RC2: 'Comment on egusphere-2024-927', Anonymous Referee #2, 14 Jun 2024

GENERAL COMMENTS

The manuscript by C. Louis, L.J.S. Halloran, and C. Roques provides an interesting and for the rock glacier research community novel hydro-chemical characterization of the previously uninvestigated Canfinal rock glacier and its surrounding springs in the southeastern Swiss Alps. I discuss the manuscript along its three storylines: (1) long-term kinematics and its relations to selected climatic drivers, (2) seasonal hydro-chemical (electrical conductivity EC, stable isotopes, major ions) characterization of several springs below the rock glacier, and (3) diurnal frequency-domain analysis of the EC of the rock glacier outflow. Finally, I have some suggestions for Fig. 10.

Author response (AR): Thank you for your thorough review and positive feedback on our manuscript. We appreciate your detailed comments and suggestions for improvement. We will address each point as detailed below.

First, the kinematic investigations, limited to a multi-year time scale by the available imagery, are interesting and well in line of the observations of the Swiss Permos Monitoring Network. Perhaps sufficient for the hydrological storyline would be the delineation and rough characterization of the rock glacier material (ice content) via the kinematics (L258–265) in support of the morphological evidence of ice-rich permafrost occurrence. Due to the scale mismatch, the relations between multi-year climatic and kinematics trends are hard to connect to the seasonal to daily/hourly hydrological analysis, although links between hydrology and kinematics undoubtedly exist. Still, keep it in the manuscript since it gives clues on the thermal state and provides valuable baseline kinematic observations on a previously uninvestigated, unknown site.

AR: We appreciate your acknowledgment of the value of our kinematic investigations. We agree that a clear connection between multi-year kinematics and annual/seasonal hydrological dynamics cannot be made with the existing data, but we agree with you that both of these analyses have value and should remain in the manuscript. Delineation and characterization of the rock glacier material does not appear feasible with a high level of confidence due to the lack of internal measurements in the RG. We will, however, elaborate our hypotheses on the evolution of ice content in the RG over the period of the available historic imagery.

Second, the seasonal hydro-chemical characterization enabled the seasonal differentiation of water sources contributing to the springs throughout the catchment. This was all very convincing and relevant. The spatio-temporal clustering of surface water chemistry (Figs. 6, S2) at small catchment scale reveals the distinct chemistry and "imprint" of the rock glacier compared to the vegetated plain "La Casina". The Canfinal borehole provides unique insights into the snow and permafrost interactions with groundwater in a thermally sensitive environment close to the lower permafrost limit. Concretely, Fig. 9 shows a link between ground water storage changes (trend of hydraulic head KB4) and EC S1, itself related to precipitation events and time elapsed since snowmelt.

AR: We appreciate your positive assessment on the hydrochemical data and interpretation.

Third, the diurnal frequency-domain analysis of EC is potentially a useful tool in shedding light on the timing of water and heat transfer and applied to the first time in alpine permafrost (to my knowledge). This is an important contribution. I think however that far-reaching interpretations on freeze/thaw cycles based on minuscule

0.5–2 µS/cm oscillations of the EC signal are presented too boldly: Low EC is associated with high discharge (L322) and linked to intense ground ice melt (L324) supposedly driven by diurnal temperature oscillations. Such a behavior might be more typical for (debris-covered) glaciers, but it is not typical for permafrost and rock glaciers. For the lack of independent, local measurements to corroborate these links, this remains a hypothesis and should be framed more cautiously. Please address:

AR: We appreciate your insights into the diurnal frequency-domain analysis. We agree that the text need to be revised to frame the interpretations on freeze/thaw cycles more cautiously, acknowledging the limitations of our interpretations and hypothesis. We will also provide more detail about the frequency-domain method in the SI.

1. No discharge observations are presented to corroborate the presumed (admittedly common) negative EC–discharge relation with discharge maxima in the afternoon. I am aware of the difficulties of obtaining a water level–discharge relation in such terrain. Given your repeated field visits: Do you have water level measurements/visual observations that would attest the afternoon high-flow at least qualitatively?

   AR: Unfortunately, it was not possible to measure discharge accurately in this diffuse system of springs. However, in the SI, if needed, we will provide evidence of discharge dynamics by providing water levels measured at the springs.

2. The assumption of low-EC, "clean" ground ice whose melt dilutes the outflow is untested on the Canfinal rock glacier. I cannot require you to dig a sample, but it should be mentioned that ground ice in rock glaciers was found to have differing solute content. The (few) available measurements of the chemical composition of rock glacier ice (exposures, drillcores) range from low-EC ice (e.g., Murtèl, Haeberli (ed.), 1990) that would result in dilution to "dirty" high-EC ice (e.g., Lazaun, Nickus et al., 2023) that would result in solute enrichment (Brighenti et al., 2021; Bearzot et al., 2023). On top of that, in a degrading rock glacier, two types of ground ice melt in the thaw season: First the 'young' ice in the active layer ('superimposed ice') and later the 'old' permafrost ice (del Siro et al., 2023).

   AR: This is a very interesting comment with very valuable references, thank you. As the reviewer mentioned, we unfortunately cannot sample the ice in the canfinal rock glacier. In the future manuscript, we will make sure to frame the interpretation cautiously and discuss the different hypotheses.

3. Please mention that dilution from ice melt is not the only behavior found in the outflow of rock glaciers. EC is also not necessarily a conservative tracer that solely hints at water provenance (ice melt) in periglacial/permafrost environments. Colombo et al. (2018) lists contrasting mechanisms of solute export and EC-discharge relations, some are regular, some are tied to precipitation events or weather spells, or related to weathering (enrichment, dilution, flushing). Briefly discuss which processes you consider likely given the measured regular EC oscillations.

   AR: Thank you again for this valuable comment. We will make sure to mention the changes in EC to potential processes, specifically related to weathering or sediment export.

4. No ground thermal data is shown to corroborate the diurnal freeze/thaw cycles. Due to the thermal inertia of the active layer, I strongly doubt that temperatures and melt rates at depths typical for ground ice melt in rock glaciers significantly vary on an hourly basis (certainly in the late thaw

season when the ground ice table has receded to depth)! The melting ice must be at or near the ground surface, not deeper than a few tenths of centimeters (the penetration length-scale of diurnal oscillations). Could you get an idea of the active layer thickness on Canfinal? This reasoning rather hints at snow or shallow seasonal ice hidden in the rough terrain – on the rock glacier but also on the adjacent talus slopes and headwalls. This explanation would be consistent with seasonally (broadly) decreasing amplitudes of the S1 diurnal cycles (Fig. 8): waning influence of snowmelt. Nonetheless, the pattern of discharge inversely varying with EC and concomitant with peak air temperatures is also reported by Mateo & Daniels (2018).

AR: Fully agree - we lack plenty of key data for the Canfinal rock Glacier which strongly limits the interpretation. Nevertheless, we will address the lack of ground thermal data and discuss the thermal inertia of the active layer in our revised manuscript. We will consider the possibility of snow or shallow seasonal ice as the source of diurnal EC oscillations and discuss the implications of this for our findings. The possibility of estimating the active layer thickness at Canfinal in the timeframe of this manuscript since ambitious. It will be the subject of future research on the site.

My point is: Your hypothesis is just one chain of processes out of many thinkable ones! Considering the complexity, it is not possible to make all links robust in the scope of this publication. My suggestion is that you introduce the novel diurnal frequency-domain analysis more cautiously as a tool and frame your ice melt-dilution hypothesis as one example of the chains of processes that can be explored with this tool. EC is a commonly measured variable. Many past & future data sets can be analyzed!

AR: Thanks - We appreciate this perspective and will introduce the diurnal frequency-domain analysis more cautiously in our revised manuscript.

Finally, Figure 10, the conceptual model sketch of the annual freeze-thaw cycles and its implications on groundwater flow. It is the first rock glacier hydrological model that focuses explicitly on their role in the entire catchments and brings up the permafrost interactions with deep groundwater flows. This is an important contribution. With a few modifications, permafrost and thermal aspects can be depicted more accurately, namely:

1. The extent of permafrost: The rock glacier, as a permafrost landform, is also in summer frozen (cryogenic, ≤0°C), hence must be enclosed by the 0°C isotherm in both panels. Vice versa for the 'unfrozen till layer'.
2. Time/spatial scales of freeze/thawing: Only the active layer, the uppermost ca. 3–10 m beneath the surface, is subject to annual freezing/thawing. Thermal changes at depth are slow. A pervasive freezing/thawing of the bedrock with large changes at depth as depicted is not possible on a seasonal scale, the sketch rather evokes a long-term (decadal) permafrost degradation. Also, adding a spatial scale would help to grasp the spatio-temporal changes.
3. Site specificity: At the relatively low-altitude Canfinal site, available permafrost distribution maps (Map of potential permafrost distribution (Federal Office for the Environment FOEN) and the SLF 'Permafrost and ground ice map'; https://www.slf.ch/en/services-and-products/permafrost-and-ground-ice-map/) concordantly hint at patchy and likely shallow permafrost in the headwall that is not necessarily connected to the permafrost bound to the rock glacier below.

AR: Thank you for your positive assessment of the value of Figure 10 and for your detailed suggestions to improve it. All suggestions are highly relevant and will be included in the next version of the manuscript.

I suggest that the authors reshape the manuscript and resubmit it. I emphasize that the seasonal hydro-chemical characterization based on your large data set of sampled springs and the connection to the piezometer borehole is convincing. The frequency-domain analysis has its merits as a tool, there is no need to overstretch to explanations that are insufficiently backed up by local measurements. I am looking forward to receiving an updated version of the work!

SPECIFIC COMMENTS

L70ff (study site). Is the catchment currently glacierized or not? What is the mean annual air temperature and annual precipitation?

AR: The study site is currently not glacierized. We will provide the mean annual air temperature and annual precipitation in the site description.

L101ff (methodology). Snow cover duration: The determination of the snow cover duration, given its important role in the analysis, merits a few sentences in the methods section (currently only mentioned on L165). How reliable is the ERA5 snow cover product for complex terrain? To what extent might long-lasting snow among the coarse blocks on the rock glacier surface contribute to melt (Bearzot et al., 2023)?

AR: We will add more details on snow cover duration estimates in the methods section. We will also discuss the reliability of the ERA5 snow cover product for complex terrain and mention the potential contribution of long-lasting snow among the coarse blocks on the rock glacier surface to melt. However, this will remain descriptive as we don't have quantitative estimates/monitoring of snow cover in the catchments. This will be the scope of future research.

L118. When/at which intervals were the five sampling campaigns carried out?

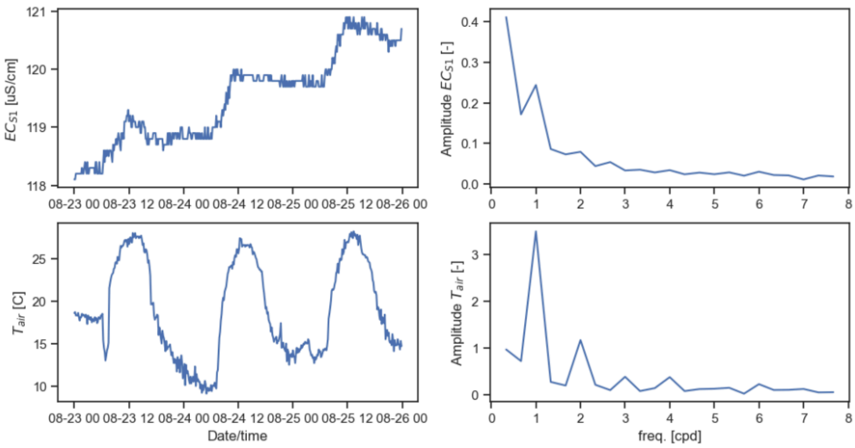
AR: We will specify the timing and intervals of the five sampling campaigns in the methods section to provide clarity on the sampling schedule.

L134. The daily EC amplitudes the frequency analysis is based on are small within 0.5–2 µS/cm (Fig. 9A). No rock glacier study (to my knowledge) has harnessed EC data down to such fine resolution. How does this compare to the precision & resolution of the EC probes? The EC is weakly sensitive to water temperature. How was the measured EC corrected to 25°C? Were the EC loggers fully submerged also at low flow (or shaded/enclosed in a stilling well?) and water temperature reliably measured? Since I am not familiar with this analysis, this is intended as a request for clarification and not a critique.

AR: The EC probes have onboard temperature sensors for the temperature correction of EC. Thus, the analysed values are corrected to 25°C. The loggers were periodically exposed to air during low- and no-flow periods. These data were removed for the frequency-domain analyses. However, absolute values are not relevant when analysing temporal variations in the frequency-domain. Measurement noise in SC (see figure below) is clearly significantly less than the 1 cpd EC variations. We will clarify these points in the text.

L136 and Fig. 9. Especially towards the end of the thaw season, the EC signal is quite irregular and far from sinusoidal. How well does the 1-cpd component describe such a signal with a broader frequency band in terms of amplitude and phase? Showing the 1-cpd component would be helpful to grasp the method.

AR: Fourier analysis enables the isolation of the 1 cpd amplitude and phase, thus eliminating higher frequency "noise" and longer-term trends. Spectral leakage is still, nonetheless, possible. We have chosen the 3-day window as a good balance for minimizing this leakage, utilising the available data (rainfall-influence and dry periods are excluded), and retaining adequate temporal resolution. As can be seen in figures 8 and 9, our approach reveals clear seasonal trends, but shorter-term variations cannot be interpreted. In our opinion, adding a subfigure to Figure 8 or 9 showing example frequency-domain data may add more confusion than clarity, but the inclusion of a figure in the SI appears suitable. For example:



L200. The PCA analysis is very intriguing! Fig. S2 is based on the Oct 2022 samples. How persistent is the found clustering over the season? This is shown in Fig. 6B but could be stated more clearly.

AR: We will look closely into the seasonal variations and include a comment on this in the manuscript. The October survey does, however, show the clearest distinction between end-members. This is expected as, for this autumn survey, snowmelt and precipitation.

L235, L239, L242. Measured facts (diurnal EC variations) are alongside interpretations (dilution behavior, melt driver). Please move the latter to the discussion Sect. 5.3 to avoid repetition (i.e., "…seasonal trends which, for the snow-free period, can be interpreted as measures of the intensity of dilution from RG melt", "The ratio of the EC 1 cpd amplitudes to those of Tair normalizes the EC amplitudes by the main driver of daily melt rate variations", "…indicating a potentially significant contribution from RG meltwater").

AR: We thank you for this comment and agree that parts of this text are better suited to the discussion section.

L301-304: "An isolated contribution from the Canfinal RG cannot be detected…" This important finding is furthermore corroborated by Fig. S2 (PCA, spatial coherence): The distinct geochemical signal is lost a few hundred meters downstream of the rock glacier front. Please add Bearzot et al. (2023) at L304 as they also provided an estimate.

AR: Thanks - The dominant contribution of groundwater at the talus that progressively hides the signal of the rock glacier is an important outcome of this work for the understanding of the hydrology of the site. We will add the interesting reference of Bearzot et al. (2023) to support this has been observed in other contexts.

L290-318: Very interesting!

AR: Thanks!

L312. "Some suggest that bacterial activity…" Who?

AR: We thought about this contribution and will mention the relevant literature in the final manuscript.

L332. Please write “the active layer thickens” instead of “the ice thickness in the active layer decreases”.

Fig. 5. A neat figure!

AR: Thanks! Complement the comment from RC1 regarding figure 4 ;).

Fig. 6. The single most important figure, panel B could be enlarged. Same color coding of the months in panels A and B eases comparison, nice! The ellipses in B) are unnecessary, the different coloring distracts. What do the different circle sizes mean? Could a few key springs (among S1) be marked so that we can follow how their chemistry evolves in the PC plot?

AR: Interesting suggestions. We will make sure to include them in the revised version of this figure.

Fig. 7, caption. Should read “July 2022”, not “July 2002”. Just a thought: Flipping the map or the order of the EC panels would place the data next to location in the map, the more so, as the labels of the EC data set are “hidden” in the subscript of the y-axis label (optional).

AR: You are right - thanks for these comments!

Fig. 9A. What exactly means ‘filtered’ here (cleaned?) and why is the S1 EC here in the range 100-130 µS/cm whereas it is 50-75 µS/cm in Fig. 7? Am I missing something?

AR: We will modify this figure for clarity and add detail to the caption. Regarding the difference between Fig 9A and Fig 7, it was a mistake as non-temperature corrected data was accidentally included in the figure.

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AR: Thank you for these references.