Response to Comments from Referee #3

- The authors of this study used a complex multiscale model to compare the travel time and spatial distribution of catchment scale groundwater flow with and without the influence of hyporheic flow. The results are loosely linked to the fate of nuclear particles that might potentially leak out from nuclear waste disposal in the bedrock of the catchment. The main finding of the study is the fragmentation of the groundwater discharge areas, the reduction of the effective volume that carries groundwater flow and the consequential reduction of the groundwater travel time in the upper 5m of the domain. A monte carlo approach was used to some extend to generate a distribution of possible outcomes where exact model parameters were unknown.
  The underlying model is without a question quite sophisticated. However, I wonder if the right model was chosen to answer the specific scientific question. The main finding, that the discharge zone is fragmented, is relatively obvious and has been published various times in model studies from the hyporheic perspective (e.g. Boano et al 2008, Trauth et al 2014, Fox et al 2016) and also investigated experimentally (e.g. Bhaskar 2012). Given that the general effect is well known, I would have expected an in-depth analysis of the correlations of at least some of the parameters involved. I wonder if this complex, multiscale, pseudo-realistic model is the right choice to draw systematic conclusions with results that have a direct use for other scientists. The model has too many unknowns to investigate general findings, e.g. correlating discharge area reduction to the ratio of hyporheic head and groundwater head or something similar.

The authors appreciate the referee for taking the time to review and comment on the paper. However, we believe that the referee might have misunderstood the applied methods, which combine a regional flow model and a local, hyporheic flow model. The focus is on the effect of hyporheic flow on the discharge of groundwater, not vice versa. Thus, there are major differences in the method and focus of this study compared to previous research, which are highlighted in this response and in the manuscript. For example, the papers of Trauth et al. (2014), Fox et al. (2016) and Bhaskar (2012) deal with ambient groundwater flow, but this is not spatially variable in a watershed. The paper of Boano et al. (2008) deals with regional groundwater circulation, but does not focus on the variability in groundwater discharge. We have tried to stress these differences more clearly in the introduction of the paper.

In summary, the main differences are as follow: (a) previous studies have investigated the impact of upwelling groundwater on the hyporheic flow field and the discharge of groundwater, without considering the hyporheic flow; (b) the suggested research that addressed hyporheic flow did not investigate the "fragmentation of discharge zones" and how the discharge areas/lengths are affected by multiscale subsurface flow interactions; (c) none of the suggested papers evaluated the hierarchically nested structure of the deep groundwater flow on a regional scale, and therefore, (d) the heterogeneous patterns of groundwater gaining and losing reach are not represented on the landscape-scale in the papers suggested by the referee.

Considering all the aforementioned factors, we believe that our results elucidate a phenomenon that is not addressed in previous research.

The following text will be added to the Introduction of the revised paper in order to highlight the focus of the research:

> *Previous studies have investigated the effects of upwelling groundwater on hyporheic flow fields (e.g., Bhaskar et al., 2003; Trauth et al., 2014; Fox et al., 2016), which fundamentally differs from the focus of this paper. This paper investigates the impact of hyporheic flow on upwelling groundwater. In addition, the hierarchically nested structure of the regional scale groundwater was not recognized in previous research on hyporheic exchange flow (e.g., Boano et al., 2008). The aim of the present study is to investigate the principal effects of hyporheic flow on the discharge of groundwater originating from depths much greater than those of hyporheic flow.*

The model's complexity and the need to support it with field data are indeed relevant issues, especially for this complex problem. The investigated flow phenomena require a modeling framework with a sufficient degree of complexity for both stream flow and regional groundwater conditions, in order to evaluate the subsurface nested flow system. The conceptual methodological framework used here allows us to address the subsurface flow interaction taking place on multiple scales within the streambed sediment in a proper and comprehensive (though not too detailed) format, which we believe could not be done more simply than in the presented research. The model's framework can probably be simplified; however, this study did not thoroughly address the optimal level of complexity for the model, and instead used sensitivity analysis to address certain aspects.

- If the authors didn't aim to draw general results but rather calculate the effect for a specific case (nuclear waste disposal in this specific catchment), I see two problems. First, case studies do not match the scope of the HESS journal. Second, some of the boundary conditions have been selected without verification or are too unclear to be used for a specific use case (especially a safety relevant topic like nuclear waste disposal). Generally, I think that a lot of choices for boundary conditions are justified only by references to past work of the own workgroup. In some cases, references to independent workgroups would strengthen the trustworthiness of the model (see detailed comments below).

The aim of the study is described in the following manner:
"The aim of the present study is to investigate the principle effects of hyporheic flow on the discharge of groundwater originating from depths much larger than hyporheic flow depths. The general approach was, thus, to decouple the subsurface flow processes and analyze the catchment-scale groundwater flow field and its associated discharge zones with and without consideration of hyporheic flows. We hypothesize that hyporheic fluxes substantially influence deep groundwater discharge into streambeds and that hyporheic fluxes, in combination with other factors (such as landscape topography, geology, and climate), controls groundwater flows at the watershed scale. In the present study, a multiscale modeling framework was established to represent the spatial scales of both the geomorphology of a region and its subsurface flow field; this model was then applied to a boreal watershed in Sweden."

Hence, the study aim was to provide a more comprehensive picture of the investigated phenomenon, rather than a case-specific investigation.

We agree with the referee regarding the uncertainty of the applied boundary conditions, but maintain that the top-boundary condition is the most important for the posed problem. Other boundaries are placed far enough away to not significantly influence the flow within the watershed discharge zone. The lower boundary was moved to a sufficient depth to not influence the discharge zones, and the same applies for the location of the vertical delimitation of the flow domain. The top-boundary condition is a combination of topography and recharge-controlled boundary condition, both of which are well-established in geo-hydraulics. Both are supported by decent field data.

- The second major finding is the reduced travel time within the upper 5m of the 500m deep domain. Also here, I miss a systematic investigation of the underlying mechanism. E.g. what is the correlation between the fragmentation and the travel time? What is the correlation with the hyporheic head and groundwater head? Is the conductivity a relevant factor in this correlation? These kind of questions should be answered with specific results rather than vague discussions, because the vague answers to these questions are obvious. Again, it would have been much easier to answer these kind of questions if the model was less complex.

The paper's results on the impact of hyporheic flow fields were quantitatively presented, both in terms of the CDF of the discharge areas (Figure 9) and the travel times of the groundwater flow (Figure 7). Although the correlation between the fragmentation of the discharge zones and the flow travel time was not directly investigated in this paper, Figures 7 and 9 help us to understand this correlation in the presence of a hyporheic flow field. Therefore, we will modify the existing discussion on the impact of hyporheic fluxes to address the comment of the referee in the revised version of the paper, as follows;

> *The correlation between the fragmentation of discharge zones and the flow travel time was not directly investigated in this study, but the reductions in travel time of both intermediate and deep groundwater flow (Figure 7), and in the discharge areas of the groundwater flow in the streambed sediment (Figure 9) in the presence of a hyporheic flow field, suggest an inverse correlation between fragmentation and the travel time of the flow. This means that the greater the fragmentation, the lower the groundwater flow travel time. This finding could be expected due to flow continuity, whereby the area and the time are directly correlated for a constant flow tube and for approximately the same flow trajectory. Thus, as the groundwater flow converges in smaller areas due to the streambed-induced fluxes, the flow velocities increase for both the intermediate and the deep groundwater. The travel time of the flow has an inverse relation with the flow velocity, meaning that higher velocities reduce the flow travel times of aquatic sediments (Figure 7).*

The following discussion will be added to the revised paper to address the correlation between hyporheic and regional groundwater hydraulic heads.

> *The stream bedform variations that governed the hyporheic flows were randomly selected from the entire catchment, then rescaled and used in a Monte Carlo simulation. Therefore, the hyporheic topographical factors are assumed to be independent of their location in the catchment, and thus independent of the regional groundwater flow.*

Further, the following discussion will be added to the revised version of the paper to address the effect of hydraulic conductivity on the intensity of the hyporheic and deep groundwater flow velocities within the streambed sediment.

> *Hydraulic conductivity is a primary controlling factor for both hyporheic and regional groundwater flow, due to its linear role in the Darcy law; thus, it causes a correlation in terms of the intensity of the two flow types. The regional groundwater flow, however, is controlled by hydraulic conductivity over larger distances, and generally the flow intensity is dominated by the lower values of hydraulic conductivity (i.e., those found in bedrock). This is because the average hydraulic conductivity over a flow path is given by the harmonic mean weighted by the distance of the path.*

- In addition to the missing universal results, I don't understand why the reduction of travel time in the upper 5m is deemed relevant. The effect of the hyporheic zone decreases exponentially with depth (see eq(5) or Elliot & Brooks 1997a), which is why only the very last segment of a particles travel path will be influenced. In Line 353 it says that the effect of the hyporheic head is strongest, where the overall seepage velocity is low. That means that particles that traveled centuries to millennia (Fig 5) to reach the surface will lose a few years on their last few meters (Fig 7). Even when the different retention coefficients in bedrock and deposits were considered (missing in Fig 7), the effect should be minor. I miss a clear explanation what processes are potentially influenced by the change in travel time in this last stage of the streampath.
  Overall, I'm afraid that the study design is not able to answer the research questions to a degree that goes beyond the intuitive and well-published findings.

It is correct that the contribution of transport in the near stream environment to the overall residence time of the water in the entire sub-surface flow path, starting at great depth in the bedrock, is small. However, this paper aims to highlight the effect of hyporheic fluxes on discharge zones, since it is in this environment that deep groundwater interacts most significantly with stream ecosystems, especially in terms of heat and solute fluxes transported via discharging water. The relatively thin Quaternary deposits of the area make this environment even more important. This research can find technical application in safety assessments of the geological disposal of high-level radioactive spent nuclear fuel, which includes scenarios of leakage where radionuclides flow into groundwater. As revealed by this study, the prolonged travel time of radionuclides in aquatic sediments exposes ecosystems and poses a threat to exposed humans. Although the ecological effects of the prolonged travel times exceed the scope of this study, an improved understanding of the flow process and the spatial distribution of ground water discharge zones will be an important aspect of dose assessment models at the geosphere–biosphere interface. Therefore, even though the water has traveled for millions of years before reaching the sediment, many important environmental processes occur in shallow groundwater zones.
Lines 405-424 of the submitted paper discuss this issue.

Detailed Comments

- L102: I don't think it is necessary to show the definition of Darcy's velocity, but if you do, please use the already defined symbol for Darcy's velocity "W_c" instead of q and q_seepage.

W_C is the vertical component of Darcy velocity, while the vector quantity **q** refers to Darcy's flow velocity (**q** = [U,V,W]). In addition, one of the comments of the second referee concerns the definition of Darcy's velocity. Therefore, we will add the following definition of Darcy's velocity into the revised version of the paper in order to address the first and the second referees' comments:

> *The momentum equation for subsurface flow in saturated porous media at a sufficiently low Reynolds number is generally derived by neglecting inertia terms, while maintaining the potential energy and adopting a linear friction-loss relationship. This leads to the well-known Darcy's law: $\boldsymbol{q} = [U,V,W] = -K\boldsymbol{\nabla}H$. Here, K (m/s) is the hydraulic conductivity, $\boldsymbol{\nabla}$ is the nabla operator, H (m) is the total hydraulic head, q (m/s) is the Darcy*

velocity vector, and U (m/s), V (m/s) and W (m/s) are the Darcy's velocities in the x, y, and z directions, respectively, while bold symbols denote vector quantities (Whitaker, 1986).

- L108: The 50-fold difference between retardation factors between bedrock and deposit is a dominating factor for the distribution of particle travel times. It could be justified by a reference to other workgroups who found similar retardation factor ratios between rock and deposit.

Thanks for the comment. Two references (Jakubick, 1979; Neretnieks, 1979) will be added in the revised version of the paper to substantiate the values employed for the retardation factors in different subsurface media in this study.

- L147: Extrapolating a decay coefficient from measurements at 3 and 7cm to a depth of 5m is questionable. The assumption that the minimum conductivity is 10^-6 (m/s) probably determines the decay coefficient much more than the actual measurements. The only reference is, again, only a single study from the own workgroup. The authors should be able to find more measurements of sediment conductivities in the literature to strengthen their assumption.

Thanks for the comment. In the revised version, we support our assumption (i.e., the depth-decaying hydraulic conductivity of the streambed sediment) by referring to field investigation studies conducted by other research groups (Ryan and Boufadel, 2006; Song et al., 2007; Singh et al., 2014), and we will also add the following to the Discussion of the revised paper.

The depth-decaying hydraulic conductivity behavior of streambed sediment has been shown in previous research using field-measured data (Ryan and Boufadel, 2006; Song et al., 2007). These studies generally confirm the presence of depth decay in the hydraulic conductivity, which is more pronounced at shallow depths and then decreases with depth similarly to an exponential function (Singh et al., 2014). Therefore, in this study, we recognized the depth-decaying trend in hydraulic conductivity, and the exponential decay function was fitted to the data measured by Morén et al. (2017). Further, one can consider how grain size distributions and soil porosity vary with depth, and theoretically argue that these trends would affect the hydraulic conductivity that supports the selection of the functional type. However, it is most important here to represent the observed ratio of K-values in streambed sediments between the 3 cm and 7 cm depths, which varies from 10 to 90%. As a consequence, the epistemic (systematic) uncertainty included in the hyporheic flow fields, which is induced by the depth-decaying hydraulic conductivity function and the spatial heterogeneity of streambed sediments, was not recognized in this study.

- L159 – 173: In line 165 you state that the realistic boundary condition would be a recharge-controlled boundary for most of the terrain. However, you choose a head boundary condition instead and use a mesh-coarsening algorithm to fit the recharge. Why didn't you simply use a recharge-controlled boundary? Coarsening the mesh to fit a result is somewhat unorthodox. All discretizing simulation techniques have in common that an infinitely fine mesh resolution results in the exact solution of the underlying differential equations. The boundary condition in this study, however, implies a tradeoff between model inaccuracy and boundary condition inaccuracy, which, in my opinion, should be avoided by choosing mesh-size independent boundary conditions.

Only the average recharge boundary condition is known for the region, whereas the landscape topography reflects the spatial distribution of the head boundary condition. In general, the top-boundary can be classified as either topography- or recharge-controlled, depending on the aquifer properties and climatic conditions of the study catchment (Gleeson et al., 2011). Haitjema and Mitchell-Bruker (2005) introduced a dimensionless water table ratio, WTR, to represent the connection between topography variation and the groundwater table, in which WTR>1 reflects the topography-controlled boundary condition, and WTR<1 represent the recharge-controlled boundary condition. Mojarrad et al. (2019) calculated the WTR for the Krycklan catchment and found the ratio to be higher than 1. Therefore, the landscape topography was set as the top boundary condition in discharge areas in this study. However, applying the topography-controlled boundary condition in recharge areas would result in too high infiltration rate. To resolve the constraints of the infiltration rate, we smoothed the landscape topography by decreasing the resolution of the mesh size (i.e., groundwater table) over the recharge areas (Marklund and Wörman, 2011; Wang et al., 2018).

- L177: A figure of the mesh/domain would be helpful. The domain is rectangular? I originally thought it was a whole (sub-)catchment with its natural boarders (and lateral no-flow boundary conditions).

Yes, the modeling domain is rectangular and hydrostatic pressure is applied along the side walls. This means that groundwater can flow through both the side walls of the domain, and the landscape water thus divides, as it should. The watershed is generally not defined by no-flow boundaries, due to the hierarchically nested character of the groundwater flow. An illustration of the domain will be added to the revised version of the supporting information. The details of the applied boundary conditions (bottom, top, and lateral surfaces) have already been described in lines 174-177. The geometric horizontal limits of the model are added in the revised version of the paper, as below:

> <u>*The bounding rectangle of the Krycklan catchment (i.e., 11.6 × 10.3 km^2) was set as the horizontal limit of the numerical model domain (64°11.8´ N -64°17.6´N, 19°39.5´E-19°54.3´E).*</u>

In addition, the mesh size and the quality of the mesh elements will be included in the revised version of the paper, as follows:

> <u>*A nonuniform mesh was used for the flow calculation of each layer. The mesh sizes varied within the ranges of 0.1-2 m, 2-17 m and 17-403 m for the streambed sediment, Quaternary deposit and bedrock layers, respectively. In addition, the maximum element growth rate and the curvature factor for the streambed sediment are 1.15 and 0.1, while these change to 1.2 and 0.2 for Quaternary deposits and 1.35 and 0.3 for streambed sediment, respectively.*</u>

- L179: Were the particles weighted somehow? In the following particle statistics, what does one particle stand for? A certain fraction of groundwater volume? A certain area/volume of bedrock? Please indicate why your choice is the best choice for the research question.

Thanks for the question. Each particle applied in the particle tracing represents a specific cross-sectional area of the release plane; i.e., the CDF is weighted by area. In this study, the groundwater analysis was conducted in the steady state condition, and thus only the spatial impacts could be evaluated. Therefore, conducting particle tracing for 10,000 random particles uniformly spaced over a flat surface could provide us with details of the distribution of deep groundwater, regardless of their location at 500 m depth. The CDFs could be easily transformed into flow-weighted CDFs, since the velocity for each starting position is known, but we feel that this information is irrelevant to the paper. However, we do agree that the definitions of the CDF type should be clear, and thus, we add the following sentence to the paper:

> <u>*A particle tracing routine was implemented in the catchment-scale model to analyze the distribution of deep groundwater flow paths (Genel et al., 2013). Each particle applied in particle tracing represents a specific cross-sectional area in the release plane.*</u>

- L196: is "c" the same coefficient as "delta" in line 147?

Not exactly. The "c" in Equation 5 is "$1/\delta$" in Equation 4.

- L213: Why did you use local regions for downscaling? Couldn't it also be a 100x100m region from somewhere else in the world? Or do you assume that there is a correlation between the catchment topography and its streambed-topograpy? I don't think that Wörman, et al., 2007 proved a local correlation between topographies. I think it should be clarified for the reader if a local correlation is assumed or if the regional topography is simply used as a sophisticated random field generator and the topography data could also be taken from somewhere else.

We agree that any region in the world could be used to represent the power spectral properties of topography, but rescaling topography in the real-world domain involves more constraints than just the "power" of the topography function. It has been determined that both streambeds and landscapes are fractal, implying a similarity in geometrical scaling across wavelengths differing by orders of magnitude. Specifically, the variance in landscape elevation shows this similarity over a wide range of wavelengths, but not necessarily other shape measures. Therefore, the method of rescaling streambeds applied in this paper is consistent with the known fractal distributions from the streambed scale to the continental landscape scale (Hino, 1968, Nikora, 1997; Turcotte, 1997; Wörman et al., 2007); however, it also assumes a similarity in the actual (real) shape, which we assume is only a regional behavior trait. This procedure is not supported by the scientific literature, but is used primarily to represent the hyporheic flux and for graphical demonstrations. It has been shown at the streambed interface that the spatially average flux is directly described by the power spectral density of the topography (Morén, et al. 2017, Eq. (7a)), meaning the rescaling of landscapes is not important for the average flux. The following discussion will be added to the revised paper to address the correlation between hyporheic and regional groundwater hydraulic heads.

> *The stream bedform variations governing hyporheic flows were represented by randomly selecting 100 × 100 m^2 areas from the entire catchment, which were then rescaled and used in Monte Carlo simulations. The rescaling of streambeds applied in this paper is consistent with the known fractal distributions between streambed and continental landscape scales (Hino, 1968; Nikora; 1997; Turcotte, 1997; Wörman et al., 2007); however, it also assumes a similarity in the actual (real) shape, which we assume is only a regional behavior trait. Even though this procedure is not supported by previous studies, it has been shown that the spatially average flux follows the power spectral density of the topography (Morén, et al. 2017, Eq. (7a)), meaning it is not important for the average flux, but it is used to demonstrate the nature of the hyporheic flow fields.*

- L179/246: Both particle tracings should be described in a single chapter.

Both particle tracing will be presented in section 2.6 of the revised paper.

- L248: If intermediate particle traces are those that did not enter the bedrock and deep particles are those that started at 500m depth, you miss the flow that enters the bedrock but not to a depth of 500m. Superimposing two particle tracings with different seeds and without weighting them properly against each other adds some randomness to the results.

Thanks for the insightful comment, which is a correct observation. This section should have been better described to avoid any confusion. The 5×5×5 m^3 cubes extracted from the catchment-scale model contained groundwater flow from various spatial scales (including flow that entered the bedrock but did not reach the 500 m depth). However, among all the flows (from different spatial scales), we took the intermediate and deep groundwater (i.e., 500 m depth) flow trajectories for further analyses (i.e., assessing the impact of hyporheic flow fields on deep and intermediate groundwater flows). For this purpose, backward particle tracing was conducted to evaluate the intermediate groundwater flow (i.e., flow particle that did not enter the bedrock). The following new section is added to the paper to describe different flow types:

> *Particle tracking was used to identify streamlines that follow hyporheic, intermediate and deep groundwater flows. Deep groundwater was defined as flow that entered the bedrock, and intermediate groundwater was determined to only be present in Quaternary deposits. Hyporheic flow contains streamlines starting and ending in the stream bottom within a 5 m spatial scale.*

In addition, L248 will be revised, as follows:

> *Cubes of 5×5×5 m^3 at the deep groundwater discharge zones contained groundwater flow from different spatial scales (i.e., from shallow to deep groundwater flows). In addition to the already-evaluated deep groundwater flow, "intermediate groundwater flow" was identified using particle trajectories as starting in recharge areas but confined to Quaternary deposits and sediment layers (i.e., without entering the bedrock domain).*

- L260/Fig2/L179: The release plane for particles was described as "approximately 500 m below the minimum topography elevation". That describes a flat plane. However, the release surface in Fig.2 two shows a curvy plane. Which one is correct and why is the depth "approximately"?

Thanks for the comment. The minimum topography elevation was +117 m a.s.l., and we released the particles from a flat surface located at -380 m a.s.l., which is approximate 500 m from the lowest point of topographical elevation (the exact value is 497 m from this point; we rounded the number for no specific reason). The particles were released from a flat plane (the straight line in Figure 2). We will replace the "curvy plane" in Figure 2 with a flat plane.

- Fig 3 and Fig 6 and the corresponding text could also be placed in the methods section.

Both Figures 3 and 6 present results, and are thus most suited to the Results section. Figure 3 shows the results concerning the impact of topography DEM resolution on the groundwater flow field (the method was already described in lines 161-174), and Figure 6 compares the relative contributions of hydrostatic and dynamic hyporheic head components to the total hyporheic hydraulic head using Monte Carlo simulation (sections 2.4 and 2.5 describe the method for this).

- Fig 7 and corresponding text: How many particle traces from the deep fraction entered the 5x5m domain to be superimposed by hyporheic flux? According to Fig4 it could only be a handful. Why are the dashed lines so smooth and the solid lines are not? Do they represent the same amount of particle traces?

In total, 1552 deep groundwater particles reached the topographical surface within the catchment area (refer to L295 of the paper), and are represented as dots in Fig. 4. The travel time of each deep groundwater particle was evaluated in a 5×5×5 m^3 area in the absence and presence of hyporheic fluxes, respectively. Then, the evaluated travel times across the sediment layer of all 1552 of the deep groundwater particles were presented in the form of a CDF (cumulative distribution function) plot. Therefore, Figure 7 presents the travel time of all the deep groundwater particles that reached the topographical surface within the catchment area (i.e., 1552). In addition, the dashed and solid lines in Figure 7 represent the same amount of particles. Both the deep and intermediate groundwater flow travel times through the streambed sediment were reduced due to the hyporheic fluxes, but this impact was not uniform for all the particles. Therefore, the range of travel times decreased (dashed lines), which led to smoother lines compared to when the hyporheic flow field was omitted (solid lines).

- L377-400: To be honest, I did not understand what you did here. What is on the y-axis of the CDF? The whole topic "coherent area" needs a better explanation. Why did you choose coherence as a measure? What environmental process would coherence be important for? My interpretation is that you created a list of coherent (however coherence was defined precisely) upwelling patches and calculated their surface area. Now you found, for example, that 50% of these patches had surface areas > 400m². Is that correct? If so, I would strongly recommend not to use a CDF for presentation but a PDF. Nothing is cumulating here, CDFs are easily interpreted as "number of particle traces that reached the surface" where 100 means all particles exited the domain or something similar.

The interpretation of the reviewer seems to be correct. The definition of coherent area in the context of this paper was presented on L269-270: "In this study, a coherent area was defined as an area in which the entire flow reflected only the catchment-scale upward groundwater flow". However, we have added this explanation of coherent areas:

> <u>*The fragmentation of the upwelling of groundwater is defined as a shift in the distribution of coherent areas of upwelling groundwater at the streambed interface towards smaller areas. A coherent upwelling area is defined as a set of all adjacent areas in the numerical model with upward flow. Such areas represent streambed areas with upwelling deep groundwater, whereas other areas are not subjected to upwelling deep groundwater.*</u>

Figure 9 shows the distribution of coherent upwelling areas via a CDF plot, which is the statistic integral of the PDF, i.e., both curves would essentially represent the same thing, translated through a mathematical transformation. As an example, if the CDF plot shows 70% (i.e., on the y-axis) for a 5 m^2 coherent area (i.e., on the x-axis), this means that 70% of all the coherent areas are less than 5 m^2, which could not be read directly in a PDF plot (however, a PDF plot might have other advantages).

The temperature and chemical composition of groundwater and hyporheic fluxes are different (Kalbus et al., 2009), thus the fragmentation of groundwater coherent areas indicates a fragmentation in aquatic chemistry and temperature, which determines the distribution of aerobic and anaerobic conditions, as well as the biological activities within the streambed sediment (Harvey et al., 2013; Marzadri et al., 2011; Zertnetske et al., 2011). Hence, the degree of fragmentation in the coherent upwelling groundwater plays an important role in the establishment and mitigation of biological communities.

- L409: Why does the scenario assume accumulation? Shouldn't it be steady state in- and outflow at some point?

Yes, this is true. We meant "long-term accumulation and transport" scenario. The sentence will be revised in the paper as follows:

> <u>*One scenario implies that radioactivity accumulates over a long time period in shallow aquatic sediments, which may be incorporated into agricultural products after future land use changes or by groundwater withdrawal used for irrigation purposes.*</u>

- L415: I'm not familiar with dose assessment but if it is based on the idea that groundwater upwelling happens on a large area without fragmentation it is obviously oversimplifying groundwater flow. If so, you should explain the dose model in more detail and propose an improvement to the model.

To our knowledge, there is no previous study that has investigated the impact of hyporheic flux on deep upwelling groundwater in a discharge zone (the factors that affect the sizes of the upwelling areas as well as the travel time of the deep groundwater flow near the bed surface). This neglected factor (induced by the hyporheic flow field) has implications for radionuclide safety assessment near the bed surface. Therefore, we suggested the often-neglected hyporheic impact should be considered in dose assessment models.

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