Reply to reviewers – Review 2

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.

The manuscript shows interesting data and the presented hypotheses are generally well supported. Overall it is well written, with some minor exceptions regarding a couple of paragraphs (see specific comments) which could be written in a clearer manner.

In my opinion, there is an issue that needs to be fixed regarding the weather station data: since this data is referenced and discussed many times in the text, it needs to be stated once and for all, and at the beginning of the manuscript, that this data must be taken with caution. It cannot be repeated in every paragraph like it is at present time in the discussion section.

The manuscript would also benefit from a more in-depth discussion regarding the total beta methodology and the major ion results.

We thank the reviewer for their positive evaluation of our work. We have addressed their remarks as described in detail below. We extended the description of the beta method: *"Every sample was filtered three times by filtering the filtrate twice. The filters were dried at 60 °C over night, and analyzed for total β-activity. Each sample was measured six times 1 h at a Berthold LB 790 detector and the results averaged. Final β-activities were blank corrected (Eichler et al., 2020) using the blank value of 33 mBq kg-1 of ultrapure water."* Furthermore, We have also merged the various statements on the data quality of the Tien Shan / Kumtor weather station, now appearing in Section 2: *"The Tien-Shan / Kumtor weather station is located at 3614 / 3660ma.s.l., ~20 km east of Grigoriev (Engel et al., 2012). The meteorological record starts in 1930, in 1997, the station was moved a few kilometers. The two names and two elevations refer to until the year 1997 / since 1997. The moving of the station introduced potential step-changes in meteorological parameters. For example, Engel et al. (2012) caution that it is unclear whether the above mentioned increase in average annual precipitation is primarily caused by a change in weather conditions or by the moving of the station. Because Tien-Shan / Kumtor is the only source of long-term meteorological measurements in the vicinity of Grigoriev, we use the data, qualitatively, to aid interpretation of the ice cores. We emphasize that any conclusions related to the station data need to be considered preliminary."*

L43-44. There is quite a difference in the data from before and after the moving of the weather station. I think this needs to be pointed out and further discussed since this data is frequently mentioned in the manuscript (see also general comments on this data). Engel et al (2012) point out that there is a significant difference in the two time series which should not be considered unitedly.

We agree with this remark and hope that the new paragraph on the two weather stations makes this clear (see our reply above).

L61-68 "For the remainder of our study…" I think it would be best to rephrase here to make it more clear why the authors chose to focus on these specific cores. It is mentioned in the following lines, but in my opinion it is not clear that these are the reasons why.

We now start the following sentence with *"We focus on these cores because ..."* followed by the explanations already there.

L62. “that is information about...” wording is unclear: are saying that when you say stratigraphy has been reported you mean that for all cores you have information on the presence and position of infiltration and recrystallization ice, but not dust layers?

This has been clarified.

L82. misspelling of “polypropylene”

Corrected.

L102. From which part of the core did you prepare the samples for beta activity and which depth range did each sample cover? In my opinion, the explanation of this methodology could be slightly expanded.

In line 102 it is already written that the ten samples for beta activity cover the range 7-16 m. To clarify that the full depth range is covered by the ten samples, we added to L102: *“…ten samples for beta activity covering continuously the range between 7 and 16 m depth and one blank sample…”*

L116. “the station was accidentally…” I don’t understand the importance of reporting on this visit. I would also rephrase this paragraph to make it more immediate which are the time periods with available data.

This remark had some importance in an earlier version of the manuscript but has since become irrelevant. We have removed the sentence. We have now added the information that after replacement of the multiplexer, the station recorded data again.

L136. “only a quarter occurs from October to April” Would it be possible to sustain this claim but with more recent data from the weather station instead of citing a 30 year old paper?

We have now complemented the citation of Mikhalenko (1989) with an estimate by Engel et al. (2012). Both numbers refer to the Tien Shan weather station.

L138. “ generally highly correlated” I think this information needs to be better quantified.

We added the correlation coefficients for quantification: *“Concentrations of the major ions Cl^-, Na^+, SO_4^{2-}, Ca^{2+}, K^+ and Mg^{2+} are generally highly correlated among each other ($0.61<r<0.94$)…”*

Fig.3a I think it would be best to highlight the difference between measurements above and below the background for the total beta activity.

The values in Fig. 3a represent blank-corrected beta activities. The blank value is 33 mBq kg^{-1}. The term “background activity” (18-60 mBq kg^{-1}) was just used to qualitatively describe the activities for all samples except the maximum values of 83 and 108 mBq-kg^{-1}. We agree with the referee that this was not fully clear from the present manuscript and adjusted this information in Lines 107, 143-144 and Fig. 3:
Line 107: *“Final β-activities were blank corrected using the β-activity of ultrapure water of 33 mBq kg^{1}.”*
Line 143: *“Blank corrected total β-activities vary between 18 and 108 mBq kg^{-1} (Fig. 3a). Two of the samples show maximum activities of 83 and 108 mBq kg^{-1}.”*
Fig. 3: a) *“… blank-corrected β-activity with analytical uncertainties…”*

L145. ”retention of 137Cs” This statement needs a reference for support.

We added the reference and adapted the sentence: *“…The dust-normalized β-activity (ratio of β-activity/[Ca^{2+}]) also reveals a maximum at the same depth (not shown). Retention of β-activity on the filter, depends on the total quantity of insoluble matter (Picciotto et al., 1963)…”*

Picciotto, E. and S. Wilgain. 1963. Fission products in Antarctic snow, a reference level for measuring accumulation. J. Geophys. Res., 68(21), 5965-5972.

L149. What is the error on the 12m depth which is arbitrarily assigned? I think it is important as this tie point is crucial in the dating of the ice core, which is thus affected by this error.

There is no "error" on the 12 m depth.
The maximum of the beta activity is represented by two samples (11.3-12 m and 12-12.9 m depth, see Fig. 3a). Thus, we conclude that the year 1986 is included in both samples, and therefore we attributed the depth of 12 m to the year 1986. Lines 147-149 were changed accordingly:
"Based on measured β-activities, we conclude that fallout from the Chernobyl accident is included in two samples (11.3-12 m and 12-12.9 m depth, see Fig. 3a). Thus, the depth of 12 m in between these two samples was assigned to the year 1986."

Fig4. Error needs to be shown for this data. It is difficult to compare the 3 cores without it. Additionally, Does it really make sense to show here the NH4 and BC data since there is no comparison with the other ice cores? It is stated that the 2003 core also had glaciochemical data, but none is shown here in comparison, why?

We have updated figure 4 by (i) adding uncertainties where possible. (ii) we have changed the scale of $\delta^{18}O$ to allow direct comparison of *T* and $\delta^{18}O$ records. (iii) We prefer to leave NH_4^+ and BC data in the manuscript for discussing long-term trends in comparison to other Central Asian sites (L292-295). iv) The glaciochemical measurements carried out on the 2003 core offer few possibilities for direct comparison. *Usubaliev* (2003) focused more on metals, Ca and Na would be the only possibilities for direct comparison to our data, as well as $\delta^{18}O$ (*Kutuzov,* 2005). However, for a comparison like shown in Fig. 4, the 2003 core would need to be dated accurately. Initial alignment of the 2003 record against the 2001 core was been done by *Kutuzov* (2005) but more statistical work would be needed. We feel this is outside the scope of our study.

L156. " for all 4 parameters…" This statement needs to be better motivated.

The updated Figure 4 now shows that the parameters overlap within their bounds of uncertainty. We also mention this now in the text.

L166-170. I think here it needs to be better explained why you are using Cl/Na ratio and showing SO4 concentrations. Just better wording to make it more clear.

The deposition of sea salt (NaCl) is the dominant source of Na^+ and Cl^- in the snow at the drilling site. Accordingly, firn core parts barely influenced by meltwater percolation show a Cl^-/Na^+ (mass) ratio of 1.78, corresponding to the sea-salt ratio. If firn layers are subject to meltwater percolation, Na^+ is preferentially removed with respect to Cl^- (see e.g. Eichler et al., 2001). Then the Cl^-/Na^+ ratio exceeds the sea-salt ratio, as observed for many years after 2005 in Fig. 5b. Similar to Na^+, SO_4^{2-} is strongly subjected to meltwater relocation as indicated by the low values in SO_4^{2-} concentrations for years with maxima in the Cl^-/Na^+ ratio. We clarified the text accordingly.

L266. "for the years 1998…" This needs to be explained better since looking at Fig. 4a there is an apparent increase in air temperature.

Indeed, Fig. 7 in Engel et al (2012) suggests that, after a strong jump in air temperatures around 1998, there was no substantial change until the end of the record in 2009. The increase in decadal mean air temperatures shown in Fig. 4a agrees with this. Nevertheless, the data quality of the Tien Shan / Kumtor series is an issue. Since we were here trying to explain why firn temperatures in 2023 are similar to the early 2000s, it is even more of a problem that the available weather station record ends in 2009. For these reasons, we have removed the third potential explanation.

L276. In my opinion, the discussion of the difference between 1990 and 2018 data would work better if moved to section 4.3

We agree that there are a few pieces of quantitative information that could be placed in the results section. However, as most of the statements made here are discussion, we prefer to keep them where they are.

L276-282. This section is not very clear and could be phrased better to convey the message that dO18 is not conserved well probably due to percolating water.

We agree with the referee and in addition to deleting line 278-280 in response to referee 1, we add a sentence at the end of the paragraph: "*In conclusion, 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".*

L285. "In general we observe lower.." Is there a significant difference between concentration in the upper and lower part of the core? If so, how much? Are there also differences with respect to concentrations measured in other central Asian cores? I think this discussion would be interesting to add.

As suggested by the referee we investigated the differences between concentrations in the upper and lower parts of the core more quantitatively. Furthermore, the discussion of the two potential reasons for these differences was better separated and explained in more depth:

"*In general, we observe lower concentrations in the topmost 7 m, i.e. from the year 2001 on. Concentration ratios between the periods 2001-2018 and 1974-2000 are 0.31 (SO_4^{2-}), 0.36 (Ca^{2+}), 0.38 (Mg^{2+}), 0.46 (BC), 0.48 (Na^+), 0.51 (K^+), 0.58 (Cl^-), 0.6 (NO_3^-), 0.68 (NH_4^+), and 0.88 (F^-). Potential reasons for the decreasing concentrations are (1) meltwater-induced relocation and runoff and/or (2) changes in the emission source strength.*

(1) *Increased air temperatures (Engel et al., 2012) likely have caused stronger melting, preferential elution, and possibly removal of ions by runoff. This hypothesis is supported by the observation that F^-, NH_4^+, and NO_3- are less depleted. Those ions are known to be less prone to removal by meltwater due to their location in the ice matrix (Eichler et al., 2001; Moser et al., 2023).*

(2) *Concentrations of dust-related highly correlated ions Na^+, K^+, Mg^{2+}, Ca^{2+}, Cl^-, SO_4^{2-} agree well between the 1990 and 2018 cores for the overlapping period (Fig. 4b, c) and generally reveal a strong decrease after the 1960s. This coincides with declining trends of dust storms in Central Asia during that time established from stations in Xinjiang and the Karakum desert (Grigholm et al., 2017). Concentrations of pollutants of mainly anthropogenic origin peak between the 1970s and 1980s (NH_4^+ and BC) or the 1990s (NO_3^-) (Fig. 4d-f). Concurrent maxima of these species have been obtained from other Central Asian firn or ice cores, such as Inilchek (Tien Shan, Grigholm et al., 2017) and Belukha (Siberian Altai, Olivier et al., 2003; Eichler et al., 2009) or from an Elbrus core (Caucasus, Preunkert et al., 2019). For SO_4^{2-}, all other Central Asian cores reveal a maximum in the 1970s reflecting the SO_2 emission history from different former Soviet Union countries (Olivier et al., 2003; Grigholm et al., 2017). The strong SO_4^{2-} decrease at Grigoriev from the 1960s on and the high correlation with dust proxies such as Ca^{2+} suggests that at this site natural dust sources for SO_4^{2-} dominate compared to anthropogenic ones (see above).*

In conclusion, despite the influence of melt, the general concentration trends of major ions and BC are consistent with observations and Central Asian ice core records, since emissions were highest during periods when melt influence was negligible. "

L286. "the reason could be…" In my opinion, the discussion would be more clear if the two hypotheses (meltwater-induced relocation and source strength change) were discussed more separately and a little more in-depth.

See answer to comment above. The detailed discussion of the two hypothesis is now done more in depth and

separately.

L288. “concentrations are less depleted” Less, but still depleted with respect to the lower part of the core?

Yes, all are depleted to some degree. This has now been quantified while this entire part of the discussion was revised.