Detailed response to comments Referee 2 in the interactive discussion

Reviewer 2

This is an interesting research article and it presents a novel dataset of discharge and contaminants measurements (& estimates) spanning two years in basin in the Swiss Alps, with analysis that starts to consider the importance and implications of the data. However, I think this manuscript requires a much stronger narrative throughout, and a slimming down of the information presented to enable a clearer and more concrete message to be shared. I believe that in its current form, the manuscript is trying to cover too much. A focus on the key aspects of the data and analysis and results will really help to narrow this down. I believe that given the limitations and assumptions in this study, potentially even a reconsider to the title to help demonstrate a reframing and focus in the manuscript as I am not entirely sure that the results seen can be solely attributed to the rock glacier identified. I think that some of the figures could be moved to supplementary to help really focus on some of the headline findings. The text throughout also requires some work to help with this reframing, and a number of the figures could be improved.

We would like to thank the anonymous referee for their thorough and insightful feedback. Your valuable comments and suggestions will strongly contribute to improving the quality of our manuscript. Thank you for your time and effort in providing such detailed and constructive critiques.

Comment 1

General comments: One of my main concerns about the manuscript as it stands and the current framing is that I am not convinced that the rock glacier is the only input to the discharge measured and sampled in AP5. This can be seen in Figure 1: AP5 is not just linked to the rock glacier springs identified on the map (AP1, AP2, AP3) but also likely the permafrost features to the east. This then affects the assumptions made about contributions further downstream at AP10.

The referee is correct, at AP5, there might be a small discharge and flux contribution from the permafrost features to the east. However, there are no visible surface tributaries from the eastern side discharging at AP5, which means that such potential contribution would predominately relate to subsurface flow. Based on the observation that the highest solute concentrations are measured at AP1 (Table 2) and because only this spring refers to a perennial rock glacier spring, it is quite evident that the majority of the fluxes measured at AP5 originate from AP1. To verify this, in August 2021 we started to measure solute fluxes also at AP1_2, which is another monitoring location upstream of AP5, approximately 100 meters downstream of AP1, where the springs from AP1 and AP2 merge (Fig. R1), and which is not affected by AP3 and the permafrost features to the east mentioned by the referee. The table below (Table R1) shows the measured fluxes of Ni and Zn at AP5 and AP1_2. The last column of this table shows the relative contribution of fluxes at AP1_2 versus those at AP5 on the same dates. In case of Ni, 75-88 % of the fluxes at AP5 come from AP1_2, whereas the contribution for Zn is between 64 and 77 %. The difference to 100% is explained by subsurface flow not captured at AP1_2 (there is evidence for that based on the flat topography and the presence of talus deposits), minor contributions from other ice-rich permafrost features such as that to the east mentioned by the referee as well as AP3. In any case, the additional data provided in Table R1 confirms that the majority (up to 88%) of the fluxes measured at AP5 originate from the rock glacier shown on Figures 1 and 2. The data from the additional monitoring location AP1_2 was not included in the Discussion Paper for simplicity. However, to clarify that AP1 and AP2 contribute the majority of the fluxes at AP5, the measured data from AP1_2 will be added to the Supplement when preparing an updated version of the manuscript. Moreover, this will be clearly stated in the updated discussion.

We would like to emphasize that potential Ni and Zn sources in addition to the rock glacier shown on Fig. 2 would not change our conceptual models for element enrichment and mobilization (Fig. 10). As detailed in our response to Comment 1 of Referee 1, significant solute mobilization requires the presence of ice-rich permafrost. Accordingly, the fluxes recorded at AP5 reflect the cumulative export from the rock glacier shown on Fig. 2 and other ice-rich permafrost occurrences not as clearly visible. This means that, even if multiple sources were relevant, differences in fluxes would still reflect variations in the export of ice melt upstream of AP5.

The possible presence of unknown additional sources of Ni and Zn was actually the reason why AP5 was originally chosen as the main upstream monitoring location. In addition, AP5 allows tracking flux contributions from AP3 and from subsurface flow in the vicinity of AP1_2. The suitability of AP5 is further confirmed by the observation that at the downstream monitoring location AP10, the fluxes of Ni and Zn are essentially the same as at AP5 (Fig. 7). This confirms that downstream of AP5, there are no additional sources and that Ni and Zn behave conservatively (see our responses to Comments 1 and 12 of Referee 1). The former agrees also well with the absence of acidic pH values in streams merging downstream of AP5. In conclusion, the fluxes of Ni and Zn monitored at the downstream monitoring location AP10, exclusively originate from ice-rich permafrost upstream of AP5, whereby the majority (up to 88%) is exported from the RG shown on Fig. 2.
Table R1: Flux measurements at the two monitoring locations, AP1_2 and AP5, and the relative contribution of fluxes AP1_2 / AP5.

Date	Fluxes at AP1_2 (kg day⁻¹)		Fluxes at AP5 (kg day⁻¹)		Relative contribution AP1_2 / AP5 (%)	
	Ni	Zn	Ni	Zn	Ni	Zn
20-Aug-2021	4.1	9.5	5.0	13.0	82.8	73.0
23-Sep-2021	2.0	4.7	2.7	7.0	75.4	66.3
20-Oct-2021	2.1	4.6	2.3	7.1	88.4	64.1
7-Jul-2022	5.2	12.5	6.7	17.5	78.3	71.5
12-Aug-2022	3.4	8.4	4.1	10.9	82.5	77.2
10-Oct-2022	1.9	4.5	2.4	6.5	76.8	69.6

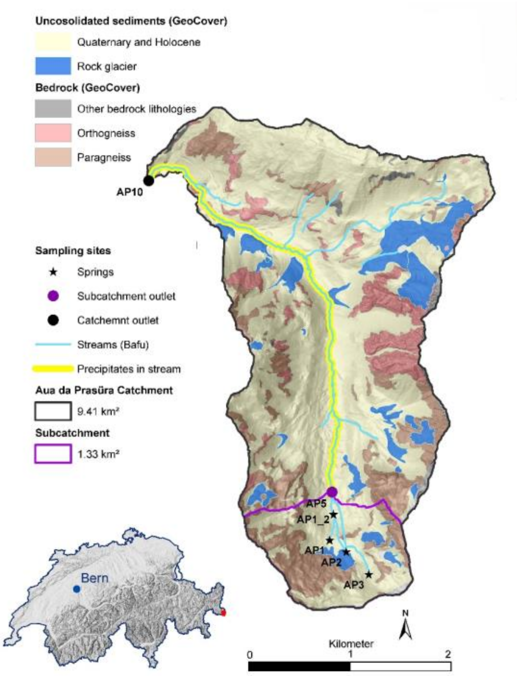

Fig. R1: Geological map modified from the GeoCover (© swisstopo) dataset from the Federal Office of Topography swisstopo (adapted from swisstopo, 2022) based on field observations of the Aua da Prasüra catchment. The star symbols refer to the locations of the three rock glacier springs at the origin of the catchment (AP1, AP2, and AP3), whereas the filled circles refer to the two sampling locations downstream of the rock glacier (AP5, AP10). Compared to the corresponding figure in the discussion paper (Fig. 1), the location of the additional monitoring location AP1_2 is added to the map.

Comment 2
General comments: I have made several specific comments here, but I think that ultimately a reframed and more streamlined revision of this manuscript is required, which will involve some more major changes to the text and figures included, and more minor changes throughout. I hope that these specific comments help with some of that.

We thank the reviewer for the valuable suggestions. As outlined below, we will take them into account when preparing an updated version of the manuscript.

Abstract: Too long for a TC research article (150-250 words). Shortening the abstract will help to convey the key aims and messages of the research. The current level of detail here is not required for an abstract.

The journal guidelines do not impose a word limit for abstracts. However, to more effectively communicate the key aims and messages of the research paper, it will be shortened. We agree that the original version was too long.

Comment 3
Introduction: L88-91: Please note that this process is not exclusive to rock glaciers, but also affects ice glaciers – so it might be worth considering this.

That is correct. Thank you for pointing it out. We will add that acid rock drainage and elevated solute concentrations have been described to occur downstream of ice glaciers as well (e.g. Fortner et al., 2011; Dold et al., 2013).

Comment 4
Site description: If this catchment/sub-catchment is ice glacier free, it is worth stating this clearly to provide clarity to the reader that the catchment being studied only contains permafrost/rock glacier features.

Thank you for highlighting this. The catchment is indeed ice glacier free. We will add this to the lines 114-115, "In addition, the catchment is ice glacier free and both intact and relict RGs are frequently found in the catchment".

Comment 5
Site description: Figure 1: The rock glacier which is central to the study/measurements is not obvious on this figure. The location of the springs are. You do have the UAV image as Figure 2 which is helpful, but consider overlaying the outline of the three rock glaciers on either figure 1 or figure 2 to ensure it is super clear to the reader.

We agree. Therefore, we will provide two overlays of the studied RG when updating Figure 2. The first one will be based on the geological map, and the second one based on our field observations (geomorphological features). The latter is slightly larger than the extent of the rock glacier shown on the geological map (see response to the next comment).

Comment 6
Site description: Furthermore, according to the unconsolidated sediments key, there is no identified rock glacier at AP3, instead this monitoring point is higher than the rock glacier being monitored?

The outlay of the monitored RG in the Figure 1 is based on the geological map. It is correct that the AP3 location is higher than the RG extent shown on the geological map. However, based on our field observations (geomorphological features, discharge temperature of AP3 below 2 °C), the extent of the rock glacier is larger and includes at least part of the catchment sampled at AP3 (the rest of the AP3 catchment is characterized as ice-rich permafrost as indicated by the permafrost and ground ice map of Kenner et al., 2019). This will be clarified when updating the site description.

Comment 7
Site description: Figure 1: I recommend adding "Switzerland" to the inset image in the bottom left.

To enhance the clarity, "Switzerland" will be added to the bottom left of the Figure 2.

Comment 8
Site description: Figure 1 caption is extremely long. I recommend removing the follow sentence from the caption and looking to include it in your manuscript in an appropriate location: "The area of the catchment at the upstream location at AP5 is 1.33 km^2 , corresponding to about 14 % of the catchment sampled at the downstream AP10 location (catchment area = 9.41 km^2) where a pressure and conductivity probe is installed."

We tried to mention the important points in the figure caption. However, the referee is correct, the caption is too long. The mentioned sentence will be moved to the very end of the "Site description" section, line 133 (see also our reply to Comment 39 of Referee 1).

Comment 9
Site description: Figure 1: I personally don't think the detail on where Figure 2 is taken from is essential on this Figure 1 or in the figure 1 caption too, so I also suggest removing this from the figure and removing "The green circle with the two tips illustrates the location and direction of the UAV for taking the photograph of the rock glacier shown in Figure 2". Instead, I recommend just placing a box over that area to state Figure 2.

We agree. We will remove this special sign from both maps and add a box around the monitored RG in Fig. 1a.

Comment 10
Site description: Figure 2: Consider adding the year or date of the UAV photograph.

That is a good suggestion. We will include the date when the UAV photograph was taken, "06.07.2022".

Comment 11
Site description: L130: "Based on historical aerial photographs, precipitation only occurs since the Year 2000" – please be more specific that you are referring to ARD precipitate here.

Thank you for mentioning it. It will be changed to "basaluminte precipitation" to avoid confusion with atmospheric precipitation/rainwater (see also our reply to Comment 36 of Referee 1).

Comment 12
Methods: You have a paragraph on water table, but realistically this information is not used later on in your work and could instead be moved to supplementary information?

The information on water table is presented in a joint section, “3.3”, with details on EC measurements by the installed probe. This information is used for the correlation between water table and discharge in Fig. 3. Nonetheless, based on our response to Comment 17 of the same Referee, we will move all the information regarding water table measurements to the Supplement.

Comment 13
Methods: Discharge measurements – I recommend giving this a different sub-heading to represent the observations more, e.g. Tracer discharge measurements

We agree, and will change the sub-heading to “3.2 Tracer discharge measurements”.

Comment 14
Methods: Snow height & precipitation data not mentioned in the methods section (but used in figure 8) - please look to ensure there is information on this data as it is used.

The Referee is correct. We only mentioned this information in the caption of Figure 8, where they were used. Accordingly, we will add another sub-heading, “3.5 Snow height, precipitation and temperature data“ to the very end of the Method section including information on the two weather stations.

Comment 15
Results: Table 1: I recommend having discharge at AP5 listed in your table before discharge at AP10 (swap the two columns over). I also recommend using “-“ to identify no measurement possible rather than n.m.

These are very good suggestions. We will implement them.

Comment 16
Results: Table 1 caption: The end of the table caption becomes quite descriptive, and states information that would ideally be in the manuscript text rather than the caption: “to illustrate that the discharge from the upstream catchment and hence the rock glacier at the source of the stream is disproportionately high”

Thank you for the good suggestion. This sentence will be deleted from the table caption and be added to the previous paragraph, lines 241-248.

Comment 17
Results: Figure 3 potentially not required for the main focus of the paper? This would help streamline your focus. This could be placed as supplementary instead.

We agree with moving Fig. 3 and the corresponding section “4.1.2” to the Supplement.

Comment 18
Results: L274-276: “The temperature of the three rock glacier springs at the origin of the Aua da Prasüra stream (AP1, AP2, and AP3, Fig. 1) is constantly below 2 °C, confirming that all springs originate from an intact rock glacier containing ice” -> confirming all springs originate from permafrost might be a safer assumption?

We agree. Therefore, we will change it to “is constantly below 2 °C, confirming that all springs originate from ice-rich permafrost as indicated by the permafrost and ground ice map shown on Figure 1b (Kenner et al., 2019)”.

Comment 19
Results: Table 2 is good. Perhaps shading cells with observations greater than the recommended drinking water limit would help the reader visualise the results even more?

We agree with the comment and we will make the changes accordingly.

Comment 20
Results: Figure 7: No need to have Feb or March on the x axis of these plots? Starting with April will help reduce the white space here. Given the inconsistent temporal measurements for this data, I believe that line graphs may be misleading to the reader.

We agree that the X-axis of Figures 7a-7d should start from April. It will be changed while preparing an updated version of the manuscript. It is totally correct that the concentrations between the individual measurements are unknown and that line graphs might be misleading. However, without the lines it would be difficult to see the general seasonal trends (i.e. maximum in early July, decreasing concentrations in late summer). What we will do instead is that we will make dashed lines to reduce the focus on the lines. Additionally, in the caption we will clarify the meaning of the dashed lines and emphasize that the detailed seasonal evolution of the fluxes at AP10 is provided in Figure 8.

Comment 21
Results: Figure 8: I am not sure that the link between precipitation events, discharge and fluxes of elements is really related to the rock glaciers themselves and their drainage? Instead this is more representing the hydrological response of the catchment/sub-catchment and its geology However, it is nice to see the peak snowmelt indicated and peak precipitation events indicated on this figure.

The reviewer is correct, that the correlation between the snowmelt/precipitation events and discharge fully represents the hydrological response of the catchment, which is definitely controlled by the local geology (e.g. occurrence and distribution of unconsolidated rock deposits such as rock glaciers, which are highly important for subsurface water storage and groundwater flow). However, the correlation between these events and the solute fluxes must be caused by a strong coupling of chemical processes occurring within the rock glacier body and hydraulic processes. Chemical process are crucial because all solutes are originally dissolved from the pyrite-bearing paragneiss rock debris of the RG (see our response to Comment 1 of Referee 1). Moreover, enrichment of the mobilized solutes in the RG ice is needed to explain the high solute concentrations currently observed in the RG springs (Table 1). It follows that the recorded fluxes reflect the mobilization of temporarily stored solutes and hence the export of ice melt from the rock glacier. The observation that solute fluxes strongly correlate with snowmelt and precipitation events as well as discharge implies that solute export is strongly controlled by the hydraulics of the RG. However, owing to the importance of the enrichment in ice, only a small little amount of solutes would be exported without the presence of ice within the rock glacier (Comment 1, Referee 1), emphasizing the importance of this landform for causing high solute fluxes.

Comment 22
Discussion: I think that a narrowing your focus for the whole of the manuscript will help to keep the discussion section also more focused. I recommend not expanding your assumptions but removing some of section 5.2, figure 9 and figure 10, and removing section 5.3 (this should be more of a results section anyway rather than discussion, but here I think it overcomplicates the main narrative of the manuscript). I believe that it would be more valuable to keep the focus of the paper on the data analysed and I believe that the limitation in your assumptions of the ice melt and contributions from the rock glacier alone are too much of a stretch. In my opinion, there are too many other contributors to the discharge sampling points to isolate the rock glacier in this way.
Instead, it would be great to really contextualise your findings and suggestions in relation to other published work, in the Alps and elsewhere.
And to ensure that you cover the assumptions and limitations to the research.

Based on the comments of both referees, it is evident that our manuscript was not clear enough and that we need to narrow down the focus. Our plan for revising it is to focus on the results of the monitoring of solutes exported from the ice-rich permafrost area at the origin of the catchment and discussing the reasons for the observed correlations shown on Figure 8. This requires at least the following changes:

1) Removing “insights on ice melt dynamics” from the title
2) More clearly stating in the introduction that the aim of our flux monitoring is to test the hypothesis regarding the interim storage of solutes in rock glacier ice as formulated in our previous paper (Wanner et al., 2023)
3) Removing Figure 9 as suggested by Referee 2 or moving it to the Electronic Appendix (see our Response to Comment 58 of Referee 1)
4) Emphasizing in the discussion that the fluxes measured at AP5 and AP10 predominantly relate to the export from the rock glacier shown on Figures 1 and 2 (see our response to Comment 1 of Referee 2)
5) Updating the conceptual model shown on Figure 10b to include that ice melt from the frozen rock glacier core enriched in toxic elements may reside in the unfrozen base-layer during wintertime (see our Response to Comments 5 and 13 of Referee 1). Accordingly, we disagree with Referee 2 that Figure 10 should be removed from the manuscript. In our opinion, it is highly important to provide an explanation for the correlations observed between snowmelt/precipitation events, discharge and solute fluxes (Fig. 8). Moreover, we are convinced that the conceptual models provided in Figure 10 are available to well explain the following observations presented in the Discussion Paper: i) high solute concentrations in RG springs showing temperature close to the melting point of water, ii) immediate increase of solute fluxes after snowmelt and precipitation events, (iii) decrease in solute concentrations with increasing altitude of the RG springs. Moreover, the models are consistent with a series of observations in other studies including (i) the enrichment of the same solutes in RG ice showing high concentrations in RG springs of the Eastern Alps (Nickus et al., 2023), (ii) the observation that in the Eastern Alps high-alpine streams only show elevated concentrations if ice-rich permafrost is present (Wanner et al., 2023), (iii) the kinetically-limited oxidation of pyrite, especially at low temperature (Palandri and Kharaka, 2004; Williamson and Rimstidt, 1994) inhibiting an instantaneous release of solutes by chemical weathering, and (iv) the observation that laboratory water-rock experiments with the same type of rocks were unable

to generate similar solute concentrations as those observed in RG springs in the Eastern Alps (Wanner et al., 2023). With the planed update that ice melt may reside in the unfrozen base layer, the conceptual model will be additionally consistent with studies showing that significant ice melt does not occur in early summer (Brighenti et al., 2021) and hence the seasonal temperature variation in such systems.

6) Strongly revising Section 5.3: We agree with Referee 2 that there are too many limitations to actually quantify the amount of ice melt being exported from the rock glacier. Therefore, we will remove the content regarding such quantification (including equations (2) and (3) and Figure 11). Instead, we intend to more extensively discuss (i) how our conceptual model compares with the explanations others proposed for the export of solutes from ice-rich permafrost, and (ii) environmental implications (see our Response to Comments 19 and 47 of Referee 1). Regarding i) we would like to emphasize that this discussion paper is the first systematically tracking element fluxes in high-alpine streams originating from rock glaciers affected by acid rock drainage. Consequently, we cannot compare our findings on solute fluxes to other studies, as none currently exists to the best of our knowledge. However, we agree that we did not sufficiently discuss how our model of solute mobilization differs from those postulated in other studies (see our Response to Comment 2 of Referee 1).

Comment 23
Conclusion: Your conclusion should then be updated to reflect the revised manuscript and the reframed/refocused narrative.

The conclusion will be updated accordingly.

Comment 24
Technical comments: L42: “rock glaciers stop to creep and are classified as relict” – please double check this sentence - I believe there is a typo or it needs to be slightly rephrased.

The reviewer is right, the sentence should read “If all ice has molten, rock glaciers are classified as relict”.

Comment 25
Technical comments: L77: “Innere Ölgrube” rock glacier – consider including the country in brackets after naming this rock glacier to allow the reader to understand the geographic region of this study.

Thank you for the suggestion. The region and the country will be added after the RG name, “Ötztal Alps, Austria”.

Comment 26
Technical comments: L352 & L353: typo on coma used: 2’570 and 1’830. Needs to be: 2,570 and 1,830.

Thank you for mentioning it. It will be corrected accordingly.

References

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