

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 #3

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This paper performs a geostatistical inversion of transient hydraulic tomography data in a variably saturated system. To achieve this, the authors use a well-established method (the SSLE by Yeh and co-workers) and use a set of adjoint-state equations for sensitivity analysis that corresponds to the variably saturated transient system. The inversion method has no novelty; the only novelty is to plug in a set of adjoint state equations that has not been used before. The authors apply their code to a synthetic study and to a real data set.

A clear strength of the work is the dual test (synthetic and real) and the straightforward writing that makes the paper easy and fast to read and to digest. However, the nov-

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elty in the paper is not well defined. The authors talk about a new inversion method, but in fact the SSLE is old. There is novelty: not in the inversion method but in the adjoint states. The authors do not make a clear statement whether transient HT in variably-saturated media is novel or not. For example, did Cardiff et al. have unsaturated/transient conditions or not? The correct claim of novelty will have to be redone.

Apart from that, there are parts of the manuscript that can be improved: the test case design is not sufficient in all parts, the style of results analysis is too favorable and some conclusions are premature. I will provide more detailed comment on this below.

DETAILED COMMENTS

- 1.) Abstract line 2: You say this is high resolution. 40x20 cells is extremely small. Also, 4x2 correlation scales in the domain is extremely coarse.
- 2.) Abstract line 3: The inverse model is not stochastic, it is geostatistical. Please correct throughout the entire manuscript.
- 3.) Abstract, line 17: the fact that S is harder to estimate is a well-known old fact.
- 4.) Last line of abstract: this conclusion is too specific. A better conclusion after the discussion at the end of the manuscript would be: HT is relatively insensitive to artificially drawn boundary conditions (which is a known fact) – even under transient conditions (this would be the new fact).
- 5.) Page 14951, 2nd abstract: a citation machine gut with many bullets. If these studies are worth citing, then please cite them for their respective contributions. Just listing a large number of studies is useless. You should use this list to define what is state of the art, and what is actually the novelty of what you are doing!
- 6.) Page 15952, line 11: “stochastic simulations”. There is nothing stochastic about the SSLE. Do you mean “conditional realizations”?
- 7.) Same page, line 22: did Cardiff et al. do transient / variably saturated HT at the

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BHRSite? This is relevant to know in order to judge your novelty.

8.) Page 14953, line 16: you “modified” the SSLE. . . I think you did not. The SSLE is the geostatistical inversion engine, you simply plugged in a different forward and adjoint model.

9.) Page 14995, top: you say that SSLE is minimizing a sum of square errors. Is this correct? I though the SSLE performs a linearized cokriging type of operation, so it accounts also for the geostatistical prior, and there must be a geostatistical regularization term in the objective function, containing the inverse of some covariance matrix.

10.) Same page, bottom: I think the point data K/S are implemented by Kriging, not cokriging. Only for inverting heads, there is a linearized cokriging approach in the SSLE.

11.) Page 13956: I would erase the last sentence of number 2, and then move numbers 4 and 5 before number 3.

12.) Page 14958: S and K uncorrelated: is this a necessary assumption for the method or not? Please derive the equations independent of this assumption, and take the assumption as late as possible. By the way: an adjoint-state sensitivity equation does not need ANY assumptions on correlations, because it follows from a simple perturbation analysis.

13.) Equation 11: I think THIS equation is your novelty. Maybe you provide the details of derivation in an appendix?

14.) Equation 12: this equation seems wrong: you should divide by the product of standard deviations. That means the sum must apply to each squared bracket individually, not to the product of the squared brackets!

15.) Page 14960, lines 15ff: if S_y would be indeed insensitive to the transient head data, then its mean value would remain entirely at the prior mean, and with correlation to K remaining at zero. Please revise this statement.

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16.) Same page, last paragraph: I find this conclusion wrong. It is know that the transient phase is not overly informative for $\log(K)$, but the speed of the transient process mostly informative to the ratio S/K . The reason why steady-state HT is as good for K as transient HT is not the redundancy of transient data, but the different sensitivity.

17.) Page 14961, lines 9ff: if you fix K and S at all well positions, then it is impossible to check how good is your algorithm. For better analysis, please check how good is the estimation when only using K and S alone, and judge the SSLE by the additional improvement over the K & S -based estimate when including the transient HT data on top.

18.) When analyzing the estimate, please plot the correlations of head perturbations and K perturbations, not the correlations of head and K . Heads have a trend due to the boundary condition, and to reproduce this trend is not an achievement of inversion, but of knowing the boundary conditions. The inversion can be good in reproducing the fluctuations about the trend!

19.) Figure 9: the y axis is used poorly. Please boost y , e.g., by plotting only drawdown.

20.) Page 14963: why did you not include the heads at the pumping wells? My personal experience is that they are hardest to match, because they are most non-linear in inversion. Please provide a reason for not including them.

21.) Figures 13/14: the results for K actually look as is an anisotropic correlation (longer correlation in x -direction) would make a lot of good sense. How did you choose your variogram? Which one did you choose?

22.) Page 14966: the increase in computational speed with smaller domain depends on the used solves and iteration schemes. The increase should be at least linear in the number of nodes (is an expensive algebraic multi-grid solver is being used), or even quadratic in the number of nodes if a more standard solver is being used. What software/solver did you use?

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23.) Page 14967: some of these conclusions do not root back to the discussions before. Example: “the lnK estimation error variances are strongly constrained by the constant head boundaries”. There has been nod analysis or discussion that supports this conclusion. Or the point is not made clear enough, so that I have been missing it.

24.) The conclusion contains unnecessary details, such as the formation description and the date of field work.

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