

General Remarks

The study by Ochwat et al. has been modified substantially. I would like to thank the authors for making a thorough effort in revising their study. Generally, I believe the study has improved and has gained in focus. The inclusion of firn modelling supports the main message that the aquifer is likely a new phenomenon (but also raises some questions, see below under detailed remarks).

My main concern is that the authors compare firn cores that are as much as ~10 km apart (the 1964 Divide core and the 2018 Kaskawulsh core, the distance between the 1964 and 2006 Divide cores is much smaller but unknown), while not commenting on potential issues of such a comparison. I am concerned with this point based on my own research experience: I was involved in a study quantifying firn changes from 1973 to 2018 on Abramov glacier, Kyrgyzstan, Central Asia (the paper will be published in *Journal of Glaciology* within the next couple of weeks). When we visited the glacier in winter 2018, we thought that we knew the exact location of the historic 1973 firn profiles and drilled at that site. When back from the field, we received further historic documents which showed us that we had drilled about 250 m away from the historic site. We visited the glacier again in summer 2018 and drilled at the historic location. Together with extended GPR measurements, this provided us with the possibility to quantify short-scale variability of accumulation rates and firn properties on the relatively large mountain glacier (24 km²). Although both drill sites look very similar (they both seem so be located in the same flat plain), the variability is large. Over a distance of 250 m, mean annual accumulation varies by a factor of almost 1.7. If we had compared the historic 1973 profile to our core drilled 250 m away from the historic site, our conclusions with respect to firn changes (accumulation, firn ice content, and more) would have been dramatically wrong. I am aware that Abramov glacier probably shows more small-scale variability as the accumulation area of the much larger Kaskawulsh. But even on the Greenland ice sheet, I would be cautious when comparing cores that are ~10 km apart. Hence, I would like to ask the authors to at least mention such potential issues and to thoroughly evaluate and discuss to what degree drill sites, conditions at the drill sites and the cores are comparable.

Detailed Remarks

Lines 315-319: I do not agree to these statements. The fact that the firn was at 0 °C in 2006 (0 to 10 m depth) and cold between ~1 m and ~15 m in 1964 does not proof firn warming. To reliably quantify firn temperature changes, temperatures below the depth of zero annual amplitude (roughly 10 to 15 m depth) need to be compared. There is a high risk that the differences discussed here and shown in Fig. 5 are just the result of weather conditions and not a climatological signal. This risk is amplified (i) by comparing average temperatures for only the top 10 m and (ii) by the 1964 core showing 0 °C at ~15 m depth, potentially indicating that the firn was temperate already then. At the minimum it needs to be acknowledged that the data have to be used with care when quantifying firn temperature changes. I also suggest evaluating and discussing potential uncertainties.

Lines 351 – 352: “Meltwater which does not ...” I do not understand the statement, does the model also simulate lateral drainage? If yes, consider updating the model description. If not, please remove the statements as this would not be a result of your modelling efforts.

Line 375: “These Characteristics ...” a bit confusing, the previous sentence describes the current situation, not the original situation.

Lines 379 – 380: As mentioned above, I do not think the data fully support this conclusion.

Lines 388 -394: This could be placed in the introduction or description of data and methods. In my opinion, these are general statements based on the literature and do not fully fit here in the discussion.

Line 397: “... affects continue ...”, something is wrong here.

Line 399: I do not understand these comments. Sorge's Law has been derived from the study of dry firn at Eismitte, roughly at 3050 m near the centre of the Greenland ice sheet (Sorge, 1935). It is intended to reflect certain basic characteristics of dry firn under a constant climate. It was not intended to be valid for firn which experiences substantial melt under a warming climate. To my understanding, this is also how the law is formulated and the term “Sorge’s Law” coined in Bader (1954).

Lines 437 – 438: My apologies for not making the Central Asia glacier studies available that document a firn aquifer on a mountain glacier already in the 1970s. Please find the studies by *Glazirin et al.* (1977) and *Kislov* (1982) available for download under this link:
<https://drive.switch.ch/index.php/s/51wrYzVb9r4XRSh>

Lines 489-490: “This is a conservative estimate ...” I do not fully understand what is meant here.

Figure 1: I could not find a clear reference to the IRRP A Site in the text. I suggest adding a clear reference to this site in the text (what, when and who measured there) or remove the site from the figure.

Figure 6: Which location is modelled? While a location is clearly indicated in, e.g., Figure 5, there is no information here and also in the text where Figure 6 gets referenced. On line 233 it says that “the study site” is modelled, furthermore it is stated that model forcing ERA data are compared to the “Divide” meteorological observations. However, the study refers to a rather large area with locations at different elevations. Hence the question what exactly is modelled?

Figure 6: Related to my comment above: It is not fully clear what is modelled and whether the model output can be compared to the Divide, the Kaskawulsh field data or both? Nevertheless, I note that the modelled firn temperatures at 10 and 20 m depth in the 1960s are around -12 °C while the 1964 core drilled at Divide indicates a temperate firn regime (0 °C at ~15 m depth). I consider this a substantial disagreement between measurements and model.

Figure 7: It looks like there are white areas at the top of Figure 7a (data gaps or variations in surface elevation?) which do not appear in Figure 7b. Why the difference?

References: Now the Machguth et al. citations are fully confused. There is a 2016 paper in Nature Climate Change and a 2006 paper in Geophysical Research Letters. You have now created a combined citation of both papers :-). Please correct this citation but also check all the other references for correctness.

References not listed in the manuscript

Glazyrin G.E., Glazyrina E.L., Kislov B.V. and Pertzinger F.I. (1977) Water level regime in deep firn pits on Abramov glacier [in Russian], volume 45. Gidrometeoizdat

Kislov, B.V. (1982) Formation and regime of the firn-ice stratum of a mountain glacier [in Russian]. *Ph.D. thesis*, SANIGMI Tashkent.

Sorge, E. (1935), *Glaziologische Untersuchungen in Eismitte, Wissenschaftliche Ergebnisse der deutschen Grönland-Expedition Alfred Wegener 1929 und 1930/31*, in: K. Wegener, im Auftrag der Notgemeinschaft der Deutschen Wissenschaft (Ed.), Band III, Glaziologie.