Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2018-279-RC2, 2018

Hydrology and Earth System Sciences
Discussions

Interactive comment on "Impact of skin effect on single-well push-pull tests with the presence of regional groundwater flow" *by* Xu Li et al.

Anonymous Referee #2

Received and published: 18 September 2018

Review for HESS – hess-2018-279

Title: Impact of skin effect on single-well push-pull tests with the presence of regional groundwater flow

by Xu Li et al.

General comments:

Groundwater tracer experiments are an important tool for the in-situ assessment of aquifer physical, chemical, and biological properties. Among other techniques, single-well push-pull tests (PPTs) have received considerable attention over the past decades for in-situ assessment of aquifer characteristics. Early PPT papers dealing with the determination of regional groundwater flow velocity and porosity (Leap and Kaplan,

1988; Hall et al., 1991) mentioned a "velocity shadow" downgradient of the pumping well, which may adversely affect the estimation of these parameters. However, this issue has not been quantitatively addressed in the literature to date.

In their current manuscript, the authors attempt to fill this gap by producing numerical simulations of PPTs in the presence of a skin effect under regional groundwater flow conditions. As such, I see merit in this manuscript, as it would provide scientists and practitioners with important information on the accuracy of parameters obtained from PPTs conducted under these particular conditions. On the other hand, I see several important shortcomings in this manuscript, which need to be addressed before it may become suitable for publication. My main concerns are listed here, detailed issues are in the specific comments section below.

1. The manuscript currently lacks conciseness in writing and a careful review of the pertinent (including recent) literature (see specific comments 8 and 9). As suggested by the title, the focus should be on the effect of skin effects on PPTs, because this issue has not been addressed quantitatively before. But as is, the results of the COMSOL simulations are presented in an excessively large number of figures. The authors should carefully consider which figures are essential to providing new insights into the skin effect during PPTs (i.e., the main objective of their paper), and consider combining these figures whenever possible. Unrelated figures (e.g., effect of aquifer effective porosity, dispersivity, etc. on PPT breakthrough curves) should be deleted or moved to a supplementary information section.

2. The simulation results are presented in "qualitative" fashion only, i.e., the reader can only visually compare the breakthrough curves and 2-d spatial concentration distributions between different simulations to judge the effect and relevance of the skin effect. To allow for a more quantitative comparison between simulations, the authors could, e.g., compute relative tracer mass recovered by the end of each PPT, or provide a moment analysis for mass distribution in the 2-d plots. In addition, the presented results are conditional with respect to the simulated scenarios. For readers to apply these re-

sults in their own work, a more general (dimensionless) analysis of PPT breakthrough curves would be preferable.

3. An important deficiency of the current manuscript is that the authors never go beyond presenting PPT breakthrough curves and 2-d spatial concentration patterns as affected by skin effects. The central question, how the skin effect affects the estimation of aquifer properties such as regional groundwater flow velocity and porosity estimated from PPTs (which is why PPTs are conducted in the first place), remains unanswered. Without such information, the reader cannot judge the importance of this phenomenon on the results presented in this manuscript, and the relevance of skin effects during PPTs in general. Quantitative information on this issue could be provided, e.g., by applying the model of Hall et al. (1991) to simulation PPT breakthrough curves in an attempt to recover values for regional groundwater flow velocity and porosity, and to compare the latter with respective simulation input values.

4. The current writing style is poor and improvements need to be made both with regard to sentence/paragraph structure as well as grammar. The manuscript should be edited by a native English speaker.

Specific comments:

1. l. 18-33: Abstract: I am afraid that the abstract is not very informative to a general audience, as it is full of unexplained, specific terminology that only an insider to the subject matter may understand. Examples are “dividing streamline”, “skin”, “positive skin”, “negative skin”.

2. l. 22: The sentence “In this study, a new numerical model . . .was established” is misleading. The authors used/adapted the commercially available COMSOL code/model to simulate PPTs in a confined aquifer under regional groundwater flow in the presence of skin effects. They did not develop a new numerical (finite-element) model.

3. l. 39: Here the authors describe PPTs as two-stage (injection/extraction) exper-

iments. Several lines below (l. 43) they revisit this subject and state that a PPT may contain four phases (tracer injection, chaser injection, rest and pumping). Why not combine the two and say from the beginning that PPTs may consist of up to four phases? This would avoid confusion and redundancy.

4. l. 44: The term “rest phase” is an unfortunate terminology in the context of this manuscript. Although I am aware that this term is used in some of the PPT literature, the PPT literature dealing with determination of groundwater flow velocity and porosity prefers the term “drift phase”. The latter term much better reflects the conditions encountered under regional groundwater flow conditions. In addition, whereas the authors mention that “the rest phase is for tracer to diffuse and/or react with the aquifer (if a reactive tracer is employed)”, they fail to mention here that such a drift phase is crucial for the determination of groundwater flow velocity and porosity (Leap and Kaplan, 1988; Hall et al., 1991).

5. l. 73-74: In light of previous findings (e.g., Vandenbohede et al., 2008), I believe that the statement regarding determination of regional groundwater flow velocity is not really supported in recent literature.

6. l. 75: Why a three-well minimum? A gradient may be obtained from two wells given that they are aligned in groundwater flow direction. A better explanation should be provided.

7. l. 84: Here the authors return to explaining PPTs (see comment 3), and now mention three phases. This is confusing and redundant. Why not combine with previous sections (l. 39/44)?

8. l. 90: “that if the solute transport drifted over the location of dividing streamline toward downstream”. First, it is unclear what is meant by “dividing streamline”. Whenever new terminology is introduced, it should be explained to readers at the first instance it is used. Second, more importantly, and to the best of my knowledge, this is not what Leap and Kaplan (1988) have reported! They do not mention a dividing streamline

(beyond which no solute can be recovered), rather they mention a "velocity shadow" downgradient of the well, in which "advective velocity may be slightly less than at a greater distance downstream...". In other words, this is the first mentioning of a "skin effect" during PPTs. Hall et al. (1991) later pick up on this issue of a "velocity shadow". Conversely, Monkmeyer and Netzer (1993) in their comment on Leap and Kaplan's 1998 paper, appear to be the first to consider a dividing streamline and a stagnation point during a PPT pumping phase (see Fig. 1b in Monkmeyer and Netzer, 1993).

9. l. 96: In their review of literature dealing with the determination of regional groundwater flow velocity and/or porosity during PPTs, the authors may want to include recent publications, e.g., by Paradis et al. (2018), Hansen et al. (2016, 2017), Johnsen and Whitson (2009).

10. l. 127: " ...so that the wellbore effect is not a concern." This statement is unclear. The authors should explain "wellbore effect". Do they mean wellbore storage? Again, when new terminology is introduced, it needs explanation at the first instance of use.

11. l. 131: The authors mention the coordinate system used and refer to Fig. 1. But why is the coordinate system not depicted in Fig. 1?

12. l. 134 and 163: Mathematical model of flow and transport: It is not clear to me why the authors present a mathematical model here, and what is new about this model. The flow model does not include equations that would take into account the skin effect in an analytical fashion, not is the model later used to quantitatively assess the COMSOL numerical output. The same holds true for the transport model. Boundary and initial conditions are of course needed to explain the COMSOL simulations performed by the authors, but they could be presented in chapter 3.

13. l. 158: Is parameter "n" in eq. 6 explained in the text? I couldn't find it.

14. l. 180: "..the inner boundary condition inside the well..". It is unclear to which boundary the authors refer to. The well casing?

15. l. 189: "During the rest phase, the solute flux from the borehole into the aquifer is zero,...". I don't agree with this statement. Given that the borehole has a finite dimension in the authors' simulations, there should be solute mass contained in the borehole at the end of the injection phase, and thus at the beginning of the rest phase. This solute should get flushed out of the borehole by regional groundwater flow.

16. Table 1: I couldn't find the skin radius rs in Table 1. Is there a reason not to list it?

17. l. 228: "..progressively refined near the well." It remains unclear how fine the mesh size actually was near the well. Readers wanting to repeat the simulations will need to know.

18. l. 239: Results: (1) The results of the COMSOL simulations are presented in an excessively large number of figures. The authors should carefully consider which figures are essential to providing new insights into the skin effect during PPTs (the main objective of their paper), and consider combining figures whenever possible. For example, Figs. 14 and 16 show PPT breakthrough curves affected by positive and negative skin effects. These two figures could easily be combined into a single figure. Other figures not immediately related to the main objective should be deleted or may be moved to a supplementary document. (2) The results are presented in qualitative fashion only, i.e., the reader can only visually compare the breakthrough curves between different simulations to judge tracer mass recovery. To improve this comparison the authors could, e.g., compute relative tracer mass recovered by the end of each PPT. This would allow for a more quantitative comparison.

19. l. 250: ".. one can see that there is a stagnation point (Sp) located at the dividing streamline (Ds) as shown in Fig.4." This statement and figure are correct, but not new (see Monkmeyer and Netzer, 1993). Also, the term stagnation point is introduced without an explanation. What is the relevance of the stagnation point?

20. l. 270: the effect of resting time: The results of this section are a logical consequence of results from the previous section, where the effect of regional groundwater

flow velocity are shown. Therefore, I would suggest to shorten this section and combine it with the previous.

21. l. 348: “Fig. 14 shows the effects of the skin thickness (positive skin) on BTCs during the pumping phase. One can see that the concentration gets higher at early stage with the increase of rs.” This is not what is shown in Fig. 14, but rather the opposite (shown is highest early-time conc. for rs = 0). In fact, data plotted in Figs. 14 and 16 look identical. I suspect that the wrong set of data was plotted in Fig. 14. Furthermore, this is another example of two figures which could easily be combined into a single figure.

22. l. 381: “Besides, the numerical model of SWPP test can be used to obtain unknown parameters: i.e., regional groundwater velocity, effective porosity, dispersivity, and biogeochemical reaction rates, by fitting to the observed BTCs.” I find this conclusion unwarranted based on the merely qualitative results provided. First, inverse modeling is not a new element, this has been done before to assess parameters from PPTs (e.g., Gelhar and Collins, 1971, Schroth et al., 2001, Vandenbohede et al., 2008). But more importantly, the authors have not provided any data or sensitivity analysis for this approach in their manuscript. It remains therefore unknown (and questionable) if such an inverse modeling approach will yield unique parameters sets with sufficient accuracy.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2018-279, 2018.