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Discussions

Interactive comment on "Technical note: Analytical solution for the mean drawdown of steady state pumping tests in two-dimensional isotropic heterogeneous aquifers" *by* A. Zech and S. Attinger

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The authors present an analytical solution that is used to compute the hydraulic head in heterogeneous two-dimensional aquifers under steady state radially convergent flow conditions. First, an effective solution is presented, which is representative of the mean drawdown for the ensemble of realizations. This solution is then re-written for the case of individual realizations. Both solutions are a function of a radially dependent value of transmissivity, which in turn depends on parameters related to the underlying heterogeneous structure (i.e. variance and correlation length of the transmissivity field).

Different sets of geostatistical models are defined and, for each of them, thousands of realizations are solved numerically. By minimizing the difference between the (ensemble) analytical and numerical solutions, the aforementioned parameters are estimated and compared to the actual values. Moreover, two single realizations are randomly selected and used to test the analytical solution when applied to individual realizations.

By the time of writing this review, prof. Shlomo Neuman had already posted a referee comment. In this comment Neuman argues that the approach used to derive the analytical solution of this technical note (Radial Coarse Graining, RCG; Schneider and Attinger, 2008) presents fundamental inconsistencies. More specifically, Neuman argues that by applying a weighted average to the transmissivity field rather than to the governing stochastic differential equation, the underlying physical principles (i.e. the conservation of fluid mass) are no longer preserved. I have neither the competence nor the scientific authority required to extend the scope of the review far beyond what presented in this technical note. Thus, my review starts with the basic premise (true or false?) that the methodology used in this work (i.e. RCG), which has been published in a highly reputed scientific journal, is based on assumptions that are valid or at least acceptable.

The analytical solutions presented in this manuscript are specific solutions (i.e. for two-dimensional flow) of a more general three-dimensional analytical model (Zech et al., 2015). Thus, their scientific novelty is not compelling. However, given the relative simplicity of this mathematical framework, I think that this work might be of interest to practitioners, as it provides an easy way to estimate information of the aquifer heterogeneity. Thus, I think that the technical note is worthy of publication. However, before being accepted, I recommend that more efforts are placed on analyzing the applicability of the solution when applied to single realizations.

This version of the paper is mostly focused on assessing the accuracy of the estimates

HESSD

12, C3167–C3170, 2015

Interactive Comment

obtained when applying the solution over the ensemble. It is just a personal opinion, but I do not find this part of the document particularly interesting as (i) effective flow parameters have been extensively studied by lots of previous works (e.g. Sanchez-Vila et al, 2006 and references therein) and (ii) the estimation of variance and correlation length from the ensemble is a nice exercise but has no real applicability.

As I said, I think that the real added value of this work is when it is applied to single realizations. Thus, I think that the examples presented in the document are not really exhaustive. For instance, the solution is tested only over a few realizations of set A (Table 1), which has a relatively small variance. What would happen with more challenging realizations (e.g. set C/D or even E/F)? Also, from the two selected realizations we observe some obvious (but still interesting) effect; i.e. when the contrast of transmissivity between the near and far field is modest, almost no information can be inferred whereas when this contrast increases, the accuracy of the estimation also increases. I think that this need to be analyzed in a more rigorous way for instance by using (individually) the whole set of realizations. Scatterplots of T_{well}/T_g vs $\hat{l}/l$ would help to get insight into the range of applicability of the solution and its dependence on the contrasts of transmissivity.

I have also two minor comments: (i) a differential operator is missing in eq.(2) and (ii) I think that set H of Table 1 is never used.

References

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