

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

### **Anonymous Referee #1**

Received and published: 19 May 2010

#### Specific Questions

In this section are my answers to the specific questions posed to referees.

1. *Does the paper address relevant scientific questions within the scope of HESS?* Yes. Advances in probabilistic inversion for hydrogeologic parameters are well in line with the scope of HESS.

2. *Does the paper present novel concepts, ideas, tools, or data?* Yes. To my knowledge, the method of anchored distributions, while not original to this work, has not yet been extensively applied.

3. *Are substantial conclusions reached?* Generally, yes. I say “generally”, because for

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the most part, this work is an advanced case study.

4. *Are the scientific methods and assumptions valid and clearly outlined?* Generally, yes. However, the authors would provide a better result if they provided a deeper analysis of the evolution of solutions with increased data. In other words, the changes in the posterior distributions due to the number of tests used is mentioned, but not deeply investigated.

5. *Are the results sufficient to support the interpretations and conclusions?* Yes, although consider the answer to question #4 in this context as well.

6. *Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?* With the exception of access to the specific data, which may or may not be publically available, the descriptions are complete.

7. *Do the authors give proper credit to related work and clearly indicate their own new/original contribution?* Yes.

8. *Does the title clearly reflect the contents of the paper?* Yes.

9. *Does the abstract provide a concise and complete summary?* Yes.

10. *Is the overall presentation well structured and clear?* Yes. However, it might be helpful to use the term 2-D in conjunction with the large-scale inversion for  $T$  to remind readers that the large scale inversion is 2-D while the local scale is 3-D. Granted, this is implied by inverting for  $T$  rather than  $K$ , but it would nonetheless be even clearer to add 2-D to the terminology.

11. *Is the language fluent and precise?* Yes. The manuscript is well-written and concise. It was a pleasure to read.

12. *Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?* Yes.

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13. *Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?* Some specific comments below indicate points of clarification. Additionally, as indicated above, I encourage the authors to discuss the changing of the posterior distributions as a function of data included. Furthermore, I think it would be instructive to provide images showing the mean (and/or conditional realizations) of the 3-D K fields estimated. These could be as supplemental online material rather than taking space in the manuscript.

14. *Are the number and quality of references appropriate?* Yes.

15. *Is the amount and quality of supplementary material appropriate?* No supplemental material is provided. If the data and code are available and public, it would be nice to share them, but I certainly do not insist on that. Furthermore, see question 13.

#### Specific Comments

1. The term “Hanford” should be capitalized in the title.
2. Page 2027, line 20: Using sensitivity to locate the anchors raises a couple concerns. Since the sensitivity is a function of the linearization about a current parameter field, it is dependent on a close prior estimate of that field. More importantly, however, it would seem that focusing anchors at observation point locations might lead to overfitting or point calibration. While there is not clear evidence of that in the solutions presented, I urge the authors to consider alternatives so sensitivity for anchor location.
3. Page 2028 (line 26) and 2029 (line 22): The authors may be underestimating total epistemic error by reporting only measurement error.
4. Page 2032 and Figure 5: The fact that adding the fourth test doesn't lead to convergence of the posterior distributions does not seem, to me, to indicate re-

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dundancy. Furthermore, it seems a bit of a stretch to suggest that adding further information from more tests would not likely push the distributions toward convergence. I would be more comfortable if further tests (which it seems would be easily obtained at least in the synthetic case) be added and a more rigorous treatment of the convergence be included.

5. Page 2033 and Figure 6: While some degree of oversmoothing is expected, it seems that the authors may overstate the power of the anchors to “capture local heterogeneity”. While some very broad trends in  $\ln K$  are indicated in the solution, the nature of inclusion-like features in the  $\ln K$  field are entirely missed in the solution. Perhaps a set of specific conditional realizations rather than only the mean  $\ln K$  value along the transect would more accurately depict the level of heterogeneity indicated by the method.

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 2017, 2010.