Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2019-699-RC2, 2020

Hydrology and Earth System Sciences
Discussions

Interactive comment on "New Model of Reactive Transport in Single-Well Injection-Withdrawal Test with Aquitard Effect" *by* Quanrong Wang et al.

Anonymous Referee #2

Received and published: 17 March 2020

The present work presents a novel analytical treatment of single-well injection withdrawal (SWIW) tests, whereas the impact of mixing in the well and the presence of confining aquitards are considered. I applaud the Authors for the efforts in the derivation of the solution (math not checked) and the commitment to introduce more flexibility in the conceptual model. Yet, I am not sure about its usefulness to other researchers, it is very complicated! Maybe, if the Authors made available a script for the calibration against data it could be beneficial to the usage among practitioners. Regarding the quality of the paper, I see many unclear points or unclear parts. I listed below a series of comments which I hope will made the paper more clear. Moreover, I have some criticisms about the employed sensitivity analysis, which it seems to be a weak one in my personal opinion.

Comment 1: line 15, why put emphasis on the use of Green' function for the extraction phase in the abstract? This leads to think 'what about the other phases?'. I would remove this comment.

Comment 2: line 17: I would replace 'tested by' with 'tested against results grounded on numerical simulations', or something similar, i.e., the numerical simulations results serve as reference values to be matched and do not verify the validity of the assumptions directly.

Comment 3: lines 17-19 'The sensitivity analysis demonstrates that the influence of vertical flow velocity and porosity in the aquitards, and radial dispersion of the aquifer is more sensitive to the SWIW test than other parameters.'. Which sensitivity analysis? The fact that the latter has been conducted is not specify earlier in the text. Moreover, specify which kind of sensitivity analysis you are using. Furthermore, the sentence is rather confusing: it says that the influence of three parameter is more sensitive to the SWIW test, than other. What is the difference between influence and sensitivity? Is it the influence that varies as a function of the SWIW test? . . . I was thinking that are the results of the SWIW (i.e., model output) to be largely sensitive (i.e., influenced by) to the three mentioned parameters (i.e., model inputs), but maybe I am biased by my previous experiences with sensitivity analysis. Please clarify.

Comment 4: line 23 'The new model of this study performs better than previous studies excluding the aquitard effect for interpreting data of the field SWIW test' too general. Please specify which field test you are referring to, since the quality of the novel solution can be worst than previous ones in case the system do not have an aquitard, for example.

Comment 5: lines 49-50 'Another assumption included in many previous models of radial dispersion is that the concentration of the mixing water with the injected tracer is equal to the injected tracer concentration during the injection phase' the sentence is not very clear. What is the mixing water? 'is equal to the injected tracer concentration' of what? Please revise the sentence. Moreover, lines 53-55 ‘This assumption implies that the mixing effect in the wellbore is not considered, where the mixing effect refers to the mixture between the original (or native) water and the injected tracer in the well.’ Now there is the native water which is not mentioned earlier. . . . I can grasp the general idea that there is a difference between the concentration of tracer between the resident water, injected water and water within the well where mixing occurs, but not in a stand-alone manner from these lines (i.e., I need to think about them and deduce that this the implied message). Please clarify, maybe with an additional figure.

Comment 6: line 61 ‘mostly because ADE could not adequately interpret anomalous reactive transport,’ this true when the ADE is used to capture the whole behavior of the system, i.e., as an effective model for all the system behavior to be characterized by a single representative value of advection, dispersion and reaction. Instead, if ADE is finely discretized (i.e., the system heterogeneity is properly detailed) and then (numerically) solved it can fairly well capture anomalous behaviors. Please clarify this point. This is in line with the mentioned superior capacity of effective transport models mentioned afterward (e.g., MMT,CTRW, fADE, MIM) to have a superior capacity in rendering anomalous behaviors of heterogeneous system when viewed as a whole (e.g., spatially integrated BTCs).

Comment 7: line 74 ‘anonymous’ I suppose anomalous.

Comment 8: line 86 ‘Some examples of weak heterogeneity include the Borden Site of Canada (Sudicky, 1988)’ this is just one example, either add others or modify the sentence.

Comment 9: lines 89-96 ‘Second, for moderate or even strong heterogeneous media such as Cape Code site (Hess, 1989) or MADE site (Bohling et al., 2012), the analytical model developed under the homogeneity assumption is also valuable, but in a statistical sense, as long as the media heterogeneity can be regarded as spatially stationary, meaning that the statistical structure of the media heterogeneity does not

vary in space. In this setting, the analytical model developed under the homogeneity assumption is used to describe the (ensemble) average characteristics of an ensemble of heterogeneous media which are statistically identical but individually different. In another word, such an analytical model will provide a statistically average description of many realizations (an ensemble) which are similar to the heterogeneous media of concern, but it cannot provide an exact description for the particular heterogeneous media under investigation’ . . . this made me think that the validation strategy based on the direct numerical simulations is not valid: those simulations are considering directly an homogenous media (with deterministic properties) and NOT the statistical average of the SWIW results across a set of Monte Carlo realizations of the conductivity fields, characterized by either small, middle or large variance. Please clarify this point.

Comment 10: line 99 ‘A schematic diagram of the model investigated by this study is similar to Figure 1 of Wang and Zhan (2013)’ please add this figure and incorporate what mentioned above in comment 5.

Comment 11: Eq.s (1)-(3) I didn’t quite understand the + notation: I would say that the fact that the velocity component is pointing towards the well or in the opposite direction is in the value of (for example) va considering (1), similar for the others velocity components in (2) and (3). I would say that the value of va (and others advective velocities) varies as a function of the SWIW phase. If not va should be the module of the advective component, no? Maybe I am wrong.

Comment 12: Eq. (12a) what’s C0? (12d) there is a θ without subscript, what’s that?

Comment 13: Eq.s (8)-(11) Highlight that in the imposition of the continuity of flux across the well and the formation only the mobile fractions are considered, for who are not familiar with the MIM model?

Comment 14: ‘For instance, if the characteristic length of SWIW test is l and the aquifer hydraulic diffusivity is D=Ka/Sa, where Ka are Sa are the radial hydraulic conductivity and specific storage, then the typical characteristic time of unsteady state flow is around tc = lˆ2/2D. For instance, for a typical lc=10 m, Ka=10 m/day and Sa=10-5 (m-1) (which are representative of an aquifer consisting of medium sands), the value of tc is found to be 5x10ˆ-5 day.' How do the authors determine the characteristic length lc? In my experience this length is typically a function of the aquifer diffusivity, e.g., for tidal fluctuations is idealized coastal aquifer (e.g., homogeneous, infinite lateral extension) there is a proportionality of the kind lc = sqrt(K/S) (see e.g., Guarracino et al., 2012). Moreover, the proposed estimate of 10 m disagree with the results presented in figures 2-3 where the solute travels up to 100 m, suggesting that the influence of the SWIW test is at least reaching that distance. I am not entirely convinced about the fact that push-pull tests can be seen as steady state tests and with the justification provided by the Authors, I leave to the Editor the judgment here. Nevertheless, I agree on the need to simplify the (already complex) analysis choosing the steady state!

Comment 15: line 289, in the comparison against the numerical solution the porosity of the immobile region of the aquifer is zero, why? There is also a general ω=0, to which mass transfer makes it reference? Why zero? Aren't these choices limiting the testing of the proposed solution?

Comment 16: lines 309-310 'As mentioned in Section 3.1, the new model is a generalization of many previous models, and the conceptual model is more close to reality.' Again, too general. This novel solution could or not be closer to reality depending on the specific case.

Comment 17: line 323 'To prioritize the sensitivity of parameters involved the new model' an in is missing (i.e., 'in the new model'). Moreover, the sensitivity is not a property of the parameters (or model inputs), but it is of the output with respect to the parameters. You want to quantify/evaluate the sensitivity of predictions with respect to the diverse parameters. Sensitivity cannot be prioritized, it is what it is and it is dictated by the way a model builds relationship between input(s) and output(s). Then you can prioritize the estimate of those parameters that influence the most the output.

Comment 18: Eq. (19), the definition and explanation is quite obscure. The only clar thing is that it sensitivity is grounded here on the concept of derivative. Then, what is ci? Moreover, the subscript i does not vary at all, what is it? Why there is lj before the derivative?. Furthermore, this equation implies (i) that only variation of a single parameter at time are considered and (ii) it seems that the index associated with a parameter is evaluated around only one value of that parameter. These features prevent the identification of non-linearities and parameters interactions, which are quite likely to occur for the present model. The proposed method is a quite restricted characterization of sensitivity to me, if the model is not expensive I would suggest to use a global sensitivity method: Sobol' indices (see Sobol, 2001) or DELSA (see Rakovec et al., 2014). On this point I leave the final decision to the Editor.

Comment 19: lines 389-390 'The new model is most sensitive to the aquitard porosity and aquifer radial dispersivity' the model results are . . . 'after a comprehensive sensitivity analysis' you discover the previous thing after performing the sensitivity analysis, and it is not the latter that implies the former results; the sensitivity analysis is just a way to quantify the former aspect. Moreover, I would avoid comprehensive, see comment 18.

Reference

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