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Interactive comment on "Bayesian approach for three-dimensional aquifer characterization at the hanford 300 area" *by* H. Murakami et al.

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This paper explores a method I developed from late 2007 through early 2009. The method introduces some conceptual elements that are promising. However, the version of the method that is used in this paper has several serious technical flaws. The biggest flaw is point 1 below. The flaw is such that it makes the method, as is used in this paper, infeasible. The second serious flaw, point 2 below, is also clearly shown in the results. These issues are being addressed in ongoing research.

The paper introduces two minor changes to the original method. However, both changes are problematic. These are briefly discussed in points 3 and 4 below.

Overall the results demonstrated in this paper are less than satisfactory, due to the

problems mentioned above and discussed below.

1.

The first comment concerns numerical density (ie likelihood) estimation and computational cost. In section 3.3.3 the computational cost is reported to be nine million (9 000 000) forward process simulations, and with this effort the result is, as will be discussed in a moment, not good. The cause of this computational demand is the high-dimensional density estimation component. Density estimation is feasible only for up to, say 5 or 6 dimensions, while the dimensionality of the problems targeted by this method is much higher. Even the demand for 5–6 dimensional densities is too high for really expensive forward models.

If the authors have checked the performance of the density estimator using some known density, they are aware of this problem.

To fix this problem, some radical re-thinking is necessary.

2.

This comment concerns the approach of discretizing the parameter space. This causes boundary problems that have no obvious fix. For example, 'variance' and 'scale' in Fig 7 and 'horizontal scale' in Fig 9 show that the marginal posterior does not approach zero at the boundaries, suggesting that if the range of these parameter components are changed (e.g. widened), the posterior obtained will be very different. The change is not only to the parameter component that appears to be problematic; the posterior of the parameter components that now look good may also change (e.g. change to looking bad). In other words, the phenomenon that the posterior at the boundaries are not negligible means one has not got the posterior yet. I believe the authors have explored this (because they must have tried different ranges for the parameters) and are aware of this problem.

This discritization approach is connected to the density estimation approach discussed

above. Heavy investment in computational power is not a fix. If one assigns a wider range for a parameter, then sampling within the range has to be sparse, risking undetected loss of features. If one samples densely, the range explored has to be narrow, and boundary effect will be pronounced.

3.

The method is designed for the case that the likelihood function is unknown and needs to be estimated non-parametrically. This paper uses a Gaussian likelihood (section 3.1.3, without necessary detail) and justifies the treatment by saying that “we have observed that ... are approximately Gaussian.” It is unclear how the authors have ‘observed’ this. However, the shape of this likelihood can not be obtained by ‘observation’; it requires very intensive simulations. If my point is not clear to the authors, the last author may want to read an email I sent on 24 September 2008 (the paragraph starting with "It’s possible to check whether the output is Gaussian"). That email also explains other problems that arise with the attempt to use a parametric likelihood function.

4.

In section 3.1.1 the likelihood function is decomposed into independent components. This may not increase the precision of the estimation, because the variance of a big chain of product can be huge if every term of it has some uncertainty. For example, if X and Y are two independent random variables (like two of the independent likelihood terms), the variance of their product is

var(XY) = var(X) var(Y) [1 + E(X)ˆ2 / var(X) + E(Y)ˆ2 / var(Y)]

which is larger than the product of the two variances.

This decomposition idea was contained in a number of research notes during the development of the original method but was not implemented, because it does not appear to be the right direction.

Below are a few random comments on the result.

a. The result demonstration places great emphasis on the marginal posterior of individual parameter components. This is somewhat misguided, because one’s goal is to get the ’field’ by inversion. The posterior is at a secondary place in some sense. The paper contains only one figure showing the field obtained by inversion, which is Fig 6. It is a single transect through the modeled 2D domain. I find this demonstration far from sufficient. At least, I would like to see the mean field obtained by inversion and some presentation of the uncertainty.

b. The marginal posteriors shown in Fig 5, 7, 9 show little sign of convergence, which is not surprising given the technical issues mentioned above.

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