Dear reviewer,

Thank you for the thorough and supportive comments. The suggestion to contextualize the study in terms of the seasonal cycle was particularly fruitful (and was shared by all reviewers). To address this, we have reported the seasonal and annual ablation recorded by a nearby automatic weather station, and added a comparison with regional meteorology reported in a recent publication that is highly relevant to our study (Tedstone et al., 2017). These data reveal that summer 2016 was characterized by conditions particularly favorable to weathering crust development, and therefore should help contextualize our findings. I provide a line by line reply to each comment below, and include revised figures at the end of the document. A revised manuscript will be uploaded following receipt of instructions from the handling editor. Thank you kindly,

Matthew Cooper

Anonymous Referee #2

Received and published: 29 August 2017

This paper presents findings from a field campaign on the Western margin of the Greenland ice sheet concerning the nature of the 'weathering crust' on the bare ice in the ablation zone. The paper provides measurements from shallow ice cores of ice density and corresponding porosity as well as water content, finding surprisingly low density ice down to at least 1 meter depth. It is pointed out that the presence of this weathering crust means that (subsurface) melt does not necessarily correspond to 'surface' lowering, as might be measured by satellite altimetry.

I think this is an interesting set of measurements, and is a valuable contribution to understanding the supraglacial hydrology of the Greenland ice sheet. Meltwater storage in the percolation zone of the Greenland ice sheet (in firn) has been well documented over the last few years, and this study suggests that a non-negligible amount of water storage and transport may occur beneath the apparent ice surface in the ablation zone too.

The paper is well written and the figures are mostly clear. I have one main comment and a number of minor comments, mostly seeking clarification.

Main comment

The measurements represent a snap-shot of the weathering crust in mid-July 2016 and it is not clear how this relates to the behaviour over the course of the melt season. I appreciate that the field campaign was limited in length so it may not be known how the crust itself evolves, but I think there needs to be more discussion of the setting for these measurements. In particular, what is the annual ablation rate in this region? At what stage of the melt season are these taken (i.e. roughly how much melting has already occurred here)? What is the ice temperature in this region? These are important issues in understanding how reflective these results are of wider spatial scales but also larger time-scales.

Author response: We agree and thank the reviewer for recognizing this important weakness in our original submission. As requested, we obtained daily measurements of ablation recorded by the KAN-M automatic weather station (AWS) during our field study. KAN-M is located ~8.3 km ENE of our field site at ~1270 m a.s.l. and is the most proximal AWS to our field site (1215 m a.s.l.). Sonic ranging data recorded by KAN-M indicate the maximum spring snow depth was ~50 cm and the snow disappearance date was ~21 June, which suggests the conditions we document developed over an ~21-day period between snow disappearance and the collection of the ice cores on 11-12 July. Following snow disappearance, AWS data indicate a cumulative ice surface lowering of ~55 cm prior to collection of the shallow ice cores on 11-12 July. The average ablation rate during this time was 2.65 cm d⁻¹. The mean annual ablation rate at KAN-M is ~1.25 m a⁻¹ (van As et al., 2017). These statistics are reported in the revised Sect 3.4 paragraphs 3-4.

To supplement these data we added a comparison with a recent publication that examines the relationship between regional meteorology and remotely sensed surface reflectance in the study region (Tedstone et al., 2017). Their analysis of regional meteorology from the Modèle Atmosphérique Régional (MAR) regional climate model (Fettweis et al., 2017) suggests ~50 cm average snow depth and mid-June snow disappearance date in summer 2016 (see Figure 3 in Tedstone et al., 2017), consistent with the AWS data we analyze. Further, their analysis suggests that meteorological conditions during summer 2016 were ideal conditions for weathering crust development. These include below average cloud cover and rainfall, and above average downward shortwave radiation (e.g. compare to Figures 1–4 in Tedstone et al., 2017). From these data and the AWS data, we conclude it is not surprising that a well developed weathering crust was present in the study area at the time of observation. We have added several discussion points throughout the manuscript to emphasize the seasonal context.

Main comment (continued)

In particular, I think there could be more discussion of how the inferred stored water thickness (15-22cm) relates to the amount of melt that has so far been produced this season (roughly what fraction of it is this), and how the depth of the porous ice compares with the amount that melts each year. Eg. are the ice lenses that form the result of recent refreezing (i.e. earlier the same year) or some earlier time?

Author response: As noted above, KAN-M data indicate ~55 cm of ice surface ablation prior to collection of the shallow ice cores on 11-12 July, equivalent to 49.5 cm water equivalent assuming solid ice density of ~900 kg m⁻³. The inferred stored water thickness (revised to 13-21 cm) is therefore ~26–42% of the cumulative seasonal melt.

The mean annual ablation rate is ~ 1.25 m a⁻¹ at KAN-M (van As et al., 2017). The shallow cores were limited to less than <1.1 m depth, though we observed weathered ice at depths >1.6 m, suggesting the weathering crust depth at the time of observation was greater than the mean annual ablation rate.

Regarding ice lenses, we have substantially revised and (we hope) improved our discussion of their nature. We do not think they are refrozen meltwater. For the crust to develop permeability, the ice must be temperate, which should prevent substantial refreezing (Schuster, 2001). Diurnal

refreezing could occur, but if so it seems unlikely that weathering would proceed at depths beneath refrozen ice lenses without also weathering the lenses, thus preventing a progressive stratigraphy from developing. The situation is different in firn, where the medium in inherently permeable, allowing meltwater to penetrate along preferential flow paths forming refrozen lens horizons. The revised discussion now reads:

"Previous analyses of weathering crusts have not reported this pattern of alternating clear, solid ice and fractured, granular ice (e.g. Hoffman et al., 2014; Müller and Keeler, 1969; Schuster, 2001). It seems likely these lenses are structural features, as refrozen meltwater in unlikely in a thermally temperate weathering crust (Schuster, 2001), and even less so weathered ice at depths below refrozen meltwater. Stratified distribution of grain size, crystal structure, bubbles, and/or impurities with depth (Hudleston, 2015) could each influence the rate of subsurface radiative heating and hence weathering (Brandt and Warren, 1993; Liston et al., 1999). Meltwater advection along micro seams or cracks may also promote differential weathering (Hambrey, 1977; Hambrey and Lawson, 2000), similar to joint block weathering of terrestrial lithology. Surface expression of differential weathering is certainly evident along the transect (Figure 1), and at broader scales in the region is associated with outcropping of impurities (Wientjes et al., 2012). The ice lenses, then, may represent structural resistance to weathering, and/or result from heterogeneity in subsurface flow paths that enhance differential "rotting" of subsurface ice (Nye, 1991). We would thus expect lenses to be localized features, which helps explain the lack of consistent stratigraphy among cores."

Main comment (continued)

There could then be some discussion of how the porous ice evolves over the course of the year. Presumably all the water freezes again in winter? In which case the ice is mostly solid at the start of the melt season (except perhaps the unsaturated surface layer, which seems to have a similar porosity to snow)? If the saturated subsurface water doesn't ever run-off, it may simply be changing the quantity of runoff rather than delaying it.

Author response: We interpret the literature to suggest the weathering crust is a seasonal phenomenon with no interannual carryover except in the case of stagnant ice (e.g. Fountain and Walder, 1998). On sub-seasonal timescales, the weathering crust can rapidly decay when the surface energy balance is dominated by longwave or turbulent heat fluxes that melt the surface, removing the crust and exposing solid ice. Common examples would include heavy rain, very warm winds, or warm cloudy conditions. However, it is conceivable that a deep weathering crust could persist to the end of the melt season if meteorological conditions allow. In this case, we would expect interstitial and surficial meltwater to refreeze following snowfall. Annual snowfall in this part of Greenland is typically <1.0 m (Tedstone et al. 2017) and therefore meltwater that does not drain from the crust likely refreezes during winter and/or at night. If snow cover is absent or ephemeral, the crust may sublimate over winter. However, interannual variability in these conditions is substantial. Though we agree the annual progression of the porous ice is a logical next step for research on the topic, we prefer not to comment on interannual carryover without a physical model to support the analysis. That said, to touch on this topic we note in the revised that the mean annual ablation rate is ~1.25 m a⁻¹ at KAN-M. Given that we document weathered ice at depths >1.5 m it is conceivable that near-surface, low density ice persists on interannual timescales.

Specific comments

Why are the findings frequently referred to as 'preliminary'? What are they preliminary to? If they are really preliminary, it begs the question why they are being published. I'd suggest that if the authors think the results are worth publishing they should not refer to them as preliminary (which does not preclude doing more work on the topic).

Author response: We appreciate this point and have removed our use of 'preliminary' throughout the text. Our original intent was to highlight the relatively immature status of the research topic in general, and more so its application in Greenland, not our specific findings.

Page 1

Line 29: Why does the routing of surface water to the ocean 'reinforce concerns' about contribution to global sea level rise? Isn't such melting part of the 'normal' operating cycle of an ice sheet?

Author response: We have removed the statement, as requested. Our original intent was to highlight the "efficient drainage" hypothesis, which is the assumption that ablation zone meltwater is transported rapidly, in its entirety, to surrounding oceans. To develop this idea more clearly, the revised introduction references works that demonstrate substantial time lags and possible meltwater retention in the ablation zone as motivation for the study of ablation zone hydrologic processes and near-surface porous ice

The revised introduction reads as follows:

"Each summer a vast hydrologic network of lakes and rivers forms on the surface of the southwest Greenland Ice Sheet ablation zone in response to surface melting (Chu, 2014; Smith et al., 2015). Evidence suggests that most or all of this water is efficiently delivered via supraglacial rivers to moulins, crevasses, and, ultimately, to proglacial rivers and surrounding oceans (van As et al., 2017a; Lindbäck et al., 2015; Rennermalm et al., 2013; Smith et al., 2015). The assumption of efficient meltwater delivery is reflected in regional climate and surface mass balance models of Greenland that instantaneously credit ablation zone surface runoff to the ocean with no physical representation of hydrologic processes or meltwater runoff retention taking place on the ablation zone bare ice surface (Smith et al., 2015). On daily to monthly timescales, however, field studies and satellite remote sensing have found evidence of substantial meltwater runoff delays in the Greenland Ice Sheet ablation zone (van As et al., 2017a; Karlstrom and Yang, 2016; Koenig et al., 2015; Lindbäck et al., 2015; Overeem et al., 2015; Rennermalm et al., 2013; Smith et al., 2015). Similar runoff delays are observed in supraglacial environments elsewhere (Karlstrom et al., 2014; Munro, 1990), owing to the presence of a degraded, porous "weathering crust" (Müller and Keeler, 1969) on the bare ice surface of glaciers and ice sheets that stores meltwater, delaying its delivery to supraglacial channels via porous subsurface flow (Irvine-Fynn et al., 2011; Karlstrom et al., 2014; Munro, 2011). The porous weathering crust may also provide a substrate for internal and/or surficial refreezing of meltwater (Hoffman et al., 2014; Paterson, 1972; Willis et al., 2002), similar to meltwater

transport, storage, and refreezing in snow and firn (Cox et al., 2015; Forster et al., 2014; Harper et al., 2012; Machguth et al., 2016). The presence of weathering crust in the Greenland Ice Sheet bare ice ablation zone, however, has gone largely undocumented, and little is known about the effect of weathering crust meltwater storage on hydrologic efficiency in the bare ice ablation zone, where >85% of Greenland ice sheet meltwater runoff is generated (Machguth et al., 2016)."

Section 2.1: It is not clear from this description how liquid water in the core is dealt with. Is it allowed to drain out? Presumably there is still quite a lot of water trapped in the core samples (due to capillary forces) and this contributes to the measured mass?

Author response: Thank you for this important observation. We did not explain this adequately in the manuscript. The drill barrel was held vertically and allowed to drain when cores were removed from the boreholes prior to weighing. After removal from the borehole, the drill was laid at a slight angle and the core was carefully removed from the drill barrel and immediately analyzed, providing additional time for drainage. Though our aim was to drain the cores completely, it is correct that some water remained owing to capillary forces. It is also possible that some non-capillary water remained owing to incomplete free-drainage. These water retention errors would result in overestimated ice density.

In adding a more thorough discussion of this issue to the methods section, we also provide more detail about the measurement uncertainty noted in the original manuscript. Namely, the natural breaks of the ice cores were irregular and some material was inevitably lost near the ends of the core segments. The 10% error estimate we provided in the original manuscript was meant to account primarily for this loss of material at the irregular ends of the ice core segments, which would tend to result in underestimated ice density.

To summarize, there are two primary sources of error we expect are important 1) ice core volume measurement error owing to loss of material near the irregular ends of the individual ice core segments, and 2) interstitial meltwater retention errors owing to capillary water retention and incomplete free water drainage. The volumetric error would tend to result in underestimated ice density, the water retention in overestimated density. Hence, the two would tend to cancel, though to an unknown extent as both errors are poorly constrained.

In the revised methods, we describe these error sources in greater detail, and we cite estimates of temperate ice water content ranging from 0-9%, though most estimates (15 of 18) are <3.4%, including all estimates made from in situ calorimetric methods (Pettersson et al., 2004). The uppermost 9% estimate is thus well within our $\pm 20\%$ specific storage uncertainty estimate. We think this is sufficiently conservative without giving undue confidence to either the measurements or the error estimate.

It may also be worth noting this same issue is present for studies of firn density. For such studies, a physical model can be used to establish a theoretical dry-firn density that can be compared with in situ measured density to estimate liquid and/or refrozen water content. While subsurface weathering crust density in Antarctica has been modeled (Hoffman et al., 2014), such an exercise is well beyond the scope of this paper.

Finally, we corrected the error estimate by adding a physical constraint that density cannot exceed solid ice density (917 kg m⁻³) and effective porosity cannot exceed total porosity (1 – ρ_m/ρ_{ice}). These issues are presented in the revised methods Sect. 2.1 paragraph 3.

Page 4

Line 9: This is a bit awkward wording, since this statement presumably assumes that the density is uniform (independent of depth). Perhaps better just to say that the geometry of the sampler means that the near-surface ice is disproportionately weighted in this average, rather than quantifying the 'center of mass'.

Author response: As requested, we have removed the 'center of mass' statistic and replaced with the following brief description " \dots the density measurements may be more representative of the uppermost ~6 cm of material because of the shape of the sampler \dots "

Section 2.3: There is some confusion here about the 'unsaturated weathering crust depth', and how it relates to how penetrable the ice is. Reading further, it seems that the 'water table' (which I would interpret as the unsaturated depth) roughly coincides with a change in the strength of the ice that is presumably what the depth probe is detecting. It does not seem obvious to me why these two surfaces (the impenetrable ice surface and the water table) should happen to coincide - perhaps the presence of air in the pores above this allows the surface ice to 'rot' more rapidly. Or perhaps the permeability of the upper layer is sufficiently large that water in this layer readily runs off horizontally keeping it unsaturated. Perhaps the qualitative description of the surface given on page 9, line 20, could be moved forward to the method section to help explain these issues. In any case it would help to be clearer precisely what is meant by unsaturated - does this mean there is no liquid water, only residually-trapped water, or that water does not fill the pore space (all of which are different)?

Author response: We apologize for the confusing presentation of the steel rod measurements and the unsaturated depth in the original manuscript. In the revised, we have removed the a priori characterization of the saturated/unsaturated transition in the methods and instead report the transition in the results, where it belongs. The <u>unsaturated depth is estimated from the depth to</u> water below the ice surface in cryoconite holes, whereas the depth probe measurements are used as a qualitative characterization of the weathering crust structure.

To contextualize the depth probe measurements and the weathering crust structure for the reader, we are working with a graphics specialist in our department to create a diagram for the introduction that merges the weathering crust conceptual diagram of Müller and Keeler, (1969) with the characteristic depth-density decay curve from LaChapelle, (1959). A crude representation is shown below for reference as the diagram is in production. Near surface weathered ice tends to exhibit a characteristic increase in density from a very low-density surface layer to a higher density subsurface that approaches solid ice density. The very low density surface layer is demonstrated by the coarse material above the water table in the conceptual diagram of Müller and Keeler, (1969) (Figure 1, left). This is the material the depth probe penetrates, which we suggest may be indicative of the "shoulder" on the density decay curve where density increase non-linearly (Figure 1, right). We hope the diagram will clarify the depth-variable nature of the crust for the reader and provide context for the depth probe measurements.



Figure 1: Conceptual schematic of the weathering crust (Müller and Keeler, 1969) and the ideal subsurface ice density curve for the near-surface of an ablating glacier, adapted from La Chapelle (1959).

Page 6

Line 5: Why is refrozen meltwater included as water storage? If it has refrozen it is ice again and should be thought of as storage (it requires melting again - with the associated energy implications - before it could run off).

Author response: We have removed refrozen meltwater from the definition of storage, as requested.

Page 6

Line 19: Is the 'potential' liquid storage capacity not just the effective porosity multiplied by depth and total area (i.e. including the currently unsaturated pore space too)?

Author response: Our use of the word "potential" was incorrect and misleading. Your characterization is correct but we present the actual (instantaneous) specific storage estimated from the shallow cores, and then scale that to the study catchment by multiplying the average storage depth by the total area to estimate a storage volume.

L19 has been revised to read "Finally, for illustrative purposes we scale our storage estimate to the study catchment by multiplying the mean S_P estimated from the shallow ice cores by the bare ice surface area of the study catchment ...".

Page 8

Line 7: There seems to be some subjectivity involved here. Why is estimating the value wrong in one direction deemed 'not problematic'? If the densities were measured including the ice lenses, would it not make sense to use the volume including the ice lenses when converting to water content using the effective porosity? Or otherwise use a solid ice density of the ice lens to infer the density of the non-ice lens part of each segment?

Author response: Thank you for this important critique. Upon consideration, we agree it makes sense to use the volume including the ice lenses when converting to water content since they are included in the density. This was an oversight on our part. We have removed the various references to the ice lens density bias, which we think will remove unnecessary confusion for the reader. Moreover, given our interpretation of the lenses as structural features, it makes sense to include their volume in the storage estimate.

Page 10

Line 4: The drill did not go below 1.8m for fear of freezing. Did you make any measurements of temperature in the porous crust? It would be helpful to know if the ice is all at the bulk melting temperature or if it goes below this at depth.

Author response: We absolutely agree that temperature measurements would be invaluable. The reason we note the "risk of freezing" is to suggest, albeit indirectly, that the ice may be sub-freezing at this depth. Unfortunately, measuring subsurface ice temperature is very difficult and error prone unless done with considerable care, and we were not able to undertake such measurements. The best we can do is to suggest the ice may have been freezing based on our observations of the ice core drill seizing up. Co-author Miege has extensive experience drilling firn cores and he suggested based on his observations of the drill behavior at these depths that the ice was freezing.

Line 20: What is meant by a storage 'rate'? I could not work out what this number means.

Author response: This should have been referred to as 'specific storage rate' and is the specific storage (i.e. storage depth) divided by the time over which the meltwater storage accumulated. In either case, we have removed the comparison with these rates because they were estimated using water budgets and reviewer 3 objected to the comparison with our core-density method.

Page 11

Line 15: The 'lower and upper mean' is a strange concept; perhaps the 'mean lower and upper values' would be better wording.

Author response: As requested (with slight modification), we have replaced 'lower and upper mean Sp' with 'lower and upper estimates of Sp'.

Line 21 (and conclusions): Why are the results not considered representative of the rest of the ablation zone? I understand the desire for caution given that this is only one location, but without other evidence (perhaps you have it?) wouldn't the default assumption be that the results do apply more widely? What do you think is special about your field site that means the results would not apply more widely? Perhaps you could just say 'We do not know whether these findings represent typical conditions....' rather than 'not proposing' it.

Author response: We do not have any evidence that this location was unique. However, weathering crust growth and decay is strongly controlled by local meteorology and therefore can be highly variable over short distance and time. To our knowledge, there are no studies of seasonal weathering crust formation in Greenland, but subsurface melting in the study region has

been modeled and shown to depend on snow cover, which varies with elevation (van den Broeke et al., 2008). Lacking spatial data, we were trying to be cautious, but upon consideration we agree there is no need to over emphasize this speculative (albeit cautionary) assumption.

As requested, we have removed the statement 'we do not propose' and replaced with 'may not be representative'.

Updated figures/table:



Figure 3: Subsurface measured ice density (ρ_{M}) and corresponding calculated effective porosity (ϕ_{eff}), and stratigraphy profiles from 10 shallow ice cores (#10-1, left to right) extracted at 80 m postings along the study transect (see Figure 1 for ice core locations). Horizontal blue shading represents solid ice layers. Vertical dashed line at solid ice density 917 kg m⁻³. Assumed ±10% measurement uncertainty represented by shaded grey bars. Hatched areas are no data.



Fig. 5: Linear relationship ($\hat{\phi}_{eff}$, solid line) between measured ice density (ρ_M) and effective porosity (ϕ_{eff}) and assumed ±10% measurement error (whiskers). Dashed line is theoretical upper limit where effective porosity equals total porosity (i.e. $\phi_T = \rho_M / \rho_T$).



Fig. 6: (a) Ice sheet surface topography along the 800 m study transect extracted from a 6 cm posting stereophotogrammetric digital elevation model derived from RGB imagery collected 10 July 2016 from a quad-copter drone and the 2nd-order polynomial best fit. (b) Ice sheet surface topography detrended with the polynomial best fit, crycoconite hole depths (vertical grey bars), and cryoconite hole water levels (vertical blue bars) sampled along the 800 m study transect, adjusted to a common reference. Locations of the 10 shallow boreholes and their depth relative to the detrended surface shown for reference.

Core	lce Core Depth	Mean Core Density	Mean Core Porosity	S _P
	(cm)	(g cm⁻³)	(-)	(cm)
1	100	0.72	0.19	11 – 16
2	100	0.72	0.19	10 - 16
3	100	0.76	0.15	9-14
4	90	0.63	0.28	15 – 22
5	89	0.63	0.27	15 – 21
6	97	0.74	0.17	14 - 26
7	90	0.65	0.26	14 - 30
8	102	0.72	0.19	14 – 27
9	90	0.64	0.26	15 – 22
10	82	0.64	0.27	13 – 25
μ	94	0.69	0.22	13 – 22

Table 1: Shallow ice core depth, mean core density, mean core porosity, and specific storage depth (S_P) , for each shallow ice core.

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