

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

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We thank anonymous referee #3 to provide valuable comments for our paper. These comments are useful to improve the presentation of our study. We will follow the suggestions to revise our paper and make the statements more clear. Several points are critical and need to be addressed in more detail here. The following is the summary of our responses to the comments from referee #3.

Responses to the comments:

1.) Abstract line 2: You say this is high resolution. 40x20 cells is extremely small. Also,

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4x2 correlation scales in the domain is extremely coarse.

Response: In here the high resolution means that the model can resolve the variations of K and S_y between wells. This is different from traditional single or multiple hydraulic tests where an averaged K or S_y value is obtained between wells. We will rephrase the sentence.

2.)Abstract line 3: The inverse model is not stochastic, it is geostatistical. Please correct throughout the entire manuscript.

Response: Yes, cokriging is a geostatistical interpolation method. We did have stochastic approach in calculating the covariances and cross covariances by using the adjoint state method.

3.)Abstract, line 17: the fact that S is harder to estimate is a well-known old fact.

Response: The sentence will be rephrased.

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).

Response: This is correct. Thanks for the suggestion.

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!

Response: We will reorganize the presentation. Thanks for the comment.

6.)Page 15952, line 11: “stochastic simulations”. There is nothing stochastic about the SSLE. Do you mean “conditional realizations”?

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Response: The feature of the adjoint state method in the SSLE is to solve for parameter covariances and cross covariances for nonstationary cokriging weightings. The sizes of matrices refer to the cost of storage and simulations for these parameter correlations. We will rephrase the sentence. Thanks for the comment.

7.)Same page, line 22: did Cardiff et al. do transient / variably saturated HT at the BHRSite? This is relevant to know in order to judge your novelty.

Response: Yes, they did the hydraulic test at BHRS for an unconfined aquifer but used a different inverse model. The information will be added in the text.

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.

Response: We will rephrase the sentences. To make it clear parts of the sentence can be changed to “modified the program (or code) in SSLE.”

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.

Response: We will make it clear. The first sentence is trying to address the concept of parameter estimations by using observed and simulated heads. In the SSLE the residual heads are used.

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.

Response: Thanks for the comment. For direct measurements of K and S_y , the kriging is used to obtain initial K and S_y distributions. The sentence will be rephrased.

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

Response: We will remove the last sentence of number 2. Sentence 3 indicate the conditional mean flow simulation. The conditional mean flow provide solutions for step 4 and 5. Current order reflects the program logic in SSLE model.

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.

Response: Sy and K uncorrelated is not a necessary assumption. We will revise the text.

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

Response: Thanks for the comment. We will insert required equations to make the derivation clear

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!

Response: Thanks for the comment. We calculate the correlation with commercial software MS EXCEL. We will check the equation.

15.)Page 14960, lines 15: if Sy 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.

Response: Thanks for the comment.

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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.

Response: This paragraph will be removed from the paper because a systematical test is required to carry out a strong conclusion. We have here is only a synthetic case with one sampling strategy.

17.)Page 14961, lines 9: 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.

Response: This is good point. We will follow the suggestion to compare the results based on different conditions.

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!

Response: Thanks for the comments. We will revise the Figures based on the K and head perturbations.

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

Response: Thanks for the suggestion. We will change y-axis to be the head 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.

Response: This is one of the reason. The other reason is relevant to practical conditions. We will obtain high head fluctuations and quick head changes at injection wells (see figure 9). Such observations may be influenced by well bore effect. We did not have enough information to judge if the head observations at injection wells can be used for the parameter inversion. We will address these points in the text.

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?

Response: In the case study, we use exponential variogram model associated with an isotropic correlation lengths. The initial correlation lengths for the model is 20m. This length covers at least three wells (or observations).

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?

Response: The conjugate gradient matrix solver was used in the model. In the SSLE model, we did not calculate the cpu times for the matrix solver. The calculation times used here are roughly recorded by checking the computer system clock.

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.

Response: This will be revised in the paper. Thanks for the comment.

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

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Response: These sentences will be rephrased. Thanks.

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