Review of the manuscript: **50 years of firn evolution on Grigoriev Ice Cap, Tien Shan, Kyrgyzstan**

by H. Machguth et al.

General comments

The manuscript from Machguth et al. reports and extension of available ice core records at the Grigoriev Ice Cap, analysing a new core collected in 2018 for firn stratigraphy, major ions, black carbon, water stable isotope ratios and total β-activity. They find a good correspondence in the period overlapping former firn cores, and a reduction in the concentration of major ions since the early 2000s, which they relate to a recent increase in temperature and melt water percolation. The firn stratigraphy was found unchanged, with the exception of the shallow layers, and after an increase in 2018, its temperature in 2023 was found similar to the early 2000s.

Then the authors discuss the results, in particular the temperature stabilization and the largely unchanged net accumulation rate, in spite of increased percolation, formulating plausible hypotheses that might explain the observations.

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?

Overall, my opinion is that this paper is publishable after a minor revision.

Specific comments

L7 - 'the firn appears remarkably unchanged': which features are unchanged?

L19 - I suggest adding 'thermal regime' as a topic of recent studies

L39 - insights 'into' how other…

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.

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?)

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

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

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?

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

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

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

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 $SO2^{-4}$ 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?).

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

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.

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.

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

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

L279 - a runoff of meltwater from summer snow?

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

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

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

L313 - contributes to stabilizing firn temperatures 'at the drilling site'

L315 - and/or reduced wind scouring?