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Discussions

Interactive comment on "Three-dimensional transient flow to a partially penetrated well with variable discharge in a general three-layer aquifer system" *by* Qinggao Feng et al.

Anonymous Referee #3

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This is my review of the manuscript submitted by Feng, Feng and Zhan to HESS. The manuscript describes an analytical solution for a confined two-dimensional axisymmetric flow problem with three layers with variable discharge rate. The solution appears correct, but not particularly novel. Its difference from several other existing analytical solutions is a technicality (there are many layered analytical flow solutions in the literature).

The authors do not present any data or comparison against reality to justify the analytical solution design. It is easier to re-derive an analytical solution for a given problem than it is to drill a well. If the authors presented field data and used this solution to gain

insight into observed physical behavior for a real-world system, I think the description of this analytical solution could be relegated to an appendix of that paper.

Specific Comments

1) The authors call their solution "three dimensional," but it is only two-dimensional (r and z).

2) Lines 85-109: the authors build up a straw man about how difficult and inaccurate numerical solutions are, to lead into their discussion of how general and robust their analytical solution is. I definitely believe analytical solutions are useful and have their place, but they are not "better" than numerical models. It may be more appropriate to discuss how analytical solutions can be quick to evaluate (but very complex analytical solutions that are essentially "numerical" like this one are often not so quick to evaluate numerically), and therefore can be used in sensitivity analyses to gain insight into physical behavior through inverse modeling problems. Most of the comments about pitfalls related to numerical solution also apply to analytical solutions. The authors performed a double integral transform, and numerically invert both of these transforms. Numerically evaluating an analytical solution that involves two integral transforms can lead to more potentially dubious numerical manipulations than involved in most "numerical models."

2a) How many terms were used in the numerical inverse Laplace and Hankel transform algorithms? (what was the criteria used to ensure the solution had converged?)

2b) What criteria was use to chose the convergence of these series? Fixed number of terms? Was the solution compared with different numbers of terms?

2c) What order were the equations inverted (inverse Hankel first or inverse Laplace first)? Does the solution depend on the order they are inverted?

2d) Many of the terms in the analytical solution involve differences of exponentials or hyperbolic trigonometric functions. Subtraction of very large terms can lead to catastrophic cancellation, was this considered? Were the terms in the solution algebraically manipulated to minimize loss of significance? Could they be written in an equivalent manner that was more accurate than is written in the manuscript?

The authors simply point to Feng et al. (2020) and Liang et al. (2018) and do not discuss any details of the accuracy or convergence of their method for evaluating their "numerical" analytical solution (line 362). I would contend analytical solutions are more finicky and failure-prone than numerical models, so they require more careful scrutiny. The convergence of finite difference or finite element numerical models for solving confined groundwater flow (linear diffusion equations for a homogeneous problem) is pretty well-known and is not going to surprise anyone. Numerical models can also consider: 1) finite wellbore radius, 2) heterogeneity, 3) variable pumping rates, 4) non-linearities (e.g., the equation of state for water). I think the authors could cut down the section that discusses general "problems" with numerical models (lines 85-109) to a sentence. They could also cut down the section that talks about how general their analytical solution is. Both these un-needed sections could be replaced with discussion about the "numerical" details of evaluating their solution, which would actually be useful to someone who was going to try to implement this (the current general discussion about how much better analytical solutions are than numerical solutions in general is not useful).

3) The authors claim they have created a general and useful solution, but they also use an infinitessimal wellbore with no wellbore storage. Wellbore storage is very important, especially if you can have any range of aquifer properties in all three layers. All wells experience wellbore storage to some degree (unless it is a constant-head pumping test), the balance of the volume in the wellbore interval to the formation storage properties indicates whether or not it is significant. This solution will only be correct in the limiting case of small wellbore storage. The authors admit this (lines 636-645), but indicate that that will be coming in the next analytical solution.

4) Variability in the pumping rate is a trivial difference between this solution and other

solutions. Since the solutions is completely linear, Duhamel's theorem can be used to superimpose solutions that are pulses in time, or other combinations of steps on and off. The authors should provide some data or an example where this type of behavior (exponentially declining pumping rate) occurs. It is only included here because it is a simple case to consider in Laplace space, not because it is physically meaningful.

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