Dear Dr. Etienne Berthier,
Thank you for the extension and allowing us the time to implement significant changes into the manuscript. Due to the extent of the changes made we do not have a document that has the "tracked-changes" on it or a point by point description of the changes. We have added several new sections to the manuscript, a surface energy balance model coupled with a firn model, historical data from 1964, data from 2006, and revised the introduction, methods, discussion, and conclusions accordingly. We removed the section on stable isotopes and revised or removed some figures and tables. We have also changed the title to "Evolution of the firn pack of Kaskawulsh Glacier, Yukon: meltwater effects, densification, warming, and the creation of a perennial firn aquifer" to better match the new manuscript. We believe these changes will make the manuscript much stronger.
Thank you,
Naomi Ochwat, on behalf of the coauthors.

Below you will find some description of the edits the reviewers requested and how we responded to them.

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 have revised the discussion to be more specific. Please review the new section on the surface energy balance and firn modeling (section 3.4 and 4.4). We have also extended the discussion to include this new information. We have also incorporated historical firn temperature data in order to better articulate the thermodynamic processes that have enabled the formation of the firn aquifer. We have also included an ERA climate reanalysis of the region to enhance this discussion (section 3.4 and 4.4).

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 have strengthened our explanation of how we retrieved thinning rates and discussed the evidence when arguing for the main conclusion. To determine how densities have changed over time we have made 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 as well as data from a 2006 field campaign at the same site. Please refer to section 3.3, 4.3, and 5.1 for this analysis and discussion.

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

We have explained 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 have revised 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 this has been added to the uncertainty analysis in the manuscript. Please refer to revised sections 3.1 and 3.2.

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 has been be added to section 2, 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. [Lines 72-79]

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?

We have addressed this by adding the sentence: "Outliers were removed for the background firn density calculations if they were not physically possible (e.g., values

>917 kg/m^3 or <300 kg/m^3 at depths greater than 4 m). The three outliers from 32-36 m depth may have had residual liquid water in them, thus causing a higher density." [Lines 115 -117]

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?

To address this we have added "The Core 2 samples could not be measured for density in the field due to lack of time, so were flown to Kluane Lake Research Station frozen, where the measurements were made within 24 hours of arrival. A random assortment of 125 out of the 196 Core 2 sample bags were damaged during this transport, so were not included in the measurements. This left 71 samples available to use for the density analysis, with at least one sample available per meter except for between 13.29 and 14.95 m. Due to these missing values, only bulk density values are presented for Core 2." [Lines 119-124]

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

We have rephrased this 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 126-171]

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 was 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).

We added the uncertainty of ± 35 kg/m^3 on line 184.

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 have incorporated historical data into the revised manuscript from the original Icefield Ranges Research Reports (as mentioned above), particularly from the 1964 density profiles of Grew and Mellor (1966). We have also included additional data from 2006, also from the Divide site. These locations are 12 km apart but are at a similar elevation, slope, and location in the icefield. Though we cannot say that their processes are exactly the same, they do possess similar stratigraphy and density (in 2006 and in 2018 (Kreutz, unpublished stratigraphy data).

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 has been 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. Please refer to Section 3.3, 4.3, and the discussion for this additional analysis and interpretation.

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 2004-2011 of snow accumulation and density data from the Divide site (12 km from our drill site, similar elevation) on the upper Kaskawulsh Glacier. We have included this data in our estimate of annual accumulation rates in order to provide more supporting evidence for the accumulation rate chosen here. We have also included the historical accumulation data present in the IRRP to aid in our snow accumulation estimate. With these additional data, the snow accumulation estimate is 1.8 m w.e. yr^{-1}. Please refer to section 3.3 and lines 321-329 for this additional information.

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) our own winter accumulation measurements from spring 2018, ii) accumulation data from 2004-2011 and (iii) the (much) earlier published data from the IRRP. This was 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. Please refer to lines 331-34.

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 have double-checked our calculations. 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 σ = 75 kg m^{-3}. This gives s_e = 4 kg m^{-3}. We have 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 have strengthened 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 accumulation rates from the 1960s appear to still be valid today. These rates are similar to the ones measured by Copland and others from 2004-2011, which have been included in the new manuscript (lines 280-301). Though accumulation may have changed since 2011 our measurement of annual accumulation (1.8 m w.e.) is consistent with the variability in the 2004-2011 and IRRP data. 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 have included a new analysis of density changes over time in the revised manuscript. Please see the revised discussion section "4.2 Changes in the upper Kaskawulsh Glacier firn" (line 393) for this analysis and interpretation.

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 have revised the manuscript such that the units are consistent throughout the text, thank you for pointing out this inconsistency. See lines 18, 501, 574. We recalculated the surface lowering as well and found it to be 0.73 m. We have elaborated on how this was calculated and 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 have addressed 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. This historical data allows us to compare the density of the firn in 1964, 2006, and 2018. It is apparent that the density has increased and that there has been an increase in meltwater percolation and refreezing. This is verified by a new climate reanalysis melt model (New Section 3.4, 4.4) that indicates that due to warming temperatures more melt has occurred, thus causing more percolation and refreezing. This occurs to a limit – once the firn is too warm for refreezing the meltwater is likely percolating down and forming a firn aquifer. This too is a new feature as indicated by an additional analysis of snow and firn temperatures from 1964 and 2006 (lines 421-426).

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 is now discussed in lines 321-334. We elaborate the different accumulation rates and how they have not varied significantly since the 1960s.

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 have reported this in the updated manuscript, and provided supporting information. This supporting information is a climate reanalysis model, a firn mode, and historical data.

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.

Anonymous Referee #2

Review "Meltwater Storage in the firn of Kaskawulsh Glacier, Yukon Territory, Canada" by N. Ochwat.

The authors study the density profile of two firn cores drilled in spring 2018 in the accumulation zone of Kaskawulsh Glacier (Yukon, Canada). These cores are used to calculate local firn density and the impact of meltwater retention and refreezing on surface lowering that must be accounted for to correct geodetic mass balance estimates. The authors obtain an average firn density of 670 ± 2 kg m^{-3} in the 36 m deep core, and estimate an average surface lowering of 10 ± 0.8 cm per year over the period 2005-2018. The authors also identify a perennial firn aquifer below ~35 m depth. The paper suffers from major issues including the robustness of the methodology, results and uncertainty estimates, making the conclusions difficult to trust. In addition, some terms used are unclear; the authors sometimes expect a priori knowledge from the readers (e.g. Section 3.3). The reviewer also noted that results reported in the main text and tables are often not matching, and that the conclusions lack of novelty. The paper is mostly descriptive and does not provide novel insight on geodetic mass balance uncertainties compared to previous studies. Therefore, the reviewer deems that the manuscript should be rejected in its current form. Below, the authors can find the reviewer's major concerns, listed as General and Point comments.

There is quite a bit to unpack here, and on some points we provide more detail with the specific responses below.

We have revised the manuscript to clarify the aims, methodology, results, and uncertainties. We apologize for confusion in the values reported – we were inconsistent and unclear in some places, and this was fixed. We have provided more supporting evidence indicating how the density of the firn has changed over time, using a range of studies completed since the 1960s, primarily from the Icefield Ranges Research Reports, as well as from previously unpublished field data collected by colleagues. We have removed the isotope section entirely as it was not fitting in our new manuscript. We have included more details on the uncertainty analysis and in the discussion of the various density values being reported in order to make the relationship between the table and the text more clear.

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 has been expanded and restructured. We have reiterated and clarified 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.

General comments

1. Results are based on "subjective" approximations that may alter the conclusions. For instance, the completeness of the two firn cores section is assessed based on "visual inspection" by three persons. How do the resulting "random" and "human" errors impact the firn density calculated in Eq. 1? In L120, the authors provide a 10-20% uncertainty in estimating the factor f in Eq. 1 (L125-126)? This would lead to a ~100 kg m^{-3} uncertainty in firn density (assuming the 670 kg m^{-3} value reported here), in line with 110 kg m^{-3} estimated in Foy et al. (2011; see L287). However, the authors report uncertainties ranging from 2 to 6 kg m^{-3}. Please elaborate. See also Point comment in L137-140.

We have double-checked our calculations. We use standard error analysis in these calculations, and have expanded on this method in sections 3.1 and 3.2. To summarize here, our point samples (10-cm core sections) have significant measurement uncertainty, as the reviewer notes. There are numerous sources of measurements error, including the subjective assessment of the completeness of each core section. We will refer to these various sources of uncertainty as "random error" in the revised manuscript, as they are all believed to be random (vs. systematic) – sometimes we will measure or estimate a section as being too long or complete, sometimes we will underestimate it. The uncertainty factor of ~15% applies to point samples (10-cm core sections): say, for instance, 500 ± 75 kg m^{-3} for a given sample. Conceptually, for the case of random errors, averaging of the 10-cm density values for the whole core leads to marked reductions in uncertainty because random errors cancel out. Based on standard error analysis, the standard error in the average follows $s_e = \sigma/\sqrt{N}$, where σ is the uncertainty in each data point and N is the number of samples. Taking the example of a 30-m core with 300 10-cm density values, $N = 300$. Taking $\sigma = 75$ kg m^{-3} as an example, $s_e = 4$ kg m^{-3}. We have gone through our uncertainty calculations again to ensure that these are accurate, but the main point here is the difference between sample uncertainties and the standard error in the mean.

2. Across the manuscript, the authors report results that are not matching between the main text and tables, making the conclusions hard to trust. For instance, in L18 the authors report an average surface lowering of 10 ± 0.8 cm yr^{-1} between 2005-2018. In L356, the authors report 10 ± 8 cm yr-1 for the same period. In L322, this annual rate is cumulated over the period 2005-2018 to obtain 1.3 ± 0.8 m in ~13 years. What uncertainty was used here (0.8 or 8 cm)? Please elaborate. Similar issues can be found across the whole manuscript and are listed in the Point comments.

We have corrected the typos noted in the text, thank you for pointing those out. We have edited Table 1 and the manuscript in order to report consistent values and depths throughout the text. We report the upper 10 m of firn density to allow comparability to other literature on firn density. We chose to compare the partial core densities because the length of Core 1 and Core 2 differed – this permits a direct comparison.

3. The 13-year period (2005-2008) is estimated using calculated total water content of 23.22 m w.e. at the drilling site and assuming an average accumulation rate of 1.76 m w.e. yr^{-1} (1960s). The authors do not assess the robustness of this estimate given the uncertainty in firn density. Please elaborate.

We have addressed this in a new section, Section 4.3.

4. The term "melt-affected firn" is often used in the manuscript but not explained. Is this firn affected by the presence of refrozen meltwater in pore space? What are the associated visual features as stated in L204-205? Perhaps a photo of the cores would help the interpretation. The same holds for "Ice content" in L134, that is sometimes defined as the cumulative thickness of ice layers in the core expressed in m, or as a fraction after being normalized by the length of the firn core (see e.g. L192 and Table 1).

A definition of "melt-affected firn" has been added to increase clarity in the methods section. "Melt-affected firn" is any firn that displays physical characteristics indicating that there was the presence of liquid water at some point. This can result in ice layers, ice lenses, or can be indicated by the lack of grain boundaries, the presence of air bubbles, texture, and opacity. "Melt-affected firn" can also be identified using stable isotopes and the cation/anions, however, this was not done in the field. [Lines 102-104]

The use of the term "ice content" is used with more precise wording in the text in order to clarify as to whether or not it is describing cumulative ice layers or the normalized fraction. We refer to the former as the "total ice content" and the latter as "ice fraction". This has been changed in table 1, lines 176, 184, 227, 266, 267, and 364.

5. The authors sometimes expect "a priori" knowledge from the reader. Section 3.3 on stable isotopes is a good example: how to interpret the summer peaks at -22‰ in Fig. 4? This section is not necessary and the results are not further discussed in the text, except in L244-246 that relates low ion concentrations to active meltwater percolation/motion in firn.

We have removed the section on stable isotopes.

6. The conclusions lack of novelty compared to previous studies that also estimated surface lowering in the region (see L334-339). The paper does not provide a convincing estimate of (local) surface lowering uncertainty for geodetic mass balance measurements, nor estimate the regional mass change accounting for density correction. In L328-330, the authors claim that density estimated at the two cores are representative of a larger region, which cannot be proved using only two cores as stated in L371-376. The authors should consider combining their core measurements with firn modeling to obtain spatially continuous density profiles and

estimate regional mass balance uncertainty due to firn processes.

There are several good points here, mostly related to points addressed above. Within our objective to characterize the firn density, densification rates, and meltwater retention on the upper Kaskawulsh Glacier, we did not initially set out to quantify changes in densification rates over time – only the annual densification associated with meltwater refreezing, and the resulting firn density profile. We have reorganized the manuscript and incorporated new data, historical data from 1964 as well as data from 2006. We have used this data to discuss the changes in density over time and densification rate. We have also included a surface energy balance model and firn model. Additionally, we have expanded on our methodology for the uncertainty and discussed it in greater detail.

It is difficult to know whether our two cores are representative of the larger accumulation area of this icefield or others in the St. Elias region, but they still provide information where little other recent work on firn properties has been undertaken. We have revised our discussion and not over-extended our claims in the revised manuscript.

Point comments L92-94: Are the measurements from the snow pit discussed somewhere in the manuscript or shown in Fig. 2? Please clarify.

The snowpit measurements are presented in Figure 2 and are included in the density data and inference about annual snow accumulation. We edited the caption of the figure in order to clarify that the measurements are part of the density figure. "The first meter of data is from the snowpit." Is added to figure caption 2.

L135-136: What does "melt percent" mean? How is this calculated?

We have explained this more clearly in the manuscript. "Melt percent" has been used in the literature (e.g., Koerner, 1977) to refer to the percent of annual snow accumulation that melts (and refreezes), in the accumulation area of polar environments. At our site we don't use this concept but use "ice content" to refer to the fraction of a core sample that is made up of refrozen meltwater.

Please see lines 164-171 for this elaboration.

L137-140: This is unclear, why should the thickness of ice lenses be divided by a factor two?

Ice lenses were partial ice layers, where the ice did not extend horizontally through the whole core section. We assume that, on average, the ice lens occupied 50% of the core; therefore the measured thickness was divided by two. We have added "In core samples that had ice lenses, ice lens diameter, on average, occupied 50% of the core sample; therefore their thickness was divided by two before being summed." [Lines 168-171]

L161-164: The authors should provide some references on the methods used to study isotopes.

We have removed the section on isotopes.

L184-190: This paragraph includes numerous errors in reporting results. In L186, "571 ± 3 kg m^{-3}" is reported in the text while Table 1 lists 518 kg m^{-3} at core 2 between 4-14 m depth. In L187, "608 ± 2 kg m^{-3}" is reported while Table 1 lists 618 kg m^{-3} between 4-21 m depth. The authors report an extremely small density uncertainty of 2-3 kg m^{-3} while Figs. 2a and b show much larger uncertainties. In L229- 230, the authors state that densities larger than 917 kg m^{-3} are eliminated. However, Fig. 2a shows values of ~1000 kg m^{-3} or larger at 6 and 10 m depth. To the reviewer, it is hard to judge whether these errors are due to negligence or calculation errors. Please elaborate.

The uncertainties of point samples versus average values for the cores are discussed above (please see the response to point #1). A more detailed explanation of the error analysis has been included in the revised paper (Line 128-178).

Figure 2 shows the point data (10-cm sections). The outliers have been removed from Figure 2 in order to be clearer, because they were removed from the calculations to determine density and background firn density.

Line 187 referred to the average of core 1 and core 2 of the density of firn at both locations – this was not reported in the table. A row has been added for the average to help clarify the confusion. This is reported in both sentences in line 187 and 372.

L185, 187, 188: For clarity, the authors should better write: "between 4 and 14 m depth" instead of "in the upper 10 m"; "between 4-21 m depth" instead of "in the upper 17 m"; and "between 4-36 m depth" instead of "representing ~32 m". The same holds for L284-286.

We have edited the manuscript throughout in order to be clearer as to the depths we are referring to when discussing the firn. We have also reworded it in the way you suggested.

L193: 660 kg m^{-3} is actually 1.5% smaller than the firn density of 670 kg m^{-3} reported in L189.

We have significantly altered the manuscript and no longer have this sentence in it.

L276: What do the authors mean by "summer melt extent"? Do they mean meltwater production in mm w.e. yr^{-1} as listed in Table 2? Please clarify.

This sentence will be reworded so that instead of saying "summer melt extent", it will be clarified as meltwater production in m w.e./yr, as listed in Table 2 (which is now table 1).

We also refer to the "last summer surface" and clarify what that means in the text.

L278: It is hard to assess the robustness of the results in this paragraph. In L278, the authors state that summer 2015 was the warmest in the period 2014-2018, whereas Table 2 shows that it was actually summer 2016 (-1.0ºC in 2016 vs. -1.8ºC in 2015). The same goes for annual mean temperature in 2015-2016 (-9.0ºC in 2016 vs. -9.6ºC in 2015). How to interpret the larger PDD and melt rates in 2015 then? Please clarify.

This has been removed entirely and replaced with a surface energy balance model please refer to sections 3.4 and 4.4.

L284: Again 608 kg m^{-3} is reported in the text whereas 618 kg m^{-3} is listed in Table 1.

All of the density measurements have been recalculated.

~~**L315: What do the authors mean by "certain amount"? Ice layer thickness?**~~

We have addressed this by adding the following lines [440-450]:

"Research in Greenland proposes that ice-layer formation and the presence of firn aquifers may delay surface run-off due to the water storage characteristics of firn (eg. pore space availability, water at interstitial grain boundaries, etc) (Fountain and Walder, 1989; Scheider, 1999). If ice layers become too extensive or thick, they can form an 'ice slab,' a thick impermeable barrier that leads to enhanced surface runoff (MacFerrin et al., 2019). The thickness of ice layers that prevents percolation varies and depends on the local climate and conditions of the firn. For example, in Greenland 12-cm thick ice layers were still permeable (Samimi et al., 2020) whereas Bell et al., (2008) reports a 1-2 cm ice layer prevented percolation at the Devon Ice Cap, Canada."

Table 3: What does "1.5-2g" mean in the personal communication of Sass and O'Neel?

Thank you for noticing the "g" that should not have been there and was removed.