

This document provides a point-by-point reply to the comments of the anonymous reviewers #2 and #3 regarding the revision of manuscript 10.5194/egusphere-2025-372, Revision 2, and outlines all relevant changes made in the newly revised version.

1. Reply to comments of anonymous reviewer #2

This is a very interesting paper, and highly relevant to HESS's diverse readership. It presents a very complex model that generalizes dual permeability flow in a river bed to bacterial residence in the fluid and solid bulk. It is well written, clearly presented, and overall of high quality. The main aspects I found that need improvement are: 1. The model presentation, specifically clearly presenting how each specific aspect is incorporated within the equations, yet not only in the parameterization part, but also on how each process is depicted as a functional form within the equation. 2. What is the added value of the bacterial parametrization implementation, making the observed results differ from other models like colloidal transport or sorption, as the results themselves are not different in a noticeable way? I'm sure there are differences, but the paper will benefit from outlining them explicitly.

We thank the reviewer for the positive feedback and the comments, which helped to further improve our manuscript. Based on the line numbering, it appears as if the reviewer's comments refer to the original manuscript submitted before revisions. We can happily state that several comments were already addressed during the previous round of revisions. Please find our detailed answers to all comments below.

The specific comments are as follows:

The abstract is well-written and provides motivation for the study, as well as its aims. I would suggest adding the implications of this study, specifically, what are “meaningful WHPA delineation and risk assessments even under extreme hydrological situations such as flood events.”

Lines 35-45 provide an excellent motivation.

As suggested by the reviewer, we clarified the implication of this study by adding the microbial contamination context to the well head protection area (WHPA) delineation and risk assessment under extreme hydrological situations discussion. This can be found on lines 28-29 of the newly revised manuscript, where we now state:

"It enables meaningful WHPA delineation and risk assessments of riverbank filtration sites with respect to microbial contamination even under extreme hydrological and microbial stress situations such as flood events"

Line 48: Does diffusion really play a role for bacteria? Maybe for 0.1 μm , but it will not have any effect on the process of early breakthrough

It was shown previously that particularly for motile bacteria the diffusion coefficient plays a dominant role especially for low-velocity groundwater flow (e.g. Bradford et al., 2014).

Line 62: “secondary energy minimum” is not defined.

Thank you for finding this missing definition. We added a definition of the secondary energy minimum to line 67-69 of the newly revised manuscript. It now reads:

"Colloid attachment under unfavorable conditions is either fast and reversible for colloids that are retained in the secondary energy minimum (i.e. shallow energy minimum for weak colloid

attraction), or slow and irreversible for colloids that overcome the repulsive energy barrier and reach the primary energy minimum (Tufenkji, 2007)"

General remark for the introduction: The introduction is well-written and clear; however, there are not many references to actual experimental results, nor to the possible parameters that may govern these experiments on bacterial/viral migration in porous media. Are there no relevant experiments in the literature?

There are relevant experiments, but to keep it short and concise, in the introduction, we aimed to give only a brief summary of the history of microbial transport, which started several decades ago, and included some recent studies that are directly relevant for the conceptualization of microbial or viral transport as implemented in our model. Instead of mentioning them in the introduction, we refer to recent experimental studies that estimated microbial transport parameters in Section 4.2.1, where we introduce and describe microbial transport in the illustrative riverbank filtration example (lines 5214-527 of the previously revised manuscript).

Line 118: "by a modified form of Richards' equation." What are the modifications? I know that a detailed model will be presented later, but a brief preview of where we differ from the Richards equation can make the following part of the model easier to understand.

The original Richards' equation was developed for flow in unsaturated soils. The modified form was developed to apply it to variably-saturated soils, where some portions of the soil can be fully-saturated, for example below the water table where water storage is a function of the specific storage of the soil, which does not appear in the original Richards' equation. The modification is the addition of specific storage to the storage term in Richards' equation. Some earlier studies that used the modified form include Cooley (1971), Neuman (1973), Huyakorn et al. (1984), as well as Therrien and Sudicky (1996) who present the model on which HydroGeoSphere was initially based. Since the primary focus of this study is the implementation of microbial transport rather than the specifics of variably-saturated flow, we considered that discussing modifications to the Richards' equation is beyond the scope of this work, but we included a note regarding the modification of the specific storage in the saturated zone along with a reference to the complete formulations (to Therrien and Sudicky, 1996) on line 136-137 of the newly revised manuscript. For information, the references for Cooley (1971), Neuman (1973), Huyakorn et al. (1984) are listed below but they have not been added to the paper.

Cooley, R.L., 1971. A finite difference method for unsteady flow in variably saturated porous media: Application to a single pumping well. *Water Resour. Res.* 7(6): 1607-1625.

Huyakorn, P.S., Thomas, S.D. and Thompson, B.M., 1984. Techniques for making finite elements competitive in modeling flow in variably saturated porous media. *Water Resour. Res.*, 20(8): 1099-1115.

Neuman, S.P., 1973. Saturated-unsaturated seepage by finite elements. *Proc. Am. Soc. Civ. Eng., J. Hydraul. Div.*, 99(HY12): 2233-2251.

Line 130: "k" is generally noting the permeability or intrinsic permeability. Consider using a different notation for the relative permeability or adding a subscript, as it is more related to occupancy of the fluid within the porous structure and less to the actual permeability, which is included in the hydraulic conductivity. I believe that in any case, it should be written as k_{rl} .

We realized there was a typo in the main text. We already used “ k_r ” for the relative permeability in the equation, which is consistent with the suggestions provided by the reviewer. Thank you for pointing this out. We corrected the variable notation in the main text (line 150 of the newly revised manuscript).

Also, is the code implementing the model available in a repository or on GitHub? In the “Code and Data availability” section, the links lead to the input files, but I couldn’t access the trial version of HydroGeoSphere even after the extensive verification stage.

All the equations that were considered and combined are given and discussed in the manuscript. Hence, the transport formulations can be readily implemented in any modeling software. As HGS is a proprietary numerical modelling software that belongs to the company Aquanty, Inc., we are not able to provide the specific code implementation in HGS. However, for reproduction of our verification or illustrative examples as well as other test models, a free trial version of HGS can be obtained from aquanty.com directly. The model input files for these examples as well as a statement on the availability of the test license is what is provided in the “Code and Data availability” section.

Equations 10-14: This section as a whole, and the equations part specifically, are not presented in a way that allows the reader to understand the model. Each specific equation represents a different aspect: equations 10 and 11 refer to the high and low concentration transport, respectively, and equations 12 and 13 refer to the density. Although the sentence preceding this one states this aspect, it requires the reader to understand it without specific guidance. I recommend dividing the equations according to their representation (separating them into 10 and 11, 12 and 13, and 14 alone) and providing a detailed explanation for each part, marking the role of each aspect in the equation, rather than just referring to each parameter within it. As an example, take the important aspect of the “microbial inactivation in the liquid and solid phases” marked by λ , the first-order sink term. This is the extension of the authors’ from a colloidal model to a bacterial model; hence, it is a seminal aspect in the study. Why not explain why the structure of $\lambda s|\rho b|s|$ marks the decay form in the solid? When the model is complex, an effort should be made to guide the reader on the role of each aspect in the model.

Thank you for the comment. We agree and split and rearranged the equations and the definition of the variables in the main text to make it more reader-friendly (lines 191-202 in the newly revised manuscript).

Equation 17 is a tensorial ADE with an exchange and sink term, and since this is a known form, it’s easier to follow. However, I believe that equation 18, which depicts the exchange between the surface to the subsurface, heavily relies on the previous section. Yet this is not clearly explained.

To improve clarity, we added a short note mentioning that we refer to the high- and low-permeability regions that were introduced in the previous section (line 222-223 in the newly revised manuscript).

Section 3.1. A paragraph should be added explaining how well the synthetic experiment matches natural conditions and which natural conditions it replicates.

The aim of the verification example was to show that the implemented feature is correctly coded. For this purpose, we designed a model which is comparable with an existing analytical

solution. This analytical solution was developed based on a colloidal laboratory column transport experiment by Bradford et al. (2009) as outlined on lines 252-253 of the previously revised manuscript. The scenario can be considered as a typical situation in a riverbed, which is fully saturated, but in the absence of complex biochemical reactions that typically happened due to the strong biogeochemical activity of the hyporheic zone. Since the analytical equation itself was developed based on a laboratory column experiment, we refrained from over interpreting this aspect by providing such explanations and believe it is better to instead discuss the transferability to real world scenarios in the illustrative example sections.

Line 279. Correct the term: “low+high-permeability”

This term refers to "low-permeability + high-permeability" and is thus a correct abbreviation.

Line 282. Can you provide the integral evaluation for the analytical solution in an appendix?

As noted in lines 290–291 of the previously revised manuscript, we employed the original published Fortran executable by Leij and Bradford (2013), which includes a detailed description of the underlying mathematics and numerical integration. Since we used this existing tool without modifying its internal numerical procedures, for further technical details, readers are referred to the original publication.

Figure 2. Looking at the BTC and colloidal adsorption to the column, the results are strikingly similar to the solute sorption-desorption process in soil. Can the authors comment on what the observable difference is stemming from the colloidal nature of this simulation, specifically referring to the various scenarios in Table 1, and even more specifically, to scenario 2, where the decay rate is introduced?

Furthermore, what is the reasoning for providing a decay rate of ~2 hours for a pulse of 2 hours? Wouldn't it make a competing aspect where, prior to attachment, there will be a decay in considerable values? And isn't that the explanation for the rapid decay with the depth, namely, that the exchange can only occur at the upper layers, prior to the decay occurring over time?

Can the authors comment on the model's sensitivity to various parameters? Which is more dominant: the exchange rate, conductivity difference for the dual permeability, or perhaps the reactive components of the attachment\detachment?

As mentioned before, the purpose of the verification example was exclusively to show that the new numerical feature is correctly coded and able to reproduce an analytical solution. The parameter variations shown in Fig. 2 were selected to provide a clear and intuitive visual indication of the correct implementation across a broad range of parameter values. We believe that a detailed analysis of individual parameter combinations would not substantially contribute to the core focus of the paper, which is the simulation of microbial transport at the wellfield scale. For this aspect, we subsequently provide two very illustrative scenarios which we also discuss at length. To avoid repeating previous work, we refer readers to Bradford et al. (2009) and other studies comparing colloid and solute transport for a more thorough discussion of parameter sensitivity in the dual-permeability framework.

Line 399: What were the steady-state criteria?

The entire model set-up and parametrization explained for the illustrative riverbank filtration site model holds for the model spin-up and the simulated 20d flood event with a simultaneous increase of microbes and ⁴He in the river water. To improve clarity for the reader, we have

slightly revised the structure of the model setup section. The initial conditions and model spin-up are now described first, followed by the explanation of the flood event (see revised sentence in lines 544–545 of the newly revised manuscript).

Figure 4. Is it possible to present the figures with a logarithmic color map? This will enable a better separation for concentration variation, allowing for the estimation of the model's sensitivity.

Yes, it is possible to visualize the initial concentrations with a logarithmic color map. However, the logarithmic visualization does not increase the separation for concentration variations. Therefore, to facilitate a direct comparison of the values between the main text and the figure, we have chosen to retain the non-logarithmic color scale.

Line 439: In a way, this sentence: “The simulated concentrations show faster transport of microbes due to size exclusion compared to the slower bulk transport of solutes like ^4He ,” could have easily been written for colloidal transport, and if we focus only on the velocity aspect, then it could be written for sorption scenarios. I believe that an effort should be made to explain how the behavior of bacteria in the model's results differs from that of colloids or solute sorption. The implementation of the bacterial aspect in the model is presented, as is the specific implementation; however, the variation in these aspects on the results from colloid transport with sorption is not discussed. The only aspect I notice is the decay rate, but it is not sufficiently stated in the context of the results.

We agree with the reviewer that microbial transport behavior closely resembles that of colloids, as microbes can be considered reactive bio-colloids. However, in our view, the sentence following the cited reference highlights an important distinction, namely, microbial inactivation processes (lines 567–568 in the previously revised manuscript). While both colloids and solutes can be subject to attachment and detachment processes, the key difference lies in their size. Due to their larger size, colloids (and thus microbes) can become physically excluded from smaller pore spaces, a process known as size exclusion. This mechanism is explained in the introduction (lines 44–45 of the previously revised manuscript) and is also explicitly mentioned in the illustrative example as a distinguishing feature of microbial transport when compared to solute transport (see line 565–567 of the previously revised manuscript). To improve clarity for the reader, we have now added a note on size exclusion when presenting the results before (lines 569–571 of the newly revised manuscript).

Figure 5. Add the “Days” as a label to the x-axis

We assume that the reviewer refers to our initial, unrevised manuscript in which the label of the x-axis of Fig. 5 was indeed missing. In the first revision round, we updated the Figure (Fig. 7 in the revised manuscript) by adding the missing label.

2. Reply to comments of anonymous reviewer #3

This manuscript is the revision of a manuscript that I have reviewed before on implementing a dual-permeability approach to address transport of (microbial) colloids in the proprietary software HydroGeoSphere that can simulate saturated-unsaturated flow and transport coupled to surface flow and transport. Upon the revision, the authors have added a new demonstration case that extends the Abdul (1985) benchmark of HydroGeoSphere, which includes coupled surface runoff and subsurface flow and transport, to include transport of microbial colloids. Such a testcase was requested by both reviewers of the original manuscript.

We thank the reviewer for the time and effort to review our manuscript a second time.

However, I am still not convinced. In my previous review I have explained that the dual-permeability model was originally developed for completely different questions, namely to address flow and transport in highly heterogeneous media (including karst systems) by an effective model that does not resolve the heterogeneity. The dual-permeability specific coefficients (second permeability, division of the pore space, exchange coefficient) are typically calibrated by fitting anomalous behavior in both flow (e.g., different head responses in nearby piezometers connected to karst features and not) and transport (e.g., double peaks, extended tailing). None of that applies to the examples of the authors, and it was obviously also not the intension of Bradford et al. (2009), who reinterpreted the approach to address size-exclusion of colloids by introduction of a second permeability. I find it odd that the manuscript contains zero critical discussion of the underlying conceptual assumptions, even after my review (, which I don't want to reiterate). The literature is full of dual-permeability papers that are entirely differently motivated, and the authors simply ignore.

We agree with the reviewer about the initial motivation behind the development of the dual-permeability concept for systems with differences in permeability (e.g. fractured rock with a permeable matrix, near-surface soils containing macropores). The reviewer's reservations are perhaps linked to the use of the term dual-permeability to describe the system we are modelling. Compared to the "classical" dual-permeability systems, it is indeed different because it is really a two-region system that specifically applies to colloids. We retained the term dual-permeability because it was initially proposed by Bradford et al. (2009). Here is the beginning of their abstract:

Recent experimental and theoretical work has demonstrated that pore space geometry and hydrodynamics can play an important role in colloid retention under unfavorable attachment conditions. Conceptual models that only consider the average pore water velocity and a single attachment rate coefficient are therefore not always adequate to describe colloid retention processes, which frequently produce nonexponential profiles of retained colloids with distance. In this work, we highlight a dual-permeability model formulation that can be used to account for enhanced colloid retention in low-velocity regions of the pore space.

They mention using a dual-permeability formulation to account for a high-velocity and a low-velocity region for colloid attachment because using the average pore velocity (with an equivalent porous medium formulation) was found to not always be adequate. They should have perhaps labelled their formulation as two-regions, or two-zone (which we mention in line 78 of the original manuscript) and specify that it is mathematically analogous to a dual-permeability formulation, but not physically similar to systems that are modeled with dual-

permeability. We clarified this in lines 88-90 of the newly revised manuscript, where we now state:

“While the mathematical formulation remains similar to its traditional use, the reinterpretation addresses a different physical system in which colloid attachment is influenced by small-scale velocity variations within the porous medium.”

I have expressed my skepticism that the dual-permeability approach is really needed in my previous review. Unfortunately, the demonstration cases of the authors don't show that the results obtained could not be obtained by a traditional particle-transport code with single permeability, attachment, detachment, and straining. The authors did not make any effort to make this comparison. They only show that the computationally very demanding and difficult to calibrate dual-permeability approach has successfully been implemented. But it is only an improvement when it leads to a qualitative difference to existing approaches. (A hardly detectable peak in a breakthrough curve does not convince me that the effort is worthwhile.) Thus, I stick to my recommendation to go without the scientifically boring 1-D verification against an analytical solution in the main article (it can be moved to supplementary material and briefly mentioned in the main text) and perform a real effort to reproduce the same results (e.g., of the Abdul test case) with a single-permeability model and the usual parameterizations for particle transport. Please demonstrate that there is a real difference!

The motivation for the work was to develop a model that represents as many physical processes as possible for future applications, such as the datasets of microbes and solute tracers we are in the process of producing and publishing. We would not have developed the model if we had initially thought that a particle tracking model based a single permeability would cover all cases. Our motivation builds on several existing studies and reviews on the matter, as referenced already starting with our original manuscript (e.g., Tufenkji, 2007; Bradford et al., 2014; Molnar et al., 2015). We believe that comparing simplified models such as suggested is outside the scope of the paper but it is certainly something that could be investigated later because we do not advocate to always use the most complex and computationally demanding model available. However, one benefit of developing a more complex model, as we have done here, is that it allows to generate a reference case to compare simplified approaches.

We still consider a verification example for a new code feature an absolute must in the main manuscript of a technical note, and therefore we are not going to remove the comparison between our implementation and the analytical equation. The appreciation of the verification example depends on the personal perspective of the reviewer, as apparent by the contrary comments of the different reviewers.

I also believe that the authors should discuss the issue of calibration in a more systematic way. While it is true that any complex model is difficult to calibrate, the introduction of the second permeability plus fractions and exchange terms in a setting that does not show anomalous flow and solute-transport behavior is an add-on to the already existing complexity. The fact that the authors use exactly the same boundary conditions in both domains might help to some extent. Unlike in karst-applications of the dual-permeability domain, the velocity fields in the two domains are doomed to be identical to a fixed factor and the heads are actually completely identical in the way the authors handle the model, at least in the saturated part of the subsurface domain. The factor of the velocities is the ratio of conductivities, and the effective single conductivity of the dual-permeability model is the fraction-weighted arithmetic average. This might be seen as a chance, as a single-domain flow calibration might

be a good starting point. To get the ratio of the two conductivities and the fractions of the two domains, and the exchange coefficient for transport (that for flow should be insensitive with identical head fields) requires solute-transport information sensitive to that [which I have not seen in the applications of the authors], and the coefficients for attachment, detachment, and straining from particle measurements. The chances are super high that model calibration is very ambiguous, and I honestly don't see how the authors could obtain crucial model parameters independently from concentration data.

The reviewer is correct that the calibration of such models requires not only hydraulic information, but also solute concentration data to derive the ADE related model parameters and resolve for possible spatial heterogeneity. We have recently published a review on the matter of calibrating integrated models with different data types, therefore we are highly aware of these aspects and consider ourselves experts in the domain (Schilling et al., 2019, doi: 10.1029/2018RG000619; and Schilling et al., 2022, doi: 10.1029/2022GL098944).

We must point out that the reviewer is not correct that our implementation adds on top of the complexity compared to a normal dual permeability implementation for matrix+fracture transport, because as elaborated at length in our manuscript and as pointed out correctly by the reviewer before, here the dual permeability approach is not used to implement dual-permeability transport that affects solutes, but exclusively to address microbial transport behavior. Thus, anomalous flow behavior that would be observable in solutes is neither expected in cases where this approach is applied for this purpose exclusively, nor is anomalous flow behavior required to calibrate the model. However, alongside solute concentration observations, models based on our implementation of course also require observation of microbial concentrations, as only these alongside solute concentration observations will allow to resolve the parametrization of the dual permeability approach for microbial transport. This we stated clearly in the discussion of both the original manuscript (lines 460-462), as well as in the previously revised manuscript (lines 587-589).

The reviewer is correct that the chances are high that observations of just one or two solutes and "one" microbial species could lead to very ambiguous results in calibration. This aspect is a general, well studied problem of inverse modelling in integrated surface-subsurface contexts (as pointed out above, see for example Schilling et al., 2019, doi: 10.1029/2018RG000619), and any analysis of it is outside of the scope of this manuscript. However, to address the reviewer's point, we now describe more clearly that we need solutes and microbial tracer data - ideally of multiple different microbes - where due to the different sizes of the solutes and the different microbes, the inverse problem is sensitive to the fraction of the two regions and the respective conductivities (see lines 608-10 of the newly revised manuscript). As already mentioned in the discussion, with online noble gas and flow cytometry analysis, we are now able to obtain temporally and spatially highly resolved concentration observation time series of multiple solutes and multiple groups of microbes (see lines 610-615 of the newly revised manuscript).

Minor stuff

1. Line 83: At the end of the traditional approaches, you should at least mention what is not possible in these approaches.

We added a sentence in the revised manuscript (lines 83-84 of the newly revised manuscript).

2. Somewhere where you discuss the Bradford (2009) model: Explain where the dual-permeability model really comes from, and how Bradford reinterpreted it.

We included a brief explanation in the introduction (lines 85-88 in the newly revised manuscript).

3. Lines 86-87: "... to occur in the small-pore, low velocity regions ... occurs in the large-pore, high-velocity region". It's not the region that is small.

The reviewer is correct. We changed it to "small-pore" to make it clear that it is not the region but the pore space that we refer to.

4. Line 119: Helium is not a radioactive compound. It is the product of alpha-decay, but you simply treat it as conservative compound.

We thank the reviewer for pointing out this typo. ⁴He is not radioactive but radiogenic (corrected in lines 124 of the newly revised manuscript). Due to its low release rate, it can be treated as a conservative tracer at the considered time scale (days to weeks to a few months) (already mentioned on lines 211-212 in the original manuscript; lines 244-245 of the newly revised manuscript).

5. Equation 12, and lines 185-186: What is the reasoning behind assuming a mass exchange between the solid parts of the high- and low-permeability domains? Is don't see any physical mechanism that could do that.

The mass exchange between the solid phases considers rolling or sliding of microbes on solid surfaces due to hydrodynamic forces (e.g. Bradford et al., 2009; Molnar et al., 2015). We have added a note explaining the physical mechanism in lines 201-202 in the newly revised manuscript.

6. Equation 17: the divergence of flux should be written with a nabla operator followed by a scalar-product dot ($\cdot$ in LaTeX)
We adjusted this in Equation 17.

7. Line 205: The upside-down triangle is the nabla operator, which may represent the gradient (typically of a scalar field) OR the divergence of a vector field. It is not always the gradient.

But in our case, it is the gradient, which is why it is used here in this way.

8. Section 3: see above. Move that to supplementary material.

As it is standard practice to validate new code against an existing model or analytical solution, we consider this an important component of our paper. We also recognize that perspectives may vary, as another reviewer specifically requested a more detailed discussion of the verification example. Hence, we leave this in the main part of the manuscript.

9. Line 261: You don't need a transverse dispersivity in a 1-D problem.

This is correct, we corrected this in the revised manuscript (line 278 and Table 1 of the newly revised manuscript).

10. Line 276: If the first-order decay coefficients are supposed to include straining they should not be identical in both domains.

The purpose of the verification example was to provide a straightforward visual impression that the new feature is correctly coded across several parametrizations rather than mimic exactly microbial transport in a soil column. Moreover, the parametrization with identical first-order decay coefficients was only employed to one out of multiple implementations.

11. Line 288: That the analytical solution contains no colloid exchange between the two solids is not a restriction, as this exchange makes physically no sense to begin with.

The reviewer is not correct. As mentioned above, there are physical processes explaining the colloid exchange between two solid surfaces. We invite the reviewer to interrogate the referenced studies.

12. Line 385 and very often thereafter: Like in the title, I would remove the word “reactive” when you talk about transport of microbes. There are no chemical reactions involved.

We agree, the term reactive might misleadingly associate microbes undergoing chemical reactions. However, compared to a conservative solute, microbes are considered to be reactive by inactivation and attachment/detachment. For clarification, we replaced reactive by the actual processes, namely inactivation and attachment (line 397 of the newly revised manuscript).

13. Lines 404 and 407: Just talk about “microbes”, you don’t really consider their species.

As explained in lines 376 of the previously revised manuscript, in this illustrative example we mimic a manure application and the transport of a faecal microbial species. The code is able to handle not just generic microbes, but can be tailored to handle specific microbial species. Therefore, we would like to keep the term microbial species.

14. Lines 408-409: You can be more specific here. I assume that the spots in the stream bed where the microbes pop up are locations with infiltrating conditions. These locations have largely to do with the discretized bathymetry of the channel.

We agree and added a more detailed description and explanation (lines 421-422 of the newly revised manuscript).

15. Lines 422-433, discussion of figure 4: I am pretty sure that that the ratio between high- and low-K contributions remains constant because the boundary conditions of the two domains are the same: $Q_{high}/Q_{low} = w_h * K_{high} / (w_l * K_l)$, which here is simply 10%

This is not entirely correct. With the beginning of stream discharge the contribution from the low-permeability region is 0%, as the fast-flow component arrives earlier. However, within half a day, a constant contribution of 10% is reached.

16. Lines 437 and 444: You faithfully talk about size exclusion, but that is not how the model is really formulated. You have simply an irreversible attachment in the low-K region, which you may use to parameterize the effects that are in reality caused by size exclusion.

The reviewer is correct, we incorporated the effect of size exclusion by an irreversible attachment in the low-permeability region. The conceptualization is not new but taken from an array of previous studies, as outlined previously. We explain this clearly on lines 192-197

of the previously revised manuscript. For this reason, we consider it appropriate to talk here of size exclusion.

17. Line 450: “As second illustrative example for coupled microbial and solute transport”

Thank you for the comment, we corrected this in the revised manuscript (line 463 of the newly revised manuscript).

18. Lines 514-515: Why do you refer to bacteriophages? You don’t simulate viruses attacking bacteria.

The reviewer is correct. However, we aimed to base the parametrization of the microbial transport on literature values derived by experiments on the well-field scale (lines 515-516 of the previously revised manuscript). Due to a limitation of experimentally derived rates for prokaryotes with two-site kinetic deposition sites, we considered rates of bacteriophages if no value for prokaryotes was available.

19. Lines 522-523: If the decay coefficient is supposed to represent straining, it should be different in the two domains. You did this better in the Abdul testcase.

As outlined in the Method section, there are two ways to consider straining, either by assuming irreversible attachment in the low-permeability pore space (as done for the Abdul testcase) or by utilizing the first-order sink terms (as done for the Riverbank filtration example).

20. Lines 554-555: Your model formulation does NOT restrict microbial transport to the high-permeability pore space. There is also microbial transport in the low-K region.

The reviewer is correct and this is of course intended; however, the high-permeability region is indeed the dominant flow region for the microbes. We clarified this better in the newly revised manuscript (lines 569-571 of the newly revised manuscript).

21. Lines 564-565: See above. There is no demonstration that the dual-permeability formulation is really needed (and a single-permeability model could not get the job done.)

As previously discussed, studies comparing single- and dual-permeability models for microbial transport already exists and have been reviewed multiple times. Another comparison is not the aim of our manuscript and we instead refer to the extensive literature on the matter.

22. Discussion: See my major remarks above

See our comments above.

23. Lines 589-590: Sorry, but the dual-permeability issues come on top of all the other issues in calibrating surface-water-groundwater model. So this is not an excuse.

This is exactly what we stated in lines 589-590 of the previously revised manuscript: “However, this [required multi-tracer data for robust calibration] is not exclusive to the presented reactive transport model implementation, but is necessary for any surface water-groundwater or larger scale groundwater model (Schilling et al., 2019).”

24. Line 621: The transport is preferential in the high-K region also for solutes. You need the irreversible attachment in the low-K region to make this formulation represent straining.

The reviewer is not correct. As explained in the Method section (lines 192-197 of the previously revised manuscript), straining can be accounted for using either irreversible attachment in the low-permeability region or alternatively an elimination term for the solid phase concentration in the low-permeability region.

25. Lines 624-626: You did not show with your applications that the dual-permeability approach is really needed.

See our comments on the major remarks and 21.