Hydrology and Earth System Sciences Discussions

Interactive comment on "Estimations of tidal characteristics and aquifer parameters via tide-induced head changes in coastal observation wells" *by* Y.-J. Chen et al.

Anonymous Referee #1

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This paper presents an inverse modelling study in which head variations in observation wells induced by tidal variations are used to estimate aquifer parameters (and tidal characteristics in the synthetic study). Modelling is based on an analytical solution from Jeng et al. (2005). The aquifer parameters are optimized using a simulated annealing algorithm. The contribution that this paper claims to make is that is the first attempt to use the analytical expression in an inverse framework. It is my personal feeling that this is only a minor contribution and something that was obviously already intended when the analytical solution was derived. Therefore, I cannot currently recommend this paper for publication. Perhaps when the general comments listed below have

been addressed, the contribution of the paper becomes clearer.

GENERAL COMMENTS

1. Simulated annealing is used as an optimization algorithm. This is a global optimization method that requires considerable computational resources. Is a global search method required for the current problem? Can the problem be solved with local search methodologies? Why did the authors choose to use simulated annealing instead of an optimization strategy from the family of genetic algorithms, which are much more established in the hydrological community and can be made more efficient than simulated annealing?

2. In the description of simulated annealing, it is not described how new proposal points are generated. This should be included.

3. The authors present a synthetic test case for parameter estimation. This can be interesting, but the focus typically is not on the optimal parameters. Instead, it is more interesting to discuss parameter uncertainty and correlation in this context. This can be achieved by presenting confidence intervals for the parameters, and presenting 2D plots of the error landscape. In addition, it would be interesting to generate the data for the synthetic case study with a numerical model instead of the analytical solution. In this case, the relevance of the approximations in the analytical solution can also be evaluated.

4. I could not follow the discussion related to Table 2. The analytical solution is valid for a small range of the shallow water parameter. If it becomes too large, the assumptions are violated, and the solution is not valid. However, if I use the analytical solution both for the forward and inverse simulation, I do not see how such model structural errors can affect the results. This needs more clarification.

5. I think it would also be valuable to present the fit to all measured WWL data from different wells simultaneously. This could give an idea on how valid the assumption of

a homogeneous medium is. Please add and discuss this result.

6. The simulated results show very specific deviations around t=10. What is the reason for this?

SPECIFIC COMMENTS

Page 9161, Line 14. The relative error is negative in Table 1 when the value is higher than the true value. This is not intuitive, and I propose to reverse the sign.

Table 1. Provide actual values as first line in the table instead of the caption. This makes the table easier to read.