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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Analysis of Groundwater Response to Oscillatory Pumping Test in Unconfined Aquifers: Consider the Effects of Initial Condition and Wellbore Storage" *by* Ching-Sheng Huang et al.

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Review of: "Analysis of Groundwater Response to Oscillatory Pumping Test in Unconfined Aquifers: Consider the Effects of Initial Condition and Wellbore Storage" By Ching-Sheng Huang, Ya-Hsin Tsai, Hun-Der Yeh, and Tao Yang

Review by: Michael Cardiff

This paper is not acceptable in its present format for at least a few reasons. My primary reason is this: the authors have claimed to have used data from oscillatory pumping

tests (data collected at the Boise Hydrogeophysical Research Site (BHRS), by myself and colleagues). Looking at the data they claim to fit, I can guarantee it is not raw data from any of the tests we collected. As far as I am aware, the authors of this paper did not contact any of the primary collectors of this data in an effort to understand it, nor did they apply an analysis strategy that is appropriate. Publishing data that is suspect under the name of the workers from the BHRS (and using a flawed analysis to do so) negatively impacts those who have worked so hard to collect the high quality data available from this site.

The current paper claims to develop a novel method for analyzing fully-penetrating oscillatory tests in which wellbore storage and the water table are taken into account. Applying this model to our data from the BHRS is completely nonsensical because: 1) While the wellbore we pumped from was indeed fully penetrating, the wellbore was packed off above and below our "oscillation zone", meaning that only a 1m interval (partially penetrating) zone served as the pumping interval. This does not fit with the model that has been developed in this paper; also 2) There is no need to consider wellbore storage for the tests performed at Boise because we used a piston to generate the signal within the well (i.e., the oscillating zone was under confined conditions, and we forced water into / out of the formation via piston). For both these reasons, the model the authors have developed is inappropriate for analyzing our data. The authors may have found this out earlier had they bothered to contact any of the field workers who spent such time and effort collecting this data.

With regards to the scientific merit / value of the model itself – I also question whether this model is necessary or useful, and whether it is being considered for reasonable ranges of the given parameters. Consider Figure 5 – Figure 5(b) shows somewhat of a difference from the Dagan and Rabinovich solution at a distance of $\bar{r} = 16$. Given the non-dimensionalization used, this means it is at a distance of 16 well radii. A standard well radius is about 5 cm, meaning that this effect is being observed only at a distance of less than 0.8m from the pumping location. I have never in my life seen wells spaced

80cm apart. A very big well might be 20cm, for which the effect would apparently decay after only 3.2m.

The authors seem to have chosen parameters that are unrealistic for most aquifers. For example, they use a specific yield value of $S_y = 0.1$. Specific yield values in aquifer pumping tests have almost never been measured to be this high (due to delayed drainage), and in the special case of oscillator tests where saturation changes rapidly, it is unlikely even partial drainage will occur. Similarly, many of the other choices in the plots are suspect. Looking at the definition of $\alpha = \frac{r_c^2}{2r_w^2 S_s b}$, for example, I find it hard to understand why the authors have focused on cases such as $\alpha = 1$ and below in Figure 5. Given that S_s is generally in the range of $10^{-5} m^{-1}$ to $10^{-6} m^{-1}$for any natural material, and that reasonable aquifers may be 10-1000m thick, can one imagine any realistic solutions where $\alpha < 1$?

It is also notable that in Figure 8, the confined solution appears to fit the data perfectly well (using the same K and S_s parameters as the unconfined solution, if I am reading correctly) almost exactly as well as the more complex model. This would indicate to me that the details considered in this more complex model matter not one bit, and the water table can simply be considered as a no-flux boundary practically in these tests.

Similarly, Figure 5(a) represents head at the edge of the wellbore itself, which is unlikely to be used in real field scenarios because measurements at the pumping location are subject to numerous nuisance factors (for example, wellbore “skin”, non-darcian flow conditions near the wellbore, inertial effects, etc. So I see no practical reason to consider the variability in this result.

While it is mathematically interesting to derive new PDE solutions, I fail to see the practical application of these much more complex solutions, given that they are still invoking many assumptions / approximations. For example, the authors do not deal with the fact that they are using only an approximation for the water table response (the linearized free surface condition of Neumann), and that realistically oscillatory tests are

likely to be subject to delayed drainage and differing yields as a function of frequency. For all of these reasons I cannot recommend that this paper be published.

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