multivariate dependence. The Kalman-Filter/EnKF/ES assumption is CALLED the "Gaussian assumption", but is is an assumption on multivariate Gaussianity. Thus, it is not only the NST that corrupts the cross-correlation, but there is also a remaining non-multigaussian dependence among all states and parameters.

(A.5) Can you plot how close to "beta" or "log-normal" the concentration data are, and how different the distributions are in space and time? This will help to understand much better why the different transforms perform so differently.

(B.1) page 13092: If you hypothesis for the different performance of EnKF and ES is true, then you can teest it by repeating the test case with a much lower and a much larger variance: in the former case the difference should vanish, in the second case the difference should increase.

(B.2) a larger variance is also important to check how far the re-start EnKF can go before it breaks down. This would not mean that you have to double or triple the entire results and discussion section, but some simply summary statistics on the performance under different variances would fully suffice!

(C.1) introduction, paragraph 1 and 2: the reviews cited here are relatively old. Hence, some methods are missing, such as the Method of Anchored Distributions (http://www.agu.org/pubs/crossref/2010/2009WR008799.shtml). It is fully formal, brute-force numerical Bayes. Hence accurate, but reserved for high-performance computing applications. What about mentioning particle filters?

(C.2) page 13086, line 5: A good discussion of the EnKF "abused" for parameter identification is provided by Nowak (2009) in WRR (http://www.agu.org/pubs/crossref/2009/2008WR007328.shtml)

(C.3) as the data transformations play a major role later in the manuscript, they should be brought up in the introduction already. This should trigger citations to Beal et al (2010 in Ocean Science, http://hal.archives-ouvertes.fr/hal-00457540/) and to Schöniger et al. (2012 in WRR, http://www.agu.org/pubs/crossref/2012/2011WR010462.shtml) already in the intro, not only in the discussion.

(C.4) When using data transforms, this is a univariate fix to a multivariate problem. The key problem is not only the marginal non-Gaussianity, but also the multivariate dependence structure between the variables. Therefore, non-Multi-Gaussianity should be brought up in the introduction.

(C.5) To explain the technical difference between EnKF and ES, you can compare those two to Kriging and Sequential Kriging (Vargas and Yeh, 1999 in SERRA, http://link.springer.com/article/10.1007%2Fs004770050047?LI=true). That paper proves the identity between step-wise and one-in-all conditioning for the linear / Multi-Gaussian case. This is an important citation in combination with comment (D.2).

(C.6) page 13091, top: this is called the re-start EnKF, and is well-known from the literature. Please provide citations (e.g., http://www.agu.org/pubs/crossref/2008/2007WR006505.shtml ; http://www.spe.org/ejournals/jsp/journalapp.jsp?pageType=Preview&jid=ESJ&mid=SPE-92991-PA)

(C.7) page 13091, line 25: L/4 is the recommendation by Ababou et al, 1989 in TiPM (http://link.springer.com/article/10.1007%2FBF00223627?LI=true)

(C.8) Particle Tracking with Pollock's method is much older than Salandin et al., 2000. Please add a more fundamental and original citation.

(C.9) page 13094, line 15: this is a known property of the "parameters-only" EnKF and of the restart EnKF: the state update is done by invoking the governing equations with the updated parameters. Therefore, the parameter-state relation is always physical. This will not be the case in the ES. Please state this fact and remind the reader of the references (see C.6 and C.2)

(C.10) Another relevant transformation is "Box-Cox", see Kitanidis and Shen (1996 in AWR, http://www.sciencedirect.com/science/article/pii/0309170896000164)

(D.1) page 13087: please mention explicitly that you have no model error here, as otherwise equation (4) would have to look differently.

(D.2) top of page 13089: Please mention here that: if the model is linear and all states, errors and parameters are multivariate Gaussian, then both EnKF and ES are exact in the Bayesian sense, and would lead to identical results at the end time of data assimilation.

(D.3) Equation (7): How do you compute X_t? From particle tracking? Is it true that you look at purely advective transport, no diffusion or local-scale dispersion? I.e., no particle tracking random walk? If you have diffusion/dispersion: what is the Peclet number?

(D.4) page 13091, line 19: either the "square root algorithm" is a necessary detail (then provide more information), or it is so marginal that you can drop the entire sentence.

(D.5) the number of data used in the test case is not provided. Still, the number of data is relevant to judge how much data is used for how much information gain on the parameters.

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Interactive Comment

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 13083, 2012.

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