

## Response to Community Comment 1

We thank Dr. Medici for constructive suggestions. In the following, Dr. Medici's comments are shown in *italic fonts*, and our responses are shown in regular fonts. Line numbers in our responses refer to those in the tracked version of the revised manuscript unless otherwise stated.

*Lines 23-54. Specify the most novel aspect of the research.*

We have added a sentence in the last paragraph of introduction to highlight the most novel aspect (Line 53-55).

*Lines 42-43. "complex flow pathways consisting of fractured rock matrix and conduits". Insert recent literature on fracture and conduit flow in palaeozoic and mesozoic carbonate aquifers.*

- Medici, G., Munn, J.D., Parker, B.L. 2024. Delineating aquitard characteristics within a Silurian dolostone aquifer using high-density hydraulic head and fracture datasets. *Hydrogeology Journal*, 32(6), 1663-1691.

- Jourde, H., & Wang, X. (2023). Advances, challenges and perspective in modelling the functioning of karst systems: a review. *Environmental Earth Sciences*, 82(17), 396.

Of the two suggested references, Jourde and Wang (2023) is more relevant to the topic of the current manuscript. We have added this reference to Line 43.

*Lines 56-100. What about minor faults? Any link with the small-scale karst landforms that you describe?*

In Line 92-93 of the original manuscript, we stated that several shafts, dolines, and limestone pavements (i.e. small-scale karst landforms) were observed on the plateau area south of Burstall Pass. Some of these karst features are located along the synclinal axis or transverse faults. We have added this information (Line 95-96).

*Lines 538-539. There are several other relevant researches on karst in mountain ranges in the Jura and the French/Swiss Alps. It would be good to enlarge the comparison.*

We have added another example of multi-level conduit system from the Jura in Line 581. We have also added other relevant studies on mountain karst systems in the introduction (Line 30-31) in response to a Referee Comment 2.

*Figure 2c. Syncline beds or tectonic foliation on the back? Please, specify if there is a link between the figure and the conceptual model in Figure 1.*

The layering of the rocks by the glacier in the photograph indeed demonstrates syncline beds. We have added this information to the figure caption.

*Figures 3 to 5. Make the figures and the graphs larger.*

These graphs were inserted in the Word template just for reviewing purposes. Font sizes in these figures will be sufficiently large even when the graphics are reduced in the final production. We will keep the figure sizes unchanged at this stage, and will adjust them if requested by the publisher at a later stage.

*Figure 6. You need to report the days on the horizontal axes on the graph of water temperature.*

Dates are indicated in Figures 6a and 6c. We believe the reader can use these dates in Figure 6b as well. The dates are not included in Figure 6b to make the entire figure package compact. We will keep the format of the figure as is.

*Figure 7. You need to report the days on the horizontal axes for all the graphs.*

Please see our response to the comment on Figure 6.

*Figure 9. Make this graph on tritium larger.*

Please see our response to the comments on Figures 3 to 5.

*Figure 10a. Is it clear enough the link between the conceptual models in Figs 10c and 14.*

We have used relatively high radon activity to infer a low air-to-water ratio (Figure 10c) and the dominance of pressurized condition in the conduit network (Line 512-515), which is reflected in our conceptual model (Figure 14).

*Figure 14. Write on top of the cartoons what the two block diagrams represent.*

We have added labels to the figure indicating an approximate time period represented by each diagram.

## Response to Community Comment 2

We thank Dr. Worthington for thoughtful and constructive suggestions. In the following, Dr. Worthington's comments are shown in *italic fonts*, and our responses are shown in regular fonts. Line numbers in our responses refer to those in the tracked version of the revised manuscript unless otherwise stated.

*This paper presents a wide range of data from a karst spring in Canada. I feel that the greatest strength of the paper is on the detailed analysis of the celerity and velocity of percolation recharge, and that the discussion on these should be expanded and better integrated with the discussion on the slow fraction of flow from isotope data. I have two major concerns with the paper:*

*Comment 1) Groundwater flow velocity*

*line 472-473 "the flow velocity through the karst conduit network is expected to be on the order of 1 m s<sup>-1</sup> during May–December when Q varies between 0.3 and 3 m<sup>3</sup> s<sup>-1</sup>."*

*The prediction of 1 m/s conduit velocities comes from calculations at lines 455-475. The calculations use the Darcy-Weisbach equation, which calculates flow in a pipe as a function of pipe size, hydraulic gradient, and friction (roughness) factor. Estimates of hydraulic gradient (lines 464-468) were based on values from Castleguard Cave (0.03: Ford et al., 1983), high flow conditions at Maligne (0.025: Smart, 1988), and Hölloch (0.0004-0.006: Jeannin, 2001). However, a later interpretation of Castleguard Cave (Ford, 2000; Worthington, 1991) suggested that hydraulic gradients during the formation of the cave could have been 0.0005 or less. Furthermore, a compilation of data in 20 karst aquifers showed that hydraulic gradients along major flow paths to springs are typically 0.0001-0.001 at low flow and 0.001-0.01 at high flow (Worthington, 1991). Moreover, field studies have shown that the Darcy-Weisbach friction factor can vary by more than three orders of magnitude in karst conduits (Jeannin, 2001, Table 3), and pipe size and hydraulic gradient also have a wide variation in their values. Consequently, the uncertainty in the 1 m/s conduit velocity is very large.*

We acknowledge the large degree of uncertainty in the estimation of the hydraulic gradient, the friction factor, and the conduit flow velocity. We have re-written Section 5 (Discussion) and revised the estimation of the conduit flow velocity using a much smaller value of the hydraulic gradient based on the literature (Lines 528-529). In the revised section on flow velocity (Section 5.2), we have put less emphasis on the velocity numbers and presented a more qualitative discussion. We have also included a comparison between our estimates of flow velocity and those measured by Jeannine (2001) (Line 542-544).

*Given the lack of information on friction factors and hydraulic gradients at the site, a better approach is to consider measured groundwater velocities from other studies. It is very rare for velocities of 1 m/s to be measured from tracer tests, and velocities are usually between 0.003 and 0.3 m/s. This is true globally (Ford and Williams (2007, p. 125) and also in the Canadian Rocky Mountains, such as in the cited references by Smart (1983, 1988) and Worthington (1991). Furthermore, conduit diameter has been measured by a scuba diver in the conduit feeding Watridge Spring (line 470), and this gives a velocity of 0.028 m/s at moderately low flow (0.3 m<sup>3</sup>/s) and 0.28 m/s at high flow (3 m<sup>3</sup>/s), assuming a circular conduit. These measured velocities give more accurate values for assessing the aquifer than the 1 m/s estimate made at line 473. The similarity of the measured velocities to velocities in other karst aquifers mean that there is no need to suggest retardation or invoke pools to explain the measured travel times.*

We agree that the order of magnitude of conduit flow velocity (1 m/s) presented in the original manuscript is higher than the common range of velocity determined by tracer tests, and we have updated the number using a more conservative estimate of the hydraulic gradient (see our response to the comment above). However, we would like to point out that tracer tests measure the transport velocity, which integrates the effects of all parts of the flow system including pools and stagnant zones interacting with the conduits. In contrast, the hydraulic method (e.g., Darcy-Weisbach equation or Manning's

equation) calculates the fluid flow velocity in pressurized conduits or open channels. The latter is not influenced by pools and stagnant zones and hence, expected to be greater than the former. We have clarified the difference between transport velocity and fluid flow velocity in the beginning of Section 5.2 (Line 504-508).

*Comment 2) Subsurface residence times*

*line 359-361 ". It is impossible to determine the residence time of water, considering the possible variability of tritium contents between Ottawa and the WKS, but in an approximate sense, it seems likely that most spring water samples had a residence time of five to ten years."*

*line 502 "This [del 18O variation] is consistent with the tritium content suggesting a residence time of multiple years (Figures 8d and 9)."*

*These two comments are the only specific references in the paper to residence times for the groundwater discharging at Watridge Spring. However, Figure 7d shows day by day residence times (which can also be referred to as transit times) for snowmelt and glacier melt from May to September 2021, with dye traces (Figures 5 and 7d) confirming the rapid transit times for specific recharge locations.*

*There have been several previous studies that have measured lag times between surface snow or glacier melt and variation in spring flow in carbonate aquifers, including Gremaud et al. (2009), Krainer et al. (2021), Smart and Ford (1986), Vigna and Banzato (2015), and Zeng et al. (2012). However, this study includes an exceptional data set on subsurface residence times for snow and ice melt (Figure 7d) and on the lag time for the pressure pulse for snow and glacier melt to reach a limestone spring (Figure 7c). Furthermore, seasonal variation in major ions, tritium, and del 18O (Figure 8), could help define the fractions of young water with an age of 0.4 to 4 days (Figure 4d) from older water with transit times up to several years.*

*The wide range of residence times in carbonate aquifers has been widely described in the literature and they are sometimes referred to as dual- or as triple-porosity aquifers. Maloszewski and Zuber (1985) and Zuber et al. (2011) presented the theory to interpret the ages of injected and environmental tracers in dual-porosity bedrock aquifers, and useful studies in carbonate rocks in mountain environments that have included transit times and the fraction of flow in each component include Maloszewski et al. (2002) and Lauber and Goldscheider (2014).*

*I think that it would be well worth adding a new section to Section 5 that specifically discusses subsurface residence times and fractions of spring flow involved. This would consider both vadose zone residence times and groundwater residence times and use most of the data sets collected, thus providing a suitable concluding section to the Discussion. If the authors adequately address the above comments, then this paper would be a fine contribution to the literature.*

We thank Dr. Worthington for highlighting the uniqueness of our data set and for pointing out the need to examine further the significance of different time scales indicated by different tracers and develop an improved understanding of the overall behavior of the karst system. We have read the suggested literature and noted relevant ideas and concepts that can be applied to our study. We cited Gremaud et al. (2009), Krainer et al. (2021), and Smart and Ford (1986) in the original manuscript. In the revised manuscript, we have cited Lauber and Goldscheider (2014), Vigna and Banzato (2015), and Zuber et al. (2011) in relevant sections (Line 31, 209, 506).

The original manuscript did not clearly distinguish different processes governing the groundwater residence times at different scales. In the revised manuscript, we have added a new section (5.1) in the discussion, which starts with a clear explanation of different processes contributing to the total residence time of groundwater in the karst system (Line 466-473). We have also added a new section (5.3) containing discussion of the vadose zone residence time (Line 547-553).

## *Other comments*

*Comment 3 - line 359-361 "It is impossible to determine the residence time of water, considering the possible variability of tritium contents between Ottawa and the WKS, but in an approximate sense, it seems likely that most spring water samples had a residence time of five to ten years."*

*This statement needs to be clarified that it is referring to tritium, and so reflects the residence of the slow-moving water through the matrix and narrow fractures. The residence time of snow and glacier melt has been very well determined in this aquifer. The three tracer tests give subsurface residence times of some days (Figure 5a), and the EC signal at the spring during the spring freshet demonstrates a snowmelt residence time of 0.5 to 2 days, rising to 3-4 days later in the summer when glacier melt predominates (Figure 7d). This strength of the study needs to be better emphasized, such as by adding a section to the Discussion that describes residence times.*

We agree that different residence times associated with different processes need to be described more explicitly and discussed more effectively. We have clarified different components of residence time in Section 5.1 (Line 466-473). Chemical and isotopic signature clearly indicate that the fast-moving snow and glacier melt water is dominated by the old water stored in the system. This is now clarified in Line 477-481 and 554-557.

*Comment 4 - line 465 ", a map of the Castleguard Cave (Ford et al., 1983, Figure 3) suggests a gradient of ~0.03"*

*This needs to be updated with the conclusions on gradients in Ford (2000), as noted above, or else omitted.*

We read Ford et al. (2000), but did not find specific information on the gradient (0.0005 or less). Figure 3C in Ford et al. (2000) gives a gradient of ~0.04 (= 300 m / 7600 m) for a tortuosity of 1 and ~0.03 for a tortuosity of 1.5. Nevertheless, we will have used a smaller value of gradient (0.001) in the new calculation of fluid velocity in the revised texts (Line 528-529).

*Comment 5 - line 484-485 "meaning that the diel fluctuations of discharge would have a delay of less than an hour between BP14 and WKS. This is contrary to the observed delay of 12–15 h (Figure 7c)"*

*The radon data suggests that "most of the conduit flow occurs under pipe-flow conditions" (lines 453-454), and active karst conduits are frequently predominantly below the water table, so the lag time of <1 hour along the main conduit seems probable. However, BP11 and BP14 are 593 m and 575 m above Watridge Spring, respectively, suggesting that there is a vadose zone that is likely to be hundreds of metres thick at these locations. Thick vadose zones are typical in karst aquifers in mountain settings, and the more than 400 caves around the world that are >600 m deep provide a graphic illustration of the widespread presence of deep vadose zones in carbonate aquifers (Burger, 2025).*

*Flow peaks in karst springs due to snow or glacier melt in mountain areas usually occur in the evening or night, reflecting a lag that is typically 6-12 hours after the mid-afternoon peak of snow or ice melt (Gremaud et al., 2009; Krainer et al., 2021; Smart and Ford, 1986; Vigna and Banzato, 2015; Zeng et al., 2012). These lags predominantly reflect the much slower hydraulic responses in the vadose zone compared to the saturated zone. Consequently, most of the 12-15 hour lag shown in Figure 7c is likely to be in the vadose zone, such as the several hundred metres from the surface at BP14 down to the water table. Thus, the alternative explanation for the 12-15 h lag offered at lines 491-492 is the most probable explanation.*

We agree that the vertical flow through the thick vadose zone plays an important role in transport processes. The vertical infiltration of snowmelt water through the vadose zone was depicted in Figure 14a in the original manuscript, but it was not effectively described in the discussion. We have expanded the discussion of this process in the new Section 5.3 (Line 547-561).

*Comment 6 – line 525 – “ transport velocity (0.05 – 0.15 m s<sup>-1</sup>) is much slower than the order of velocity (1 m s<sup>-1</sup>) expected for conduit flow or open-channel flow. Therefore, there likely are many pools within the conduit network, causing the physical retardation of solute transport.”*

*See Comment 1.*

We have used a smaller value of the hydraulic gradients in our new velocity calculation (Line 528-529) and have added a new paragraph discussing the difference between transport velocity and actual flow velocity through conduits (Line 504-509).

*Additional references cited above, to consider for inclusion*

*Burger, P., 2025, <https://cave-exploring.com/index.php/long-and-deep-caves-of-the-world/world-deep-caves/>, accessed October 3, 2025.*

*Ford, D., Lauritzen, S-E., Worthington, S., 2000 Speleogenesis of Castleguard Cave, Rocky Mountains, Alberta, Canada. In: Speleogenesis: Evolution of karst aquifers (Eds. Klimchouk, A.B., Ford, D.C., Palmer, A.N., Dreybrodt, W.), National Speleological Society, Huntsville, Alabama, USA*

*Lauber, U., Goldscheider, N., 2014. Use of artificial and natural tracers to assess groundwater transit-time distribution and flow systems in a high-alpine karst system (Wetterstein Mountains, Germany). Hydrogeol. J. 22, 1807-1824.*

*Maloszewski, P., & Zuber, A., 1985. On the theory of tracer experiments in fissured rocks with a porous matrix. Journal of Hydrology, 79, 333-358.*

*Maloszewski, P., Stichler, W., Zuber, A., and Rank, D., 2002. Identifying the flow systems in a karstic-fissured-porous aquifer, the Schneealpe, Austria, by modelling of environmental 18O and 3H isotopes. Journal of Hydrology, 256, 48-59.*

*Vigna, B. and Banzato, C., 2015. The hydrogeology of high-mountain carbonate areas: an example of some Alpine systems in southern Piedmont (Italy). Environmental Earth Sciences, 74(1), pp.267-280.*

*Zeng, C., Gremaud, V., Zeng, H., Liu, Z. and Goldscheider, N., 2012. Temperature-driven meltwater production and hydrochemical variations at a glaciated alpine karst aquifer: implication for the atmospheric CO<sub>2</sub> sink under global warming. Environmental Earth Sciences, 65(8), 2285-2297.*

*Zuber, A., Róžański, K., Kania, J., Purtschert, R., 2011. On some methodological problems in the use of environmental tracers to estimate hydrogeologic parameters and to calibrate flow and transport models. Hydrogeol. J. 19, 53-69.*

We thank Dr. Worthington for the suggestion. We have included Lauber and Goldscheider (2014), Vigna and Banzato (2015), and Zuber et al. (2011) in the revised texts (Line 31, 209, 506).

## Response to Referee Comment 1

We thank Dr. Fryar for thoughtful and constructive suggestions. In the following, referee comments are shown in *italic fonts*, and our responses are shown in regular fonts. Line numbers in our responses refer to those in the tracked version of the revised manuscript unless otherwise stated.

*The authors have done a commendable job of explaining seasonal and lithologic controls on groundwater flow in an alpine karst basin in the Canadian Rocky Mountains. This study stands out through its integration of multiple, complementary data sets, including continuous monitoring of stage and electrical conductivity over multiple years; development of a stage-discharge rating curve for the spring; repeated sampling for hydrochemical and isotopic analyses; tracer testing; time series analyses; and information from other sources (geologic mapping, meteorological data, and satellite imagery). The approach and findings contribute to the literature on the hydrology of alpine karst, which has been understudied outside Europe and which constitutes an important resource for human use and ecological functioning in various parts of the world.*

*In general, I would answer yes to all the questions posed to reviewers. I reviewed the manuscript before reading the comments already posted online. In particular, I affirm Dr. Worthington's comments on clarifying the discussion of groundwater velocity and expanding the discussion of the range of residence times in the system. My suggestions primarily address providing additional information, plus some minor editorial corrections. Note that I did not check citations against the references and vice versa.*

*Main comments:*

*line 36: Re: "the unique alpine settings offer potential advantages that are not available in lowlands"—What are these advantages? Rephrase following lines 549–551.*

This refers to the dominance of snowmelt recharge as the referee points out. We have highlighted this in the revised introduction (Line 36-37).

*lines 75–76: Note that the age of the Palliser Fm. is Devonian.*

We have added this information to the revised texts (Line 79).

*Fig. 1 and lines 90–91: Which direction does the syncline plunge? Are conduit development and flow paths to the spring structurally controlled?*

The axis of syncline plunges towards northwest. We have included this information in the figure caption.

*lines 195–198: The assumption that diurnal timing of maximum snowmelt coincided with maximum air temperature seems reasonable, but I recommend citing one or more references to support this assumption.*

We have included a reference suggested by Dr. Worthington in Community Comment 2 (Vigna and Banzato, 2015) (Line 209), which used the same method for estimating the timing of snowmelt.

*lines 264–265: Re: "a is an empirical coefficient taking a value of  $0.082 \text{ m}^{-1/2} \text{ s}^{-1/2}$  for an intermediate value of SF (= 1.4) within the expected range" — Fig. 5b shows  $a = 0.077$ . Which value is correct?*

The value in Line 264 of the original manuscript should have been 0.077. This has been corrected in the revised texts (Line 281).

*line 325: SO<sub>4</sub> should be > 99% of total anions analyzed by ion chromatography (emphasis added). HCO<sub>3</sub>, which was analyzed by alkalinity titration, is the dominant anion in groundwater here.*

In the original texts, > 99% referred to the total anion including alkalinity and SO<sub>4</sub> (Line 347). No change has been made.

lines 336–337: Re: “the component associated with snowmelt recharge having a higher Ca/Mg ratio and carbonate fraction but lower ion concentrations” — the lower Ca/Mg ratio between snowmelt periods could reflect proportionally more solute contributions from dolomite, which dissolves more slowly than calcite (for example, see Barna et al., *Environ. Eng. Geosci.* 26(3), 2020, p. 281, and references therein).

We thank the referee for this suggestion. We have added the slower dissolution kinetics of dolomite in the new section discussing groundwater residence time (Line 486-487).

lines 342–344: Were saturation indices for dolomite, calcite, and anhydrite calculated?

We calculated saturation indices (SI) for the three minerals for spring water samples for which in-situ pH measurements were available. We have included the SI values (Line 363-366) and the calculation methods (Line 135-136, 147-149) in the revised texts.

lines 345–347 and Fig. A2: How were rain and snow samples collected? Were snow samples fresh? Were steps taken to limit evaporation or sublimation, which can alter the isotopic composition? How does the LWML compare to other LWMLs from the region?

This was an omission. Collection of rain and snow samples should have been included in Section 3.2 (Line 137-140).

lines 416–417: The observation “The component with low carbonate fraction represents groundwater released from over-winter storage, which is presumably in contact with evaporite deposits within the Palliser Fm.” is consistent with Barna et al. (2020, p. 281) and references therein.

We thank the referee for suggestion. We have included the similarity with the Barna et al (2020) study in the revised texts (Line 442-444).

Fig. 14: Do dashed lines on the surface represent local drainage basin divides? Note on legend.

Dashed lines represent ridges but not necessarily basin divides. We have added ‘Ridges’ in the map legends.

Minor edits:

lines 8, 19, 546, 565: By definition, inaccessible terrains can’t be accessed. Use “remote” or “relatively inaccessible” instead.

We have changed ‘inaccessible’ to ‘remote’ in the revised texts (Line 8, 19, 590, 610).

lines 36–37: “accumulation and melt has” should be “accumulation and melt have”

Corrected (Line 37).

line 74: “Front Range...is” should be “Front Ranges...are”

Corrected (Line 77).

line 128: “with a 0.45- $\mu$ m membrane filters” should be “with 0.45- $\mu$ m membrane filters”

Corrected (Line 132).

lines 134–135: “by a cavity ring-down spectroscopy” should be “by cavity ring-down spectroscopy”

Corrected (Line 143).

line 135: “to the Vienna Standard Mean Ocean Water” should be “to Vienna Standard Mean Ocean Water”

Corrected (Line 144).

line 142: “and the V-SMOW” should be “and V-SMOW”

Corrected (Line 153).

*Fig. 3 caption: “Augst” should be “August”*  
Corrected.

*line 206: “the kth the element” should be “the kth element”*  
Corrected (Line 220).

*line 273: “unlikely serve” should be “unlikely to serve”*  
Corrected (Line 291).

*line 305: “hereafter referred to” should be “hereafter is referred to”*  
Corrected (Line 327).

*line 336: “ion concentration” should be “ion concentrations”*  
Corrected (Line 358).

*line 355: “appear” should be “appeared”*  
Corrected (Line 380).

*line 423: “majority discharge” should be “majority of discharge”*  
Corrected (Line 449).

*lines 450–451: “the Darcy’s law” should be “Darcy’s law”*  
Corrected (Line 512).

*lines 455–456: “discharge...and velocity...is estimated” should be “discharge...and velocity...are estimated”*  
Corrected (Line 517).

*line 465: “Jennin” should be “Jeannin”*  
Corrected (Line 526).

*line 470: “than ~3.7 m” should be “than the value of ~3.7 m”*  
Corrected (Line 532).

*line 733: “mapping procedure map” should be “mapping procedure and map”*  
This reference has been deleted in response to Referee Comment 2.

*line 746: “Wavel” should be “Waveland”*  
Corrected (Line 795).

*line 765: “Alberta British Columbia” should be “Alberta and British Columbia”*  
Corrected (Line 816).

*line 775: “Tracer test” should be “Tracer tests”*  
Corrected (Line 830).

*line 855: “Savanna” should be “Savannah”*  
Corrected (Line 926).

## Response to Referee Comment 2

We thank Dr. Chen for thoughtful and constructive suggestions. In the following, referee comments are shown in *italic fonts*, and our responses are shown in regular fonts. Line numbers in our responses refer to those in the tracked version of the revised manuscript unless otherwise stated.

*The authors investigated a remote, high-alpine catchment in the Canadian Rocky Mountains using multiple approaches, despite having very limited information at the beginning of the study. They combined hydrological, geological, hydrochemical and isotopic information to characterize the karst system, which is strongly influenced by snow and glaciers. The main outcome is a hydrogeological conceptual model that reflects the system's main hydrological and hydrogeological processes. Additionally, the authors highlight that the methods applied to this study could be transferred to other high alpine systems with similar characteristics. The manuscript is clearly structured and well written. The results seem solid. Overall, it is great work, demonstrating considerable effort in terms of fieldwork in such a remote area, as well as the combination of many different investigative methods and guidance to reach reasonable and logical interpretations, which is surely a strength of this study. However, critical discussions are missing in some places, which could be improved. I recommend considering this work as a minor revision.*

*Major comments:*

*1) Introduction / Novelties:*

*I suggest that the authors elaborate better on the novelties of this research work in the introduction. In my opinion, there are three main novelties in the current research work: 1) improvement of the process understanding about the impact of snow/ice behavior on recharge, storage, drainage and discharge in high alpine karst aquifers; 2) the combination and application of multiple investigation approaches (hydrological, hydrogeological, hydrochemical and isotopic) to a high alpine karst system to achieve a robust and coherent conceptual site model and 3) knowledge production for the local karst system (Watridge Karst Spring).*

We thank the referee for the suggestion. We have highlighted novel aspects of our work in the introduction of revised texts (Line 53-55).

*2) Representativeness of weather parameters (P and T):*

*The only available weather station is located at 2260 m a.s.l. within the study area. The study area varies between 1870 (WKS) and 3406 (Mount Sir Douglas) m a.s.l. How representative are P and T measured for the entire study area, and how will the analysis and interpretation relate to the hydrological water balance and diel fluctuations of discharge and EC be influenced by these data uncertainties? It is expected that there will be spatial and temporal variation of snow/ice accumulation and melting behavior within the study area. How does this influence the diel fluctuations of discharge and EC? I suggest the authors incorporate/consider this aspect, at least for critical discussion.*

We thank the referee for pointing out the importance of elevation dependence of precipitation and air temperature. Hypsometric analysis of the effective catchment (yellow polygon in Figure 1a) indicates that 70% of the catchment is contained within an elevation range of 2100-2500m, and the median elevation of the effective catchment occurs at 2420m. Therefore, the Burstall Pass weather station (2260m) represents average precipitation and temperature of the catchment reasonably well. We have added new sentences to explain this in the method section (Line 167-170) and the results (Line 299-300).

*3) Artificial tracer experiments:*

*BP14(2) shows a mass recovery of 115%, which is impossible. The authors mentioned a technical issue regarding sensor calibration. How reliable are the measurements taken by the field device installed in the*

*spring generally? Have factors such as UV, turbidity, and temperature, which can disturb the tracer measurements, been considered? To better understand the results of the tracer experiment, the breakthrough curves (BTC) should also be compared with the system input signals (e.g., precipitation, air temperature), as the shape of the BTCs differs significantly. The BTCs do not show significant tailing, but multiple peaks, which indicate the transport via multiple paths within the main karst drainage conduit network. If we assume that the measurements are reliable, how can the significant difference in mass recovery rate between BP14(2) and BP14(1) & BP11 be explained?*

We did not have high confidence in the calibration coefficient of fluorometer (Line 265-266), resulting in a large degree of uncertainty in the quantitative estimate of mass recovery. We originally thought about reporting sensor response in millivolts, instead of concentration, but we decided against the idea. We have deleted the mass recovery values from Table 1 and added a sentence stating that the mass recovery rates could not be determined (Line 271).

We did not monitor turbidity in spring discharge. Slight increase in turbidity was visually noted in late August but we do not believe it affected the sensor performance. Water temperature was stable during the warm season when tracer tests were conducted (Figure 6b). We have added a sentence to explain these (Line 254-255).

Air temperature remained mostly positive during the warm season (Figure 6a); therefore, we do not believe air temperature data are relevant to the interpretation of tracer break through curves. We have added a graph showing six-hour precipitation as Figure 5a.

#### *4) Recharge area delineation*

*The uncertainty of meteorological forcing, as well as its impact on recharge area delineation and water balance calculation, should be critically discussed. In my opinion, this is one of the main sources of uncertainty when estimating the water balance of high Alpine catchments. The authors estimated that there was about 90% recharge rate (if I understood the estimation correctly) for the studied karst aquifer system. The map (Figure 1) does not show any surface drainage features (streams), so it indicates high karstification of the studied system, and precipitation or melt can easily infiltrate. The study for estimation of recharge rates for karst systems in the European Alps can be used here to support (Malard et al. 2016: A novel approach for estimating karst groundwater recharge in mountainous regions, and its application in Switzerland). To validate the recharge delineation and also the associated water balance calculation, hydrological karst modelling should be applied for future work.*

Please see our response to Comment 2 regarding the uncertainty in meteorological forcing. Relatively little surface runoff occurs within the effective catchment as pointed out by the referee (Line 98-100). We thank the referee for pointing us to Malard et al (2016). Characteristics of our study catchment (lack of vegetation, karstified surface, etc.) is similar to the study area of Malard et al. (2016). We have cited this paper in the revised texts (Line 312-314).

#### *5) Conduit flow and transport*

*In general, regarding the terms 'open channel' and 'pipe flow', I personally think that the descriptions of open and pressurized conduit flow are more accurate. Most conduits have a closed section. Depending on the water level, the conduits can be unpressurized or pressurized. The authors discussed how the existence of large pools can cause retardation during solute transport along the conduit network. In my view, there is an inconsistency or at least an open discussion point, regarding the concept of large pools when compared with dye tracer experiments. If such large pools exist in the conduit network, they should significantly impact the shape of breakthrough curves (Hauns et al. 2001: Dispersion, retardation and scale effect in tracer breakthrough curves in karst conduits; Yang et al. 2025: Effects of karst conduit structure on breakthrough curves: Experiments and modeling). Can this effect be observed or identified in the measured breakthrough curves? Additionally, the authors tried to use the concept with large pools to explain the observed increase in transport response time concurrent with the reduction in discharge. However, I think a more general reason is the change in hydraulic gradient in the groundwater level distribution in the karst aquifer during varying flow conditions. The authors used the Darcy-Weisbach*

*equation to estimate potential flow velocity in a pressurized conduit. To my understanding, the transport time (derived from tracer experiments) and the transport response time (derived from time series analysis) should reflect the transport process through the entire aquifer (including the unsaturated and saturated parts) along fast flow paths. This is surely more than pure transport in a saturated drainage conduit. I recommend that the authors explicitly consider and discuss the role of the unsaturated part of the studied karst aquifer (e.g., the epikarst) to transport processes (transport time and transport response time). This would provide a more complete picture.*

Similar points were made by Dr. Worthington (Community Comments 2). We have re-written the discussion section (Section 5) by reorganizing the contents and revising our conceptual model. The following are the key elements of our responses.

- (1) We have revised our interpretation of tracer breakthrough curves. We have stated explicit links between the late arrival of tracer pulses and the presence of multiple flow paths potentially influenced by retardation mechanisms (Line 270) and followed it up in discussion (Line 538-541).
- (2) We agree that the gradual reduction of transport velocity is primarily caused by the reduction of the hydraulic gradient. We have clearly stated that in the new discussion (Line 573-574) and conclusions (Line 597).
- (3) We have revised the estimate of gradient in the Darcy-Weisbach equation (Line 528-529) and made a qualitative comparison of the estimated flow velocity and transport velocity to hint at the possibility of solute retardation by stagnant pools or vadose-zone processes (Line 538-540).
- (4) We have examined the role of vadose zone flow processes and indicated them as the primarily mechanism controlling the hydraulic response of the karst system to snowmelt inputs (Line 554-559).
- (5) We have revised our conceptual model (Figure 14) to reflect the important role of the lateral hydraulic gradient in controlling spring discharge and transport velocity.

#### *Minor comments*

*1) Line 30: please remove Chen et al. 2017 and add the following references (Goldscheider 2005; Lauber & Goldscheider 2014; Lucianetti et al. 2016; Frank et al. 2019):*

*Goldscheider 2005: Fold structure and underground drainage pattern in the alpine karst system Hochifien-Gottesacker, Eclogae geol. Helv.*

*Lauber & Goldscheider 2014: Use of artificial and natural tracers to assess groundwater transit-time distribution and flow systems in a high-alpine karst system (Wetterstein Mountains, Germany), Hydrogeol J*

*Lucianetti et al. 2016: Preliminary conceptual model of an Alpine carbonate aquifer (Pale di San Martino, Dolomites, Italy), Italian Journal of Groundwater*

*Frank et al. 2019: Sulfate variations as a natural tracer for conduit-matrix interaction in a complex karst aquifer, Hydrological Processes*

We have removed Chen et al. (2017) and add the suggested references (Line 30-31).

*2) Figure 1: Where are the glaciers on the map? There is a legend for them, but I cannot find any in the study area.*

This was an omission. We have added glaciers on the map (Figure 1a).

*3) Figure 14: The symbol for flow in open conduits is difficult to see. It would be useful to have a symbol for snow cover in both figures: extensive snow cover in (a) and no snow cover, only glaciers in (b). Please ensure that the saturated part in (b) is fully colored (some saturated conduits are not colored).*

We have enlarged the size of arrow indicating open-conduit flow and have added snowcover. The upper most parts of some conduits within the matrix-saturated zone in Figure 14b were intentionally left unsaturated to demonstrate the function of seepage faces. In the revised Figure 14b, the thickness of the saturated zone at the bottom of the Palliser Formation has been reduced to enhance the effects of the lower hydraulic gradient during the low-flow period (see our response to Comment 5). This has reduced the thickness of the seepage faces.