Anonymous Referee #1

Ochwat and co-authors use firn cores drilled in the accumulation area of Kaskawulsh Glacier, Yukon Territory, Canada, to estimate changes in surface height over the period 2005 to 2018. In the deeper one of the firn core, they could observe a perennial firn aquifer at more than 30 m depth. Based on the cores they state that the surface at the drill site has lowered by ~1.3 m over 2005 to 2018. They emphasize the importance of this result for geodetic glacier mass balance estimates of the glacier and the region. Ochwat and co-authors make a valuable contribution to the understanding of firn properties in a heavily glacierized region. The area is also considered one of the key regions contributing to global sea level change. The authors address tow main points: (i) the perennial firn aquifer and (ii) surface lowering in the context of geodetic mass balance estimates. Both topics are very interesting scientifically and important in the context of regional to global glacier change. However, I have the impression that in its current form, the manuscript fails in making sound contributions to either topic.

The reporting on the discovery of the aquifer is very valuable, but the discussion of the observations remains unspecific, rather general with mostly qualitative comparison to other firn aquifers.

We will revise the discussion to be more specific. The intention of the project was to study the densification of the firn in the region. When we drilled to 32 m we discovered the firn aquifer and realized that it had not been previously reported. We do not know much about the aquifer, but its presence is unequivocal – liquid water dripping from the firn core samples from 32 to 34 m depth – and it is a significant discovery that will be of interest to the *Cryosphere* audience. To our knowledge, this is the first reported firn aquifer in the St. Elias Mountains and the Alaska-Yukon region. We have interesting video footage of the liquid water discharge from the core samples that could be added as a link to the online manuscript. We have also been in touch with Dr. Karl Kreutz and Dr. Seth Campbell of the University of Maine, who have recent unpublished radar data from a site 12 km away that provides evidence for the firn aquifer, across a larger area than we originally reported. We will incorporate their observations into the updated manuscript, together with a discussion of the climatology of the region, which is consistent with the formation of firn aquifers found elsewhere (high accumulation rates, cold winters, moderate summer melt).

I believe that the analysis of the firn core, the way it is presented, does not allow retrieval of thinning rates. To do so, additional information is needed, namely evidence of changing density or ice content over time. This evidence is missing, or little used in the argumentation. Consequently, I doubt in the main conclusion of the study.

We will strengthen our explanation of how we retrieved thinning rates and discuss the evidence when arguing for the main conclusion. To determine how densities have changed over time we will make direct comparisons with density data recorded in a 15 m core taken in 1964 (Grew and Mellor, 1966) in a location close to our field site. Preliminary analysis indicates that the firn was less dense then, and this information will be combined with other unpublished historical data from the region (available from colleagues and historical reports) to quantify how the densification rate has changed over time.

Uncertainty analysis generally appears incomplete (see below for details).

We will explain the uncertainty analysis in greater detail and offer more details in the specific responses below. We did complete careful uncertainty analyses for the firn density itself, and there was misunderstanding concerning the uncertainty in point samples vs. average densities, as averaging greatly reduces the uncertainty for random errors, by a factor of $\sqrt{N}$. We will revise the text to be clearer about this. We agree with the reviewers, however, that we did not adequately address the uncertainty in the estimate of average accumulation (hence age) of the core, so we will add this uncertainty analysis to the manuscript.

Detailed Remarks

2.1 Study area: Where is the longer-term average equilibrium line elevation (ELA) on Kaskawulsh glacier? This would be useful to better understand the glaciological situation of the drill site (e.g. 100 m or 1000 m above the ELA?)

The ELA will be added to this section, as reported in Foy et al. (2011) and Young et al., (2020). In Foy et al. (2011), an ELA of 1958 m a.s.l. is used throughout their study as a long-term average determined by satellite imagery. It will also be noted that the ELA has shifted upwards since that report by almost 300 m. Young et al. (2020) state a mean of 2261 ± 151 m a.s.l. for the years 2013-2019. Our core site is nonetheless well above the ELA at an elevation of ~2640 m a.s.l., within a broad plateau in the main accumulation area of the glacier.

Line 111: upper threshold of 917 kg m^{-3}: in a firn aquifer, higher densities are physically plausible provided that the water is still in the core segment when weighing. Was this an issue in the context of your study?

The cores drilled in the firn aquifer were drained before being bagged and there was no noticeable liquid water in the sample bags when weighed. That said, this is a good point and our samples could have contained residual liquid water; some may have refrozen due to sub-zero surface temperatures during the drilling. We will note this in this section, and acknowledge in the manuscript that the three outliers from 32-36m depth may have had liquid water in them, thus causing a higher density.

Lines 115-116: I do not understand why damage to sample bags affected density measurements, I understand density measurements were carried out in the field, before transporting samples?

Density measurements were carried out in the field for most of the measurements. However, due to the need to leave the glacier at the end of drilling, a few remaining measurements were carried out at the research station within 24 hours of being back. Some of the sample bags were damaged in this transport and were not included in the measurements at the research station. We will clarify this in the text.

Lines 120: What exactly is meant with "human error"?

This term will be rephrased to random error. We meant the error associated with core measurements (length, diameter) and the subjective assessment of core completeness. These are all considered to be random rather than systematic sources of error.

Lines 145-146: Note that for example Harper et al. (2012) measured a lower density for pure ice (843±36 kg/m^3). Furthermore, you list the wrong study of Machguth et al. (2006 instead of 2016) in the references. Please check references for more errors.

Thank you for noticing the typo in the year of the citation. This will be fixed and other references have been double-checked.

We noticed that Harper et al. (2012) determined a lower density. Based on four core sections that had 100% ice in our study, we measured pure ice to have an average density of 907 ± 14 kg/m^3 in our data. This is above the reported values of Bezeau et al. (2013) and Machguth et al. (2016), which are in turn higher than the value of Harper et al. (2012). We chose to go with the middle value. In the revised manuscript we will assign an uncertainty of 35 kg/m^3 in order to accommodate both ends of the possible spectrum of density for the pure ice sections (i.e., inclusive of both Harper's work and our own data).

Lines 154-159: Why thinning? I would agree if reference is an ice core without ice lenses, but it needs to be show that this theoretical reference actually existed at the drill site earlier (2005).

The thinning discussed in the original manuscript is due to annual densification of the accumulation from meltwater percolation and refreezing: a thinning that is not associated with mass loss. The reviewer is therefore correct that it did not indicate densification changes over time. To address changes in densification over time, we will incorporate historical data into the revised manuscript from the original Icefield Ranges Research Reports (as mentioned above), particularly from the 1960s density profiles of Grew and Mellor (1966).

Discussion: The discussion is clearly structure but I perceived the flow of arguments as poor. The text meanders between more general, partly speculative and maybe too qualitative discussion of firn aquifers to the impact on geodetic mass balance estimates. It is not fully clear what the focus of the manuscript is, or what the main message(s) of the manuscript should be.

We apologize for this lack of clarity. The main objective of the original manuscript was to characterize the firn of the upper Kaskawulsh Glacier: a significant ice mass within a major icefield where little or no published data is currently available on firn density or densification rates, meltwater retention, or liquid meltwater storage. The revised paper will be expanded to clarify and reiterate the three main messages: 1) firn density and ice content, 2) changes in densification rate, and 3) the new firn aquifer in this region. The results and discussion will focus around these three points. Number (3) is admittedly a bit of an aside, but it is of great interest and is relevant to meltwater retention and mass balance studies, as well as affecting the glacier thermal and hydrological behavior.

Lines 265: The statement cited from Christianson et al. (2015) appears incorrect. Already in the 1970s detailed studies of a perennial firn aquifer were carried out in the accumulation area of Abramov glacier (4400 m a.s.l.), Pamir-Alai, present-day Kyrgyzstan. In contrast to other studies, the scientists studied the aquifer in a deep firn pit (up to ~25 m deep). This allowed continuous monitoring of changes in the water table in relation to, e.g., surface melt intensity. The related studies, however, are mostly published in Russian (Glazirin et al., 1977; Kislov, 1982) and thus not widely known to a broader glaciological audience.

We would like to include these Russian papers for reference in the manuscript. However, despite extensive searching we have not been able to find them and hope that the reviewer can forward them to us so that we can include the data mentioned in the above comment.

Lines 270-272: Here an estimate of annual accumulation rate is mentioned, based on literature and the authors' own interpretation of the cores. Above (lines 196-198) the authors use a literature value (other sources than here) of 1.76 m w.e. yr^{-1}. How do these two numbers relate? Is the implicit assumption made that accumulation rates have remained stable since the 1960s? What is the uncertainty introduced by this assumption?

We have obtained a new dataset from 2003-2013 of snow accumulation and density data from the Divide site (12 km from our drill site, similar elevation) on the upper Kaskawulsh Glacier. We will include this data in our estimate of annual accumulation rates in order to provide more supporting evidence for the accumulation rate chosen here. The interannual variability in this data could be used as an estimate of uncertainty. We have three additional lines of evidence for the annual accumulation rate: (i) preserved peaks in the oxygen isotope record, (ii) our own winter accumulation measurements from spring 2018, and (iii) the (much) earlier published data from the IRRP. This will all be taken into account to assess a conservative uncertainty in the annual accumulation rate, which can then be propagated through to the uncertainty in the age of the core.

Line 284 (as well as 184-190): 2 kg m^{-3} appears to be a very low level of overall uncertainty. I assume there must be some potential sources of systematic errors that prevent such a very low uncertainty?

We will double check our calculations, but believe this is just confusion regarding the uncertainty in point samples vs. average values for the core. We use standard error analysis in these calculations, which we are happy to walk through in supplementary material if the reviewers would like to see it. To summarize here, our point samples (10-cm core sections) have significant measurement uncertainty, e.g., 500 ± 75 kg m^{-3}. Sources of uncertainty are random rather than systematic, to our knowledge. Figuratively speaking, for the case of random errors, averaging of the 10-cm density values for the whole core effectively leads to reductions in uncertainty because random errors cancel out. Based on standard error analysis, one can take the example of a 30-m core with 300 10-cm density values ($N = 300$). The standard error in the average follows $s_e = \sigma/\sqrt{N}$, where in this illustration we can take $\sigma = 75$ kg m^{-3}. This gives $s_e = 4$ kg m^{-3}. We will nonetheless go through our uncertainty calculations again to ensure that these are accurate.

Section 4.3: I think your interpretation stands on weak grounds. There is little evidence presented that accumulation rates from the 1960s are still valid today. As outlined below, the fact that ice lenses exist in the firn does not automatically mean that the surface lowers. For this to be true, the ice fraction has to change over time. The authors present some evidence of an increase in ice content (lines 288 to 291), but not for the time period represented by the two cores.

We will strengthen our interpretations by bringing in more of the recent measurements reported by Foy and others, as well as those made by ourselves in this region over the past ~15 years, to quantify whether accumulation rates have changed over time. The point we were making is that meltwater refreezing increases the density of the firn, thinning the annual accumulation layer without an associated mass loss. This also makes “dry firn” models inappropriate for density estimates that are needed for geodetic mass balance measurements, so even the basic reporting of firn density values and firn ice content are of value. We do understand that firn densification associated with multi-year changes in ice content is also of great interest, so we will include a new analysis of density changes over time in the revised manuscript.

Line 322: Surface lowering of 1.3 m: It is confusing to mention this result in cm yr^{-1} in the abstract, not in the results (at least I couldn’t find it there) and then again in m yr^{-1} in the discussion.

We will revise the manuscript such that the units are consistent throughout the text, thank you for pointing out this inconsistency. Additionally, we will elaborate on how the 1.3 m surface lowering has been calculated, as well as the uncertainty associated with the value.

Lines 322 – 332: I do not understand why there needs to be surface lowering because of the ice lens formation and refreezing? If we knew that there was no or less refrozen water in the firn in 2005, then the surface would have lowered as calculated. However, based on the evidence the authors present, I have the impression we do not know whether ice content has changes 2005-2018. If ice content in the firn would be constant, there would be no surface lowering.

We understand the reviewer's point, and it makes it clear that we did not explain our calculations and objective clearly. In the original manuscript we reported the ice content present in the firn during the period of our study, which we used to determine the extent of surface lowering related to the meltwater percolation and refreezing affects within the firn. This process of densification creates an effective thinning, but not the multi-year changes in this process (i.e. classical firn densification). In the revised manuscript we will address this by using historical density information from the Icefield Ranges Research Reports to provide evidence as to how the firn density and ice content has changed over time.

Furthermore, the authors make the critical assumption of annual accumulation rate equaling 1.76 m w.e. yr^{-1}, leading to the conclusion that the core represents the time period 2005 to 2018 (Lines 196-198). What is the uncertainty of this assumption? Accumulation rates could have changed since the 1960s. Associated uncertainties are neither assessed nor discussed.

This point is addressed above. We agree that uncertainties need to be assigned and discussed, including error propagation through to the calculation of the age of the core.

Lines 353-355: The fact that an aquifer exist does not mean that the surface has to lower. Evidence is needed that firn properties altered over the time of investigation. If they have not changed (i.e. there was similar ice content earlier, an aquifer existed and accumulation rates remained constant), why should the surface lower?

The presence of any kind of liquid water would be a form of firn densification, but in the sense that we were referring to (potentially confusing to readers): surface melting causes surface lowering, and where this meltwater is retained as liquid water or refrozen ice there is a measurable surface lowering that is not accompanied by mass loss. We have undertaken a comprehensive literature search in response to this comment, and found no evidence that the firn aquifer existed in the past. The presence of the new firn aquifer therefore makes it likely that the surface has lowered due to the firn aquifer's presence in the recent past, and we will provide estimates of how much this lowering has been. We will report this in the updated manuscript, and provide supporting information.

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, SARNIGMI Tashkent.

Please see the previous comments regarding the addition of these references into the manuscript.