Hydrol. Earth Syst. Sci. Discuss., 10, C7552–C7554, 2014
www.hydrol-earth-syst-sci-discuss.net/10/C7552/2014/

Hydrology and Earth System Sciences
Discussions

Interactive comment on "Stochastic inversion of sequential hydraulic tests for transient and highly permeable unconfined aquifer systems" *by* C.-F. Ni et al.

Anonymous Referee #1

Received and published: 30 January 2014

The paper describes an application of hydraulic tomography on an unconfined aquifer. First using a synthetic aquifer, then on a real data set sharing many similarities to the synthetic example. In my opinion the paper has some major shortcomings that should be corrected before the paper could be accepted in HESS:

1) In the introduction, hydraulic tomography is presented as "the method" for inverse modeling. There have been many stochastic inverse methods before hydraulic tomography was introduced, and many methods after it; without having to do an exhaustive review of them, the authors should, at least, put hydraulic tomography in the context of inverse modeling in general, and add some comments on why they decided to use this

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

inverse method instead of other (possibly better) methods.

2) The method description is too terse, to the point of being incorrect at some points. For instance, equation (5) does not follow simply after substituting the stochastic variables in equation (1) and taking expected values; there is a need of further manipulation of the resulting equations and some additional assumptions to get to equation (5). For this reason, even if some of the equations and developments can be found elsewhere, the authors must include a much more detailed description of all the derivations needed for the application of the method; i.e., which are the covariances and cross-covariances used to start the iterations? how are they updated to continue iterating? how is the adjoint state equation derived? how are equations (10) and (11) derived?

3) If there is no need to include these derivations because they can be looked up elsewhere, then, what is the novelty of the paper? The authors claim that they have derived a new iterative algorithm but at no place within the paper there is a clear emphasis of which such a novelty is. The authors must state clearly what is the scientific significance of their paper beyond an application of an existing method to a real data set.

4) The example chosen is not particularly challenging: just 800 numerical cells and an exorbitant amount of data. Will the method work in a realistic case with say two orders of magnitude more cells? The variability for K and Sy is not very large either, implying that the approximations needed to compute covariances and cross-covariances would be acceptable. These approximations will fail when the variances are larger.

5) It it is difficult for me to understand the significance of the case study. Which is the purpose of characterizing, at a resolution of 1 m by 1 m, an area of 40 m by 20 m in an aquifer that extends over kilometers? And then, why the piezometric head evolution at the injection wells is not used in the inversion? How come you do not have conductivity measurements either from cores or from slug tests (although given the conductivity of the formation, these might not be feasible), which could provide you either conditioning

HESSD

10, C7552–C7554, 2014

information, or validation data?

6) The case study cannot serve to justify the functioning of the method unless some validation data is included, such as some local measurements of K and Sy.

Some other comments:

The authors insist in describing the method as being able to characterize conductivity at a high-spatial resolution. I beg to disagree, a numerical model with 800 cells is far from being a high spatial resolution model.

I cannot understand why the conductivity variance at constant head boundaries must be zero; it will be zero for heads, but never for conductivities.

Which values did you use to start the iterations in the real case? Did you check if the final results changed for alternative starting values?

You say that all injection tests were done on the same day. Did you wait for stabilization of the piezometric heads in between tests? Can you consider that the boundary conditions remained unchanged throughout?

In summary:

Major revision is recommended addressed to clarify the scientific significance of the contribution, to clarify all mathematical derivations and algorithmic steps of the method, to explain the relevance of the approach against alternative inverse methods, to explain the practical interest that such a localized characterization may have in a large groundwater model, and to include some validation data to provide some credibility to the case study.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 14949, 2013.