Reply to reviewers – Review 1

Dear editor, dear reviewers. We would like to thank you for your efforts regarding editing and reviewing of our study "50 years of firn evolution on Grigoriev Ice Cap, Tien Shan, Kyrgyzstan". Please find below in blue our replies to the reviewers' comments.

In my opinion, the paper is well written, concise and clear. It requires only small formal adjustments and some integrations, as detailed in the specific comments. In particular, a better description and discussion is required for the measurement techniques and instruments, and some assumptions deserves further details. Formulated hypotheses are completely agreeable, however I think that the authors could include other hypotheses (e.g. for the stable net accumulation I would add the effect of snow metamorphism on wind drift) and possibly use their own data in support. For example, are the SR50 data useful for evaluating snow accumulation, redistribution and ablation?

We thank the reviewer for their positive evaluation of our work. We have addressed their remarks as outlined below. We agree that susceptibility to snow erosion through wind depends on temperature and that the process needs mentioning. The effect on Grigoriev is difficult to quantify but so are our other hypotheses. We have added a brief discussion of the potential impact of snow erosion and temperature.

The SR50 data are not easy to use. Remember that air temperature measurements failed early, together with the thermistor chain. Hence, we cannot correct measured distances for air temperature fluctuations. Furthermore, in 2021 the distance between SR50 and snow cover fell below the minimum measuring distance, making it difficult to interpret the readings. For these reasons we would like to refrain from basing interpretation and conclusions on the SR50 data.

L7 - 'the firn appears remarkably unchanged': which features are unchanged? L19 - I suggest adding 'thermal regime' as a topic of recent studies

Has been changed to *"the firn's thermal regime appears remarkably unchanged."*

L39 - insights 'into' how other…

Changed

L62-63: here the authors report that two categories are recognised, i.e. infiltration ice and recrystallization ice. However, the occurrence of surface melt and significant percolation suggest the likely occurrence of other type of ice formations, for example melt and refreeze crusts (formed at the surface). Please clarify.

Agreed that other types of ice formation play a role. However, given that 55 to 70% of the cores are ice, with typical ice layers being several decimetres thick, more delicate features like melt and refreeze crusts or even wind crusts are of very limited relevance. We also have no records of very fine features from other cores. We have added *"Given the high ice content of all cores (see Section 4.5), already earlier studies focused on infiltration ice and less so on delicate features such as melt-freeze crust formed at former snow surfaces."*

L63 - what is the measurement technique for firn temperature? Was it homogeneous among compared cores? What was the measurement error? (i.e. are there possible discrepancies related to the measurement techniques or instruments?)

The technique of measurements was not homogeneous. We know that some of the early measurements were done by lowering a thermometer into the borehole, other measurements were done using thermistor chains. The main difference to earlier measurements is that we installed the thermistor chain permanently and measure continuously. Before we installed the thermistor chain, the longest measuring period was around a week, possibly also a few days longer. The different measuring techniques might impact the results. However, we have confidence in the earlier measurements for two reasons: (i) The number of past measurements seems high enough to conclude that our 2023 temperatures are within the range of earlier measurements. (ii) looking at our own data directly after installation, we see that about six to ten hours after installations, temperature at 17 m depth is already within 0.04 °C of the value around which it fully stabilized approximately two days after installation. Given that we know that earlier researchers were aware of the time it takes for temperatures to adapt, we are confident in the earlier measurements.

L165 - I think this is a crucial assumption, is there any evidence of negligible ice flow at this site (measured or modeled)?

Unfortunately, there is no measured ice flow at this site. *Van Tricht et al* (2022) modelled ice dynamics of Grigoriev ice cap. Their study indicates horizontal flow velocities close to zero at the summit. It is unclear how representative these simulations are, in particular over longer time periods and thus we feel that citing this study is not helpful in this context.

L180-181 - possible malfunction of the thermistor string? What was the trend in temperature before the multiplexer failure? Any impact from multiplexer failure?

There was no obvious trend before the failure, except for very close to the surface where the firn had warmed since installation. This agrees with our expectations as going from winter into spring, air temperatures had increased since the installation of the chain. We see no reason why the chain should have malfunctioned with the first multiplexer.

L195 - how was the pole sustaining the sensors installed? I mean, was there a support at the bottom of the pole, to prevent sinking in the snow/firn? This could have significant impact in automatic snow depth measurements. Are there snow pits measurements and/or snow depth soundings that confirm SR50 readings and support discussions on snow melt and percolation?

See Figure 1 below. The station had a rather large construction attached to the mast to minimize sinking into the snow and firn. This construction was likely not needed as the ice content of the firn is very high, even near the surface, and numerous thick ice layers prevent sinking in. It would be most challenging to dig into the very icy subsurface and no snowpits were dug. As there is virtually no chance for the station to sink in, we are confident in our measurements. We have slightly modified the text and made clear that our own accumulation measurements are not only based on the sonic ranger (who is too close to the surface since 2021) but also on manual measurements of snow height change.

Fig 1: Grigoriev firn station on 8 February 2018, immediately after installation. Distance from snow surface to the horizontal boom is 2.74 m.

L200 - Arkhipov et al. (2004) 'and' Mikhalenko et al. (2005) state that….

Corrected

Figure 7 - in the caption please add: Shown 'by the blue bars' is the percentage of infiltration ice….

Done

L203 - in my opinion this discussion would benefit from a new figure (or a remake of figure 5a) that compares these different estimates of the accumulation rates, see also the following comment

Unfortunately, we do not fully understand the comment. Does the reviewer mean that previous estimates should be compared with our estimates, which are shown in Fig. 5a? We would like to refrain from adding an additional figure or modifying Fig 5a. We believe that from the text it is clear enough that the various estimates are similar.

L205 - in my opinion the authors should make a clear distinction between percolation and runoff. What do they mean with 'partial meltwater runoff'? Possibly, the authors means that percolated meltwater exceeded the irreducible water content and refrozen water in the firn layers at the top of the 2018 core? How can this be checked and/or quantified? Or is it only inferred by the melt proxies Cl^-/Na^+ ratio and SO_4^{2-} concentration shown in Figure 5b? A discussion is required of these aspects, because they are expected to affect accumulation rates, their historical trends, and possible alterations of former estimates at given depths/periods (I mean, is it possible that percolation and refreezing on firn layers of a given period lead to modification of former estimates of accumulation rates in that period?).

Previous studies showed that ions like Na^+, Ca^{2+}, and SO_4^{2-} are enriched at the firn grain surfaces, from where they can be mobilized easily by small amounts of meltwater, and are preferentially eluted with meltwater (Eichler et al., 2001). The increased melt-proxy Cl^-/Na^+ and strongly depleted concentrations of Ca^{2+}, and SO_4^{2-} suggest a significant influence of percolating meltwater on the chemical records after the beginning of the 2000s. Since no exceptional concentration peak is visible below the layers affected by meltwater, we assume that the meltwater containing the mobilized ions run off. Since this was most likely a small amount (other ions not disturbed), the effect on the accumulation rate is negligible.

We have expanded the discussion on these aspects, also on request from reviewer #2. We have also once again checked the entire manuscript to make sure that the usage of the terms percolation and runoff is unambiguous.

L206 - is it really a loss or a relocation? Please see the previous comment.

See our reply to the comment above.

L232 - Another possible explanation is the positive correlation between air temperature and net accumulation at high-elevation sites, due to effects on surface snow metamorphism and lower susceptibility to wind erosion (please see e.g. Haeberli and Alean, 1985)

Haeberli, W. and Alean, J.: Temperature and accumulation of high altitude firn in the Alps, Ann. Glaciol., 6, 161–163, 1985.

We agree to this important comment. While we cannot quantify any possible effect, we now mention the process and also refer to the study suggested by the reviewer. Towards the end of Section 5.1 we added: *"We note that warmer air and snow temperatures reduce snow erosion by wind (Haeberli and Alean, 1985). As wind certainly plays a role on the exposed summit of Grigoriev, it is possible that warmer air temperatures have reduced snow erosion. The possible effect is difficult to quantify and we emphasize that already in the past, accumulation took place mainly during the summer months."*

L240 - were there possible alterations due to the drilling operations in 2018? Unfortunately, there were not 0°C temperature to be used for checking temperature measurements, such as in 2023.

We do not fully understand what is meant with "positive alterations". Conditions during the drilling where very cold, below -20 °C. Until failure of the multiplexer we did not encounter 0 °C temperatures. The chain was produced in autumn 2017 and calibrated in a 0 °C water-ice bath prior to delivery. Maybe also our above reply on the observed adaptation time of firn temperatures help to clarify this point?

L268 - in addition, the AWS data do not cover the period after 2009

Correct, this should have been clearer. However, we have not added this information here as this comment is related to Reviewer's #2 request of more clearly communicating the limitations of the Tien Shan / Kumtor time series. We have added a paragraph focusing entirely on the Tien Shan / Kumto station (see answer to reviewer #2).

L278 - it is actually a decrease if the authors refer to figure 4a

This is correct and we have modified the text accodringly.

L279 - a runoff of meltwater from summer snow?

Yes. The sonic ranger measurements show how in the summers of 2018 and 2019 snow accumulates (increase of surface height) followed by surface lowering. We interpret the decrease in surface height to be partially due to melt (and partially compaction).

L280 - if a larger fraction of annual precipitation falls during summer, wouldn't the decadal means in δ18O be expected to increase?

Good point. The $\delta^{18}O$ record does not support the hypothesis of increased summer precipitation. We deleted this hypothesis in lines 278-280.

L306 - here I would add a short sentence describing detected trends in major ions and their main causes.

We agree with the referee and rewrote the first two paragraphs of the conclusion to include this information about major ions and also water stable isotopes:

"We analysed major ions and oxygen stable isotopes in water in an 18 m firn core, drilled in 2018 on the summit of Grigoriev ice cap, at 4600 m a.s.l. in the inner Tien Shan mountains. Annual layer counting resulted in a core-bottom age of 1972±3 years. Subsequently we analysed our data in the context of legacy cores drilled at the same location. There is good agreement in major ion concentrations and $\delta^{18}O$ where the 2018 record overlaps temporally with legacy cores.

Firn temperatures measured in the borehole are similar to those from the early 2000s, but also show that temperatures fluctuated substantially over the past five years. During the last ~15 years the melt-index (Cl^-/Na^+ ratio) increased and major ion concentrations became depleted, which we interpret as evidence of accelerated melt and the onset of runoff. The general decrease of mineral dust related ions Na^+, K^+, Mg^{2+}, Ca^{2+}, Cl^-, and SO_4^{2-}, in the period 1972-2018 is in line with declining trends of dust storms in Central Asia. The concentration maxima of anthropogenically derived species NH_4^+, NO_3^-, and BC between the 1970s and 1990s are consistent with other studies carried out in Central Asia. Obviously, despite the influence of melt, the major ions and BC concentrations still reflect atmospheric emission trends, since emissions were highest during periods when melt influence was negligible. The strong increase of the air temperatures during the last decades is not reflected in the Grigoriev $\delta^{18}O$ record, implying that water stable isotopes are not controlled anymore by temperature variations at this site.

The firn was more icy and more dense in the top two to four meters, compared to cores drilled in the 2000s. Firn stratigraphy at greater depth is largely unchanged. We hypothesize that near-surface infiltration ice might support lateral runoff. The latter might result in the removal of latent heat and thereby contributes to stabilizing firn temperature…."

To be consistent with the changes in the conclusions, the abstract was reworded as well:

"…A good agreement is found in major ion concentrations and water stable isotope ratios for the overlapping period. Concentrations of black carbon and major ions, susceptible to being washed out, are reduced since the early 2000s. This indicates the onset of meltwater runoff. Nevertheless, general concentration trends of black carbon and major ions are consistent with observations and Central Asian ice core records, since emissions were highest during periods when melt influence was negligible. The record of water stable isotopes does not reflect the strong increase of air temperatures during the last decades, implying that water stable isotopes are not controlled by temperature variations at this site. Apart from runoff evidence, the net accumulation rates and firn temperatures appear remarkably unchanged since the 1980s. Firn temperatures fluctuate, with 2018 temperatures being the highest on record (∼-1.6 ◦C at ∼17m depth)…"

L309 - please see the comment to L205. Runoff from the shallow firn layers?

Yes, please see our replies there. We have tried to make our interpretation clearer and added also here that we assume that runoff takes place laterally somewhere in the near-surface.

L313 - contributes to stabilizing firn temperatures ‘at the drilling site’ L315 - and/or reduced wind scouring?

Thank you for the important remarks on the effect of temperatures on wind. We have added *“and/or reduced wind scouring”* here.