Dear Referee 3:

Upon the recommendation, we have carefully replied our manuscript HESS-2020-586 entitled "Three-dimensional transient flow to a partially penetrated well with variable discharge in a general three-layer aquifer system" after considering all your comments. The following is the point-by-point reply to all the comments.

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.

Reply: *From the values of hydraulic parameters for each layer used in this study, one can see that the pumped or unpumped layer is composed of sandy soils or clay soil in nature, thus the observed physical behavior for a real-world system can be gained by using the new derived solution. We also feel that it is necessary to validate the model and the choices of boundary conditions using controlled laboratory experiments and field pumping tests as well in the future.*

Specific Comments

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

Reply: *We have corrected this in the revised manuscript.*

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."

Reply: *We have rewritten the content of L85-109 in the revised manuscript.*

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?)

Reply: *40 terms of the series used in de Hoog algorithm has sufficient accuracy for the inverse solutions. For the inversion of Hankel transformation, the Ogata (2005) method has two free parameters, h, the step size, and N, the number of steps performed, which respectively determine the resolution and upper limit of the integration grid. These can be modified to accurately transform any function that theoretically converges. And we found that h=0.00001 and N=170 are enough for the inverse Hankel transformation in this study. We have clarified this issue in the revised manuscript.*

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?

Reply: *For the inversion of Hankel transformation, the Ogata (2005) method has two free parameters, h, the step size, and N, the number of steps performed, which respectively determine the resolution and upper limit of the integration grid. These can be modified to accurately transform any function that theoretically converges. How to choose these values, and the estimated error of the transform under a given choice, are discussed in detail in the study of Ogata (2005) and the reader is referred to Ogata (2005) for more details. We have also clarified this issue in the revised manuscript.*

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

Reply: *When we derive the general solution, the Laplace transform with respect to time t is applied, and then the Hankel transform with respect to r is carried out, after that we apply the inversion of Hankel transformation to obtain the semi-analytical solution in Laplace domain. One can obtain the final time-domain solution with application of inversion of Laplace transformation. For the inversion procedures, the readers can consult the detailed derivation shown in Appendix A in this study. We have further clarified the inversion order in the revised manuscript.*

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?

Reply: *The method of Ogata (2005) shows good performance for similar exponentials or hyperbolic trigonometric functions (Liang et al. 2018; Feng et al., 2020), one may consult Ogata (2005) for more details. We have tested the accuracy of the method with the classical solution of Hantush (1964) in the revised 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) nonlinearities (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 is than numerical solutions in general is not useful).

Reply: *The details of the accuracy or convergence of our method for evaluating the semi-analytical solution involve the method of de Hoog algorithm (De Hoog et al., 1982) for Laplace transformation and the method of Ogata (2005) for Hankel transformation, the details have been discussed thoroughly in those two references. We have also addressed this issue in the revised manuscript.*

In addition, some sections as suggested will be also deleted in the revised manuscript.

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.

Reply: *The available solutions have been shown that the effect of wellbore storage can only be found at early pumping time. We will study its influence on drawdown response in three-layer aquifer system in future.*

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.

<u>Reply</u>: *The latest literature for an exponentially declining pumping rate test can be found in Chen et al. (2020). In their study, a variable-rate pumping test was performed in a borehole (YLW02) in Yanglinwei Town, city of Xiantao, situated in the Jianghan Plain, Hubei Province, Central China. The upper aquitard of clay and the lower aquitard of silty-clay are separated by the pumped aquifer, actual pumping rate can be expressed by an exponentially decay function.*

Some data or an example with an exponentially declining pumping rate are implemented in the revised manuscript.

On behalf of the authors
Sincerely Yours,
Hongbin Zhan

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