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Hydrology and Earth System Sciences Discussions

Interactive comment on "Groundwater flow inverse modeling in non-MultiGaussian media: performance assessment of the normal-score Ensemble Kalman Filter" *by* L. Li et al.

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We thank reviewer #2 for his/her comments to our paper.

As the reviewer points out, the paper is an evaluation of the performance of the NS-EnKF, it goes beyond the seminal paper by Zhou et al., in that a thorough analysis is carried out involving many aspects that could affect the algorithm.

The paper is based on the analysis of a channelized aquifer since it is a paradigmatic case of an aquifer that cannot be characterized by a multiGaussian distribution. Therefore, a good performance of the NS-EnKF in such an aquifer could be considered as a

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very good indicator that the NS-EnKF will work in other non-multiGaussian settings.

We have mentioned that we use the same training image as used in many other papers for benchmarking purposes.

The objective, as already said, is to demonstrate the NS-EnKF, we have reworded many of the statements in reference to the ability of the method to identify structures. Now, the message is more directed towards the performance of the NS-EnKF in a non-multiGaussian setting.

The reviewer is right in that section 4.4 is quite relevant to analyze the limitations of the method. As already mentioned in the response to reviewer #1, the analysis of the impact of the number of piezometers is relevant enough to deserve an analysis by itself. It is clear that there is a number of piezometers below which the characterization is impossible, and, as the reviewer points out this number and its spatial distribution must be related to some specific characteristics of the underlying conductivity field, such as channel widths. We are working on this subject, but we feel that the paper already contains enough comparisons for the detailed impact of the number of piezometers be left out.

We have reworded the sentence “has been recently developed by Zhou et al (2011)” to avoid misleading the readers. Although we were aware of some of the references pointed out by the reviewer, to the best of our knowledge the NS-EnKF proposed by Zhou et al (2011) is the first one in which the normal-score transform is applied to both the parameters and the state variables, all the references in the review apply the normal-score transform to the state variables only. Much of the success of the NS-EnKF is due to the normal-score transformation of the parameters.

The sentence “a continuous implementation of the Bayesian update rule” has been removed. The reviewer is right that, as is, the sentence is too terse and may not be understood. It can be shown that, for a linear filter with Gaussian variables, the updated estimate is the maximum likelihood estimate, likelihood that is obtained by application

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of Bayes' rule.

There was a small mistake in page 6752, line 15, we meant that the log conductivities follow a multiGaussian distribution. It has been corrected.

Regarding the comment about the marginal distributions in page 6752, line 27, it is something already discussed in the papers by Zhou et al. in which it is clearly stated that the marginal transformation only ensures univariate distributions, but it will not make the multivariate distributions necessarily closer to being multiGaussian; Zhou et al also discuss that after the normal-score transformations applied to both state and parameters the forecast equation is even more non-linear than before the transformation, but the strength of the EnKF is, precisely, dealing with non-linear prediction equations. We have added a small sentence at the end of the paragraph recalling these two issues.

Chen and Zhang do not talk about inbreeding, but they do mention the importance that the magnitude of the RMSE and the ES be similar as an indication that the uncertainty characterization by the EnKF is good (see end of p. 1115 in Chen and Zhang, 2006). In any case, we have reworded this sentence.

A reference to CONNEC3D has been added.

Regarding the distance range of the taper function, its main objective is the removal of spurious correlations. The distance between piezometers is important in deciding the shape of the localization function, and for this reason we opted for an isotropic function that goes down to zero at 80 m.

As already mention, section 4.4 could have been a paper by itself, we could not replicate all the analyses made for the other scenarios; however, we did compute the connectivity functions to find out that, as expected, the connectivity is worse reproduced when the number of piezometers goes down. A sentence has been added in this respect.

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It has been clarified that “a sufficiently large number” of piezometric head data are needed for the characterization of the conductivity field.

Reference has been corrected.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 6749, 2011.