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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.

Xu Li et al.

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Thank the reviewer very much for his/her careful check on the manusript. The point to point response can be found in the following. Please note that the referred page number and line number are referred to the marked version in the supplement files. The supplement files include: a marked version of the revised manuscript, a clean version of the revised manuscript, a supplementary material of the revised manuscript, and the response letter to all the referees' comments.

1. According to lines 114-116, the objective of this work is to study the impacts of skin effects on SWPP tests for estimating groundwater flow velocity. However, the article only shows that the skin effect has a significant influence on the shape of the BTC and

on the 2D distribution of concentration. It does not quantify the impacts of skin effects on SWPP tests "for estimating groundwater velocity". Knowing the impact of skin effects on the estimation of groundwater flow velocity (and of transport parameters) is of primary importance for practical applications. For example, in section 4.1 do the authors manage to estimate correctly the velocity in all cases? Ideally the authors should estimate groundwater flow velocity in the different skin configurations and compare it to the real one. At least, they should reformulate the objective of the paper and give an indication of the expected error on the identified groundwater flow velocity.

Reply: To quantify the impacts of skin effects on SWPP tests to estimating groundwater velocity, we have analyzed quantitatively the tracer mass recovered under the skin effect impact, and an error analysis has been conducted for the non-skin model to interpret BTCs obtained from the model with a skin. The results indicate that the skin can produce considerable error for parameter estimations. See section 4.4 and 4.5.

2. I am not sure to understand the reason why the authors show the impact of groundwater flow velocity on BTCs (section 4.1). Groundwater flow velocity affects the BTC and that is why SWPP tests can be used to estimate this parameter. It is interesting to better understand how groundwater flow velocity influences the SWPP test. However, it is not clear how this analysis is related to the objective of the paper (assessing the impacts of skin effects on SWPP tests). In the conclusions, the authors points out that groundwater flow velocity should be considered in order to design the experiment so that as much tracer as possible is recovered. This could be the reason that justifies section 4.1. Nevertheless, it is not explained how it is related to the objective of the paper.

Reply: One objective of this SWPP test is the determination of the unknown ambient groundwater velocity, and we need to know the characteristics and identifiable features of BTCs under different regional groundwater velocity scenarios. Therefore, types of BTCs should be analyzed for different regional groundwater velocity. To interpret this behavior further, we have introduced "dividing streamline" and "stagnation point" to

better understand how groundwater flow velocity influences the SWPP test. In the conclusions, we have pointed out that groundwater flow velocity should be considered in order to design the experiment so that as much tracer as possible can be recovered, and our purpose is to offer a proposal to estimate the groundwater velocity using the equation of Leap and Kaplan (1988). See p.18, lines 345-347 and p. 29, lines 586-590.

3. The same remark of point 2 can be done for the analysis of the impact of the duration of the rest phase, of porosity and dispersivity (sections 4.2, 4.3, 4.4).

Reply: The analysis of the impact of the duration of the rest phase, of porosity and dispersivity (sections 4.2, 4.3, 4.4) have been moved into a supplementary material as references.

4. Too many figures are presented: I suggest the authors to choose only the figures which are relevant to the objective of the article. In my opinion, figures 6, 7, 8, 9 are not necessary. Moreover, figures showing results with positive skin effects could be combined with figures showing results with negative skin effects.

Reply: Previous Figs.6-9 have been moved into a supplementary material as references. Previous Figs. 10 and 12 have been combined into a new Fig. 6 in the revised manuscript, and previous Figs. 14 and 16 have been combined into a new Fig. 9 in the revised manuscript. See p. 53-56, Fig.6 and 7, p. 60-63. Fig.9 and 10.

Technical corrections:

l.23 “the finite-element COMSOL Multiphysics”: add “software”.

Reply: Implemented, we have added it. See p. 2, line 27.

l.27 Dividing streamline: this sentence becomes clear only after having read the article. The authors should explain what they mean by “dividing streamline”.

Reply: We have explained the terminology “dividing streamline” in the section 4.1, and the terminology has been deleted in the abstract. See p. 18, lines 360-363.

l.29-30 I think the sentence is not very clear. It could be reformulated as: “a smaller ratio between the hydraulic conductivity of the positive skin and that of the aquifer formation”. Moreover, there is no need here to write the mathematical symbol\delta.

Reply: Implemented. See p. 2, lines 34-35.

Figure 1 The term “formation zone” is not very clear to me. Maybe it can be changed with “aquifer” or “aquifer formation”. The caption should precise what are S1, S2, S3, S4. The coordinate axes are missing.

Reply: The term “formation zone” has been changed with “aquifer formation”. See p. 10, line 177. S1, S2, S3, S4 have be explained in the caption precisely. The coordinate axes have been added in Fig. 1. See p. 37, line 722-723 and p. 43 Fig. 1.

l. 127 What is the “wellbore effect”? If this is important for the understanding of the paper it should be explained, otherwise it can be removed.

Reply: The term “wellbore effect” has been replaced with “wellbore storage”. See p. 9, line 168.

l. 152 r was already defined at l.147. Reply: Implemented. See p. 10, line 192.

l. 159 I think it should be specified that H is head. Reply: H is the total head. See p. 15, lines 280-281.

l. 200 and l.70 Wang et al (2017) is missing in the references section Reply: Implemented. See p. 35, line 711-712.

l. 270 In the section title, symbol tres should be changed with “the duration of the rest phase”?

Reply: Implemented. See the supplementary material.

Figures 10 and 12: The symbol should be ndelta and not nsigma.

Reply: Implemented. See Fig.6.

Figures 11, 13 Why some of the flow lines are interrupted?

Reply: We have corrected them for 2D horizontal planes. See p. 56, Fig. 7 and p. 63, Fig. 10

l.331 “In contract to”: I would rather say “In agreement with”

Reply: This paragraph containing this phrase “In contract to” has been deleted in the revised manuscript.

Figure 14 is probably wrong: concentration decreases with rs, differently from what is said at line 350.

Reply: We have corrected it. See p. 60, Fig. 9.

Figures 11, 13, 15 and 17: The skin zone is not very evident in the figures. Maybe it could be highlighted with a thicker line.

Reply: We have corrected it. See Fig. 7and Fig. 10.

Table 1: the skin thickness default value is missing.

Reply: We have corrected it. See Table 1.

Please also note the supplement to this comment:
https://www.hydrol-earth-syst-sci-discuss.net/hess-2018-279/hess-2018-279-AC3-supplement.zip

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