Dear reviewer,

First, thank you for your thorough and tremendously helpful review. It is clear you invested considerable time into this review and your comments and suggestions have greatly improved the quality of the manuscript. We hope we have adequately addressed the various suggestions and concerns raised throughout. 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 #3

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This paper reports on the findings from a field campaign on the ablation zone in southwestern Greenland. The focus of the paper is the so-called "weathering crust" that characterises glacier and ice sheet surfaces, and its potential hydrological storage role. The authors use a set of shallow ice cores (n=10) to describe the variability in near-surface ice density over depths of < 2 m. From these observations, the authors explore an effective porosity of the near-surface ice, and examine a potential water storage based on observations of a water table evident within the weathering crust. A specific storage of ~0.2 m is derived, suggesting that at the time of observations a water volume equivalent to 1 hours' worth of discharge from the local supraglacial catchment was essentially stored within the weathering crust.

The findings are a useful demonstration that this weathering crust exists on the Greenland Ice Sheet, and provides a sensible trigger for future work assessing the supraglacial drainage system and its functionality. Although some recent focus in Greenland has included the firn aquifer at higher elevations, it is an interesting insight to an overlooked hydrology of the ablating bare ice sector of the ice sheet. The growing recognition of the supraglacial realm as an ecosystem, and the potential importance of water storage on biogeochemical cycling at the ice sheet surface ensures this is a timely contribution and serves as a useful benchmark in this type of work.

Overall, the paper is well written, sensibly referenced, and the figures are clear. The methods are intelligible and could be repeated, and the calculations utilised are sensible within the limits of the data available/presented.

However, major comments would include:

A stronger description of how the weathering crust forms, and the subtlety of its growth and decay would be beneficial both in the introduction and in the later discussion. Specifically, would you expect a deep weathering crust at the time of your observations? Does the timing of snow melt, dominance of shortwave radiation, absence of rainfall give reason to consider the weathering crust (and ice lenses) you describe?

Author response: We have added a stronger description of how the weathering crust forms in the introduction, as requested. We draw attention to the depth dependency of radiative heating and the depth-density profile from LaChapelle (1959). We are working with a digital graphics specialist in our department to create a diagram that merges elements of the weathering crust conceptual schematic from Müller and Keeler, (1969) with the depth-density curve that we think will greatly improve the weathering crust description.

To address the temporal context of our findings, 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.

Major comments (continued):

Is this a glacier ice weathering crust or one that perhaps is superimposed ice derived from snow and refrozen lenses forming therein? If this is glacier ice, then you should at least mention ice structure in addition to refreezing processes (particularly given the evidence of marked structure in the locality).

Author response: It was a glacier ice weathering crust, as data from KAN-M and from Tedstone et al. (2017) suggest the maximum snow depth in the region was ~0.5 m and had completely melted by 21 June. We observed a few remnant snow patches in the field. The snow was heavily metamorphosed into uniform spherical grains and was easily distinguished from the pervasive coarse bubbly weathering crust ice. We do, however, think superimposed snowmelt ice is an important part of the annual surface ablation cycle in the study area and probably contributes to the initial formation and structure of the crust, though we do not have data to support this presently and the influence of superimposed snowmelt on our results is likely negligible owing to the ~55 cm of ice surface lowering. Regarding ice structure, we thank you for encouraging us to explore this more thoroughly and we have added considerable discussion of ice structure in the revised results, copied below in response to specific comments.

Major comments (continued):

A clearer emphasis regarding the results being a snapshot which reveals something about the supraglacial hydrology of the Greenland Ice Sheet could be beneficial. In your discussion, albeit subjectively, are you able to comment on the likelihood of greater or less storage to be seen at other times of the summer, is this a seasonally progressive hydrological feature or is the observation just that, a discrete observation – there are climate records for the locality which might allow some extrapolation of these ideas.

Appreciably, it is not possible to go beyond perhaps a statement on this, given the limitations of the data set, but it would be helpful.

Author response: As noted above, we have added a comparison with seasonal climate records from the nearby KAN-M AWS and with Tedstone et al. (2017) which together demonstrate climate conditions prior to the field study were favorable for weathering crust development. Regarding the seasonal progression, we report the annual ablation rate is ~1.25 m a^{-1}, therefore it is conceivable there could be some interannual persistence given we document a >1.5 m deep crust. We note this is dependent on meteorological conditions favorable to crust growth through the end of summer. Additionally, we emphasize in several places, including a new closing paragraph in the discussion, the transient nature of the crust growth and removal. We are hesitant to comment further without field data or a physical model of crust development.

Major comments (continued):

A slightly strengthened assessment of the uncertainties would be important I feel – this should include an assessment of the water content of the cores themselves as it is not clear if all interstitial water was removed prior to evaluating mass for density estimates. Temperate ice can have interstitial water content of up to ~10% (see Petterson et al., 2004, JGR), and certainly the saturated lower-most ice in the developing weathering crust may exhibit such water content if this is a seasonally temperate ice layer. Can you perhaps try to assess uncertainties associated with this water content, and the resultant impact on other estimates presented here. The 10% and 10% quoted seemed a little arbitrary when slightly more detailed and thorough approaches could have been taken. Furthermore, can you account for the ablation of the ice surface if cores were not all taken on one day – can the core profile figure be corrected/adjusted for surface ablation – making crude assumption that ablation over transect broadly similar, or using a point estimate from the energy balance? Correcting for the 7 days ablation period might be informative and aid inferences – such that for example, ice lenses may be better aligned perhaps if representative of refreezing events or local thermal conditions.

Author response: Regarding ablation, the cores were collected over a period of two days (11-12 July, noted in the revised). The ablation rate was ~2.6 mm d^{-1} during the field campaign. As such we have not adjusted the cores to account for this.

Regarding water content, thank you for raising this important point that was not adequately addressed in the initial 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 to 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 as requested, 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). We find no estimate of depth-dependent water content for near-surface <2m deep ice, hence a uniform water content error seems sensible, and the uppermost 9% estimate is well within our ±20% error estimate for specific storage. We think this is sufficiently conservative without giving undue confidence to either the measurements or the error estimate. Perhaps more important is to interpret the error and provide expectations. For example, we think the storage is more likely to have been underestimated owing to interstitial meltwater retention than overestimated owing to poor volumetric measurements, but we don't have a rigorous way to demonstrate this.

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, to improve the error estimate we have added a physical constraint that density cannot exceed solid ice density (917 kg m^{-3}) and effective porosity cannot exceed total porosity ($1 - \rho_m/\rho_{ice}$). These issues are presented in the revised methods Sect. 2.1 paragraph 3.

Major comments (continued):

Could more analysis of the data presented in Figure 6 be made available here? There are opportunities to examine patterns over elevation (small range though that is) and in relation to the detrended surface and 'roughness'. Similarly, it would be interesting to see if there is from the profiles (e.g. what are the patterns of phi-eff at, say, 33cm and 87cm depth, where it looks weathering crust (not ice lens) data is available across all cores – assuming these positions remain if adjusted for ablation over the 7-day sampling) – is there anything to be gained from a slightly deeper examination of the density and porosity data over depth and along the transect?

Author response: Regarding spatial variability along the transect, we tested for statistically significant linear relationships between distance/elevation and 1) depth of cryoconite holes, and 2) depth to water in cryoconite holes. No relationship was found for depth to water though there was a slight trend toward shallower holes (-0.014 cm m^{-1}, $p<0.004$). We report these findings in the revised Sect. 3.3. The same trends were found for elevation owing to the gradual increase in elevation along the transect, but the elevation distribution was strongly skewed toward higher values. The lack of any trend in depth to water with distance confirms that cryoconite water levels generally mirrored the small-scale roughness, which was noted in the original manuscript but was difficult to see in Figure 6. In the revised, the grey filled area has been removed to improve interpretation of Figure 6.

Regarding density and porosity depth-variability, we have reframed portions of our analysis in terms of the theoretical depth-density curve presented in LaChapelle (1959), as suggested. We appreciate this suggestion and we hope it has improved the framing of the depth-density results. As requested further down, we gap-filled the missing data in Figure 3 so the depth-density profiles are complete, which provides a better comparison with the depth-density curve of LaChapelle.

Regarding ice lens stratigraphy, we do not think they are controlled by temporal meteorological variability. As suggested, we think they are structural features possibly controlled by stratification in ice grain size, crystal structure, bubble size and/or content, or impurities, each of which could influence radiative heating. We also speculate that meltwater advection along micro seams or cracks may promote differential weathering, similar to joint block weathering of terrestrial lithology. We hope this general discussion of ice structure provides guidance to the reader, but we were not able to find patterns in density or porosity across cores at specific depths. This may not be surprising. For example, ice lens stratigraphy in firn is highly variable and discontinuous over spatial scales as short as 1.5 m (Brown et al., 2011; Machguth et al., 2016). Each of these would suggest lenses are highly local features, helping to explain the lack of consistent stratigraphy among cores.

The new discussion of the ice lenses is provided further down in response to specific comments.

Major comments (continued):

In places the writing style became less clear, or seemed to have a slightly reduced scientific quality. Similarly, a couple of key references seemed to be absent or choices of references seems a little misplaced, while in other places there was a proliferation of sources when perhaps just one or two examples would suffice. Some further editing and subtle revisions would likely be beneficial, to strengthen this paper, in addition to perhaps examining a few more relevant publications that would be of help in supporting these results and findings and their significance.

Author response: Thank you for these suggestions. We carefully reviewed the citations and found several instances where, we agree, the chosen citations were misplaced, especially in the introduction. To address this, we have carefully (we hope) separated key citations that refer to theoretical ice permeability studies (Lliboutry, 1996; Mader, 1992; Nye, 1991; Nye and Frank, 1973) from those that deal with in-situ glacier ice studies (Cook et al., 2016; Fountain and Walder, 1998; Irvine-Fynn, 2008; Müller and Keeler, 1969), from those that deal with subsurface radiative properties of glacier ice (Brandt and Warren, 1993; Liston et al., 1999; Liston and Winther, 2005), and finally, we made effort to support statements focused strictly on cryoconite holes with relevant publications (Boggild et al., 2010; Cook et al., 2016) from those that deal with microbial communities in glacier ice more generally (Cook et al., 2012; Irvine-Fynn and Edwards, 2014). We hope the writing style has been improved.

Minor comments and suggestions (some touching on points above) would include:

<u>Page 1</u>

Line 1: Suggest hyphenate "near-surface" throughout. (There are some variations, e.g. P3 L13 and L15).

Author response: As requested, 'near-surface' has been hyphenated throughout.

Line 2: "Greenland Ice Sheet", as used throughout the manuscript. It is refreshing to see authors correctly use the appropriate capitalisation for proper nouns (it shouldn't be the Greenland ice sheet, given it is a specific location and entity) and at times I wish publishers would adhere to grammatical correctness – but that is another discussion altogether. L2: "Meltwater storage in low-density near-surface bare ice in the ablation zone of the Greenland Ice Sheet" might read a little better perhaps?

Author response: We agree the title reads better (with slight modification) as "Meltwater storage in low-density near-surface bare ice in the Greenland Ice Sheet ablation zone"

Line 16: Suggest referring to this as "specific storage".

Author response: "liquid meltwater storage" has been changed to "specific storage" here and throughout the manuscript, as requested.

Line 17: Clarify the water level is depth from the surface or from the base of the auger holes, and is "recharge" a more preferable term than infilling (given this is a hydrology paper).

Author response: Here, water level is the depth from base of the holes (i.e. height of water in holes). We report these water levels as they are suggestive of water storage in cryoconite holes. Elsewhere, we report water levels relative to the surface. "Infilling" is replaced with "refilling" here and throughout the manuscript, as requested.

Line 18: “These observations are consistent. . .” given you present results and discuss them. Analysis might be provisional with clear directions to follow, but don’t negate the potential utility of these observations.

Author response: Thank you for this suggestion, we agree and have removed “Though preliminary … “.

Line 21: “supraglacial catchment”

Author response: “catchment” has been changed to “supraglacial catchment”, as requested.

Line 25: A longer opening paragraph would be stronger as an opening. Can there be a clearer link from mass balance or ablation to runoff models for Greenland, and the assumptions regarding the efficient delivery. The sea level aspect here seems misplaced, as the study looks at in-season delays or reductions in discharge. Surely, noting the assumed efficient drainage is now being examined more closely with reference to the firn aquifer and so on would allow for a stronger introduction paragraph here.

Author response: We have lengthened the opening paragraph, as requested. To clarify the efficient delivery to surrounding oceans statement, we added reference to several 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. We first note the evidence for, and assumption of efficient runoff delivery, then note evidence of time lags and possible retention. Finally, we suggest the weathering crust as a possible mechanism for runoff delays based on evidence from other supraglacial environments. Weathering crust formation, ablation, and density are then discussed in the second paragraph.

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)."

Line 28: what is a "terminal moulin"? And not all runoff goes to moulins – there are supraglacial routes to proglacial regions, and lakes and crevasses. Suggest more circumspect and/clarified text here.

Author response: A "terminal moulin" is a moulin that exists at the terminal drainage point of a supraglacial catchment. Analysis of sub-meter resolution WorldView-1/2 satellite imagery suggests that every supraglacial river in the study region drains to a moulin before reaching the ice edge (Smith et al., 2015). To avoid confusion, we have dropped the word "terminal".

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Line 5: Cite Muller and Keeler (1969) for the introduction of the term "weathering crust". Might an additional diagram be helpful here to conceptually illustrate what you are focused on here for the less familiar reader?

Author response: As requested, Muller and Keeler (1969) have been cited following the first mention of 'weathering crust'. We agree a diagram would be useful and are currently working with a digital graphics specialist at our university to produce an updated diagram.

Line 6: Fountain and Walder (1998) also note minimal delay to supraglacial runoff and so text and citation here, given phrasing, might be slightly inappropriate. Suggest check the source again.

Author response: Fountain and Walder (1998) has been removed, as requested.

Line 9: What is a "seasonally temperate glacier"? Poor terminology, please revise. Seasonally temperate surface ice perhaps, but glacier thermal regime is a very different thing.

Author response: Thank you for this important correction. We were referring to the seasonally temperate near-surface, not the broad glacier thermal regime. We replaced our various usages of "temperate", "polythermal", etc. with "thermally transient ice surface" as in Irvine-Fynn et al., (2011). Elsewhere, we have removed mention of thermal regime altogether and replaced with "supraglacial environment" etc.

Line 11: Surely the depth is ice-type dependent, and stating "~2m" is not strictly correct. Consider rephrasing (see Cook et al., 2016). It was also surprising that at no point is Munro (1990, AAAR) cited here, a source confirming the subsurface melt and bulk ice density variations leading to uncertainty in runoff volumes at Peyto Glacier. Suggest consideration of this source, especially with regard L19.

Author response: We agree "~2m" is unnecessarily specific and we have replaced with "typically <2 m thick, owing to the exponential attenuation of radiation penetration with depth (Brandt and Warren, 1993; Irvine-Fynn and Edwards, 2014; Müller and Keeler, 1969)."

Regarding Munro (1990), thank you for suggesting this highly relevant reference. Though we cited Munro (2011) in the original manuscript, this additional citation strengthens the literature review for the reader. We have added this citation to L19, as requested.

Line 17: Doesn't lateral meltwater motion result in sensible and frictional heat transfers, contributing to further removal of ice mass. Also suggest clarification over the vertical extension of the weathering crust, and how this influences mass for any given vertical position. The process described by Muller and Keeler (1969) is a little more complex than perhaps is given credence here, and perhaps a more careful description could be afforded. See also Cook et al., 2016.

Author response: Regarding sensible and frictional heat transfers, we have added this to our interpretation of the weathering crust structure in Sect. 3.1, where we suggest meltwater advection along micro seams and cracks may enhance subsurface weathering.

To address weathering crust vertical structure, we reference the characteristic depth-density profile from LaChapelle, (1959), and note the exponential attenuation of solar radiation in the upper few meters of ice (e.g. Brandt and Warren, 1993). We are working with a graphics specialist in our department to create a diagram that merges the weathering crust conceptual diagram (Müller and Keeler, 1969) with the characteristic depth-density profile (LaChapelle, 1959) that we hope will further clarify the depth-variable nature of the crust.

Line 22: The opening of this paragraph is not entirely appropriate, the structure and the content seems slightly superficial and/or repetitive (e.g. mention of delay in runoff is already in L6). Suggest revisiting this text through to L26 and P3.

Author response: Considering this and the next several comments, we have substantially revised the introduction. The revised introduction has the following structure:

1. Broad motivation – ablation zone hydrology is poorly represented in models, particularly with respect to seasonal runoff delays and retention
2. Definition / description of weathering crust formation and relevance to supraglacial hydrology and surface mass balance
3. Description of broader relevance with respect to microbial habitat and albedo
4. Justification and purpose for this study in Greenland

Line 22: Is subsurface melting in Antarctic contexts the same as the definition provided of weathering crusts on "temperate ice" (see L9)? Strongly advise some differentiation between subsurface melting and weathering crust terminology. This sentence could be removed at no loss to the paper.

Author response: We agree and have removed the reference to Antarctic contexts, as requested.

Line 25: Slightly unconvincing use of the literature here: some references focus on cryoconite holes, others on the weathering crust as a habitat. Recommend revisiting, with perhaps consideration of recent messages regarding glacier ecohydrology (e.g. Dubnick et al., 2017a,b, JGR and Hydro Proc.; Hotaling et al., 2017, Env Mic.; Milner et al., 2017, PNAS). Yes, the weathering crust is a substrate for cryoconite holes (see Muller and Keeler's 1969 diagram), but the focus here should be the hydrological aspects and for example disturbance to cryoconite holes that might influence their ecology (Edwards et al., 2011, ISME J; Mieczan et al., 2013, PPR) or distribution (e.g. Hodson et al., 2007, JGR). Then develop the 'undescribed in Greenland to date' message and the guide of what is to follow (subsequent paragraph). If you do touch on the biogeochemical cycling aspects, it might be helpful to touch on these again in the discussion section.

Author response: In the revised introduction, we discuss weathering crust relevance to microbial habitat in a standalone paragraph. As suggested, we emphasize the relevance of weathering crust hydrology (Cook et al., 2016) as a control on glacier ecology via 1) cryoconite hole distribution (Hodson et al., 2007; Takeuchi et al., 2000), and 2) saturated interstitial void space (Irvine-Fynn and Edwards, 2014). We then develop the 'undescribed in Greenland to date' message in the subsequent paragraph. The biogeochemical message is revisited in the discussion, as suggested, where we provide slightly more detailed discussion and additional references.

Page 3

Line 1: Remove "In sum"

Author response: Removed, as requested.

Line 2-4: Consider revisiting (see L25 above), and bringing in energy balance and ablation (see again Munro, 1990, AAAR) and describing the reasons for weathering crust relevance. Then have a single paragraph giving the justification for the study in Greenland. I just found these two paragraphs jump around a little and felt that a more logical progression through material could be achieved.

Author response: Thank you again for these helpful suggestions. We hope the revised introduction provides a clear progression and justification for the present study.

Line 20: delete "mechanical" – not necessary. Be consistent with hyperlinks/formatting if used for www sources.

Author response: Deleted, as requested.

Line 21: "drilling" in glaciology typically implies more than shallow coring, might just talking about "coring" and "core sites" be sufficient? (e.g. P4 L1 "core sites" seems more appropriate).

Author response: "drilling" and "drilling sites" are changed to "coring" and "core sites", as requested.

Line 23: Issue of mass for calculation of density is relevant here. Are you measuring water and ice? If so, are not the estimates of density in error. This issue needs to be addressed and accounted for; ice density has to be properly estimated given the depth variable water content.

Author response: The analysis assumes we are measuring dry ice density. Though water retention errors are inevitable, we are not aware of an explicit estimate of depth-dependent water content for the very near-surface weathering crust we document in this study. As suggested, 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 well within our ±20% storage error. We provide a thorough reporting of this error source in the methods.

Line 29: Suggest "This uncertainty is incorporated into calculations of ice porosity and water content (see Sections 2.2 and 2.4)". In places, as here, writing clarity and conciseness could be tightened up.

Author response: As requested, the sentence has been revised to read "This uncertainty is incorporated into calculations of ice density and porosity, propagating into ±20% uncertainty in specific storage (see Sect. 2.2 and Sect. 2.4)."

Page 4

Line 8-9: Clarify the relevance of the centre of mass, if you are using the method to estimate the upper 14-30cm ice, just indicate that the upper 20cm is used, but the sampler geometry results in bias toward the uppermost ice and so leads to an underestimate of density. This just seems to be introducing terms which could be seen as confusing.

Author response: As requested, the reference to "centre of mass" is removed and the sentence now reads: " … the density measurements may be more representative of the uppermost ~6 cm of material because of the shape of the sampler (see Fig. 2)".

Author changes in manuscript:

Line 11: Issue of water content in core sections and uncertainties in density measurements remains problematic.

Author response: Thank you for drawing attention to this important source of uncertainty. We hope our clarified discussion of the two primary sources of expected error (water retention and loss of material) are sufficiently addressed in the revised methods.

Line 12-15: This could be condensed: e.g. "for context, two 1.8m cores were extracted but ice density measurements were not undertaken, these cores are described further in Section 3.3".

Author response: The sentence now reads as suggested.

Line 15: Estimates of porosity will be affected by ice core sections that were weighed still containing interstitial water. Removal of this water is not a trivial problem, as exposing the core to positive air temperatures will initiate further melt, and methods of forcing water out via centrifugal force is similarly challenging. An estimate of uncertainty is needed here, and this needs to be incorporated into the data derived from these potentially erroneous ice mass measurements.

Author response: Most estimates of temperate ice water content are less than <3.4%, well below the assumed ±10% uncertainty we prescribe for density and porosity. Water retention errors would tend to cancel (to an unknown extent) with the expected volumetric errors due to loss of material at the irregular ends of the core segments. We appreciate the point and do not take it lightly, but we do not think there is a reasonable way to construct a physically based error estimate beyond the provided ±10% (density and porosity) and ±20% (specific storage). We highlight these sources of error in the methods and remind the reader as they are presented in the results and discussion.

Line 20: rationale for the change in ( ) to [ ]?

Author response: Thank you for noticing, [ ] has been changed to ( ) throughout.

Line 25: Might combining the equations here to A = B > C (as Eq 1) seem a neater and more consistent presentation of the equations? Would allow a slightly smoother explanation.

Author response: The equations are combined, as requested.

Line 30: Does the time-frame and temperature of the water present any issue here? Given the thermal potential of supraglacial water (which I presume was used?), could you estimate and mass loss (or confirm this is negligible). The size of the weathering crust crystals might be important here – were the samples used representative of the upper 20cm for all sites?

Author response: Supraglacial water was used for the reason you mention. Water was immediately applied and, as noted, we carefully observed ice crystal structure and air bubbles and saw no evidence of melt. To some extent, the focus on the quality of the linear regression (noted below in a separate comment) was motivated by this concern. The linear relationship suggests 0% effective porosity for solid ice density (917 kg m^{-3}), which provides some confidence in the measurements, and the estimates made from the relationship. Regarding representativeness, we collected samples at every core site as well as 15 additional sites along the transect to increase the sample size (N=25). This has been noted in the revised methods.

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Line 9: While it is good practice to cite, do we need more than one of two examples here? Just considering journal space.

Author response: We reduced the citations to two, as requested.

Line 19: “8m intervals”?

Author response: We changed to “8m posting”, to distinguish them as physical locations, e.g. “cryoconite holes were measured within 1 m radius of the posting …”

Line 20 & Page 6 Line 2: were cryoconite holes ubiquitous features, or did you measure those proximate to the sample point? Clarify here. If one hole was measured – is this representative of local water table – might measuring 4 holes in at each site have provided more robust estimates?

Author response: They were ubiquitous, but for expediency we measured the nearest cryoconite hole within a 1 m radius of the posting. We note this in the revised text. Regarding representativeness, we were not able to confirm this via direct measurements at each posting (again, owing to severe time limitations). Measuring 4 holes would certainly provide more robust estimates of local variability but we were more concerned with obtaining an adequate sample size to establish conditions along the transect. We have added the following statement where we report water levels: "The height of water in these holes likely varied diurnally and could have steadily drained or filled during the study period (Cook et al., 2016). The 15.5 cm average depth to water thus likely represents a snapshot of the transient water table height. Further, owing to severe time limitations we measured a single hole at each 8 m posting and thus cannot quantify local variability."

Line 21: The steel rod measurements are not entirely convincing, can you justify this a little more clearly. Furthermore, as above, a conceptual model might help here. Perhaps you need to consider the density decay curve (LaChappelle, 1959) and clarify your reasoning here, or use some alternative term in terms of a qualitative measure of "weathering crust mechanical resistivity" to the steel probe to indicate perhaps the shoulder on the density decay curve? There is also the issue of capillary draw in the weathering crust, are you able to confirm the water table in the crust is the same as that in the ice matrix? Does the water table truly exist as a broadly consistent level? If not, is this an uncertainty you can at least note if not estimate.

Author response: Thank you for this important critique. We agree the steel rod measurements were not presented clearly. We have removed the a priori characterization of the saturated/unsaturated transition where the depth probe measurements are described in the methods. In the revised manuscript, we use the depth to water below the ice surface in cryoconite holes as an estimate of the depth to saturation, whereas the depth probe measurements are used as a qualitative characterization of the weathering crust structure, drawing on the characteristic subsurface depth-density profile for weathering crust (LaChapelle, 1959) as suggested. Thank you for this suggestion, we think it will substantially improve the communication of our results to the reader.

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Line 5: Refrozen water while a component of storage in an overarching sense, is not the liquid storage, and is likely to be a proportion of the total available liquid water following a freezing event or water drainage to a cold front in the ice. If you are talking about liquid "water storage" then surely it is a negative value/term in that it is water lost to freezing? I'd also caution here given the inference is that the ice lenses are refrozen water – which may or may not be the case (see comment below regarding ice structure) and so a clearer definition of water storage might be helpful here.

Author response: We agree and have removed the term, as requested.

Line 10: see L19, but are you exploring a total storage potential or just the saturated ice. Can you remove "saturated" here, and discuss both the observed water storage volume and the potential storage volume?

Author response: We are exploring the actual storage within saturated ice. Our use of the word "potential" was incorrect and misleading. 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 …".

Line 11: do you not just "extract" cores, rather than excavate them?

Author response: We have changed "excavated to "extracted", as requested.

Author changes in manuscript:

Line 19: I think you need to better define the "potential total storage volume" (i.e. the entire weathering crust) vs. the estimated snapshot of water storage yielded by your observations – given the weathering crust storage potential will be time-variant given the nature of the weathering crusts formation. You could then discuss the total storage potential (under the conditions at the time of measurement), and the proportion of that which did indeed hold liquid water (the stored volume), and compare those to melt production or runoff volumes.

Author response: We present the actual (instantaneous) specific storage estimated from the shallow cores, and then scale that to the study catchment to estimate a storage volume. Our use of the word 'potential' was incorrect.

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Line 24: "metre rule"?

Author response: The sentence has been removed altogether as it is redundant with the methods.

Line 32: Given the strong surface expression of structural features in the sector of the ice sheet studies here, you might give an indication that ice structure might underlie this (and perhaps cite papers that note the strong evidence of structural glaciology at the locality, and its theoretical background – for example Hambrey et al, 2000, Geol Soc; Hudleston, 2015, J Struc Geol.). You could at least provide a hypothesis here as a potential guide for future work. Given ice lenses are discussed, and given the ice sheet surface is ablating, these lens features must be emergent – and while it is possible refreezing of meltwater may contribute, do ice temperatures or meteorological conditions support this given the prevalence of these lens features? Were the lenses truly horizontal in formation or exhibit slight orientation?

Author response: We agree these lenses must be emergent and have substantially revised our discussion, including the suggested references. Regrettably we did not make careful observation of their orientation. 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."

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Line 3: "The reported pM values therefore"? Missing word.

Author response: We added "values", thank you.

Line 4: Perhaps use "lens ice" to clarify your meaning here.

Author response: We have substituted 'lens ice', as requested.

Line 6: Surely if lenses are refrozen water their density will likely approach that of pure ice, and if structural features, their persistence would suggest higher glacier ice density values. As such, can you not include and quantify potential uncertainties here?

Author response: In keeping with our interpretation of ice lenses as structural features, we now include the ice lens volume in our estimates of density, volume, and porosity. Either way, as pointed out by another reviewer, it was incorrect to omit the ice lens volume from the original estimate as the mass was incorporated into the density calculation. As such, we have removed the various references to this source of error, which should reduce confusion for the reader.

Line 12: You have two data points above the theoretical limit, so "consistently smaller" is not strictly correct. You also refer to Figure 5 here three times in as many lines – consider using just one reference to the graphic.

Author response: We have removed all but one reference to Figure 5, and replaced "consistently smaller" with "generally smaller".

Line 15-16: "unphysical"? Perhaps use "physically implausible".

Author response: "Unphysical" is replaced with "physically implausible", as requested.

Line 15 & 18: There seems to be an overemphasis on the quality of the data here, please recall the equation used to derive the porosity value means there is circularity here – the porosity is a function of the two densities. Avoid overstating something here. You can simply report the observed relationship, the fact that how robust this is beyond the bounds of observations is equivocal, and that the relationship was used to estimate porosity.

Author response: We removed the overemphasis on the quality of the data here, following the suggested progression of reporting the relationship, the uncertainty beyond the range of observations, and the application of the relationship to the shallow core densities.

Line 28: This section seems a little less flowing than others, and is characterized by short paragraphs. Can the core holes and the water levels noted in these be described further? The first paragraph and third surely belong together? But there is repetition here. Consider revisiting this section.

Author response: As requested, elements of the first and third paragraphs have been combined and the section has been condensed to four individual paragraphs with the following progression:

1) Description of ice surface topography, cryoconite hole depths, and depth to water along the transect
2) Description of refilling of drilled holes and water filled cryoconite holes as evidence of saturation
3) Description of the two-layer structure referencing the LaChapelle depth-density profile
4) Description of higher density material structure and possible lower bound on permeable ice from the two 1.8 m cores

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Line 5: So do dry holes indicate the water table is more complex and not a level surface?

Author response: Perhaps, but dry holes were on average 8 cm shallower than wet holes, suggesting this result may be due to random sampling of shallower holes. More importantly, we think, is the lack of a trend in depth to water below the surface, suggesting the water table generally mirrored the local topography.

Line 7: not sure you need to use caption detail in the figure reference here.

Author response: Caption detail was removed, as suggested.

Line 11: I think you need to define where the ice is saturated – it isn't the full depth of the weathering crust, or is it? Just feel a little more clarity in needed here to ensure the observations and inferences are clearly described.

Author response: As requested, we state clearly that depth to saturation is inferred from the depth to water in cryoconite holes.

Line 21: You don't really have a handle on the "transient" nature of the weathering crust here – yes, you can conceptualise this as a two-layered feature. But although you show spatial variability, you have no detail on temporal change. I would focus on the message relating to the snapshot of water storage – and the volume that represents. And only in your discussion, mention the processes of weathering crust formation and how this would mean the depths of the porous and saturated ice would potentially vary.

Author response: As requested, 'transient' has been removed and the paragraph/section now focuses on the snapshot of water storage by reporting the measurements we made, namely, the average depth of cryoconite holes, the average depth to water, the trend toward shallower holes with distance (-0.014 cm m^{-1}), and the lack of trend in depth to water, suggesting the water table generally mirrored the local topography.

We continue to think the two-layer description is helpful, simply because there was such a distinct transition in density/structure observed in the field, and because it was remarkably consistent with the structure implied by the diagram in Müller and Keeler, (1969). The primary evidence for the two-layer transition comes from the depth probe and shovel, since the corer was unable to remove the upper material intact and sampling resolution of density was limited to ~13 cm. As you suggest, the depth probe and shovel provide indirect evidence of a certain material strength, and is merely qualitative. To clarify this for the reader, we reference the density decay curve as suggested. We think the conceptual schematic we are preparing for the introduction will also greatly improve the communication of this idea to the reader. That said, we have debated the usefulness of the two-layer description and can remove it if you think it detracts from the quality of the presentation.

The sentence now reads "Based on these observations, we characterize the near surface ice as consisting of two bulk, though in principle continuous, layers."

Line 34: Further evidence for structural controls on the ice crystallography?

Author response: We think so, yes.

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Line 4: Repetition of freezing leading to cessation of coring from method section, un-helpful here as a result section.

Author response: The repetitive statement was removed, as requested.

Line 6: It would be nice to see a little more result reporting here – not solely the reference to the table and the mean for all sites. Perhaps expand a little.

Author response: This section has been expanded, as requested. We report the specific storage depth (referencing the table), and then use this as a transition to discuss how the depth of weathered ice and storage relate to the seasonal context. Here, we report the maximum spring snow depth, date of snow disappearance, cumulative ice surface ablation following snow disappearance, and comparison with regional meteorology reported in Tedstone et. al., (2017).

Line 13: Just wondered if a clearer summary section leading to discussion might be helpful – in following with the results. For example, open with the lacking recognition of the weathering crust, and how here, observations of ice density revealed X Y and Z on a portion of the Greenland Ice Sheet, then move to how the two-layered structure matches previous work, and how density and water storage values compare to the other limited reports.

Author response: The opening paragraph has been combined with the second paragraph and reorganized according to the recommended structure i.e. 1) lack of recognition of weathering crust storage volumes, 2) broad restatement of our findings, 3) two-layered structure consistent with previous work, and 4) density and water storage consistent with previous limited reports.

Line 14: Cite the Larson reports and Irvine-Fynn et al review that discuss near-surface surface storage here. Would citing the Jansson et al (2003, J Hydro) review also be useful here?

Author response: We have added the Larson, Irvine-Fynn, and Jansson citations, as requested.

Line 15: Why the specifics on polythermal ice sheets here? The references cited discuss temperate glaciers and a polythermal glacier, respectively.

Author response: References to thermal regime have been removed, as requested.

Line 21: "stagnating"??

Author response: "stagnate" has been replaced with "stagnating", thank you.

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Line 1: Recall the reports you cite simply modelled water storage via water budgets – and so you can't compare core observations to hydrological models. Previous work hasn't examined ice cores to identify or report crystallographic changes. Please revisit.

Author response: The references to specific storage rates have been removed and replaced with a general statement of consistency with previous findings of substantial water storage: "Moreover, these results are consistent with observations of substantial water storage within the weathering crust of supraglacial environments worldwide (Irvine-Fynn, 2008; Larson, 1978; Munro, 1990)."

Line 4: See earlier comments on ice structure.

Line 8: See earlier point about water budget equation, and the ice lenses being a negative 'storage' value, as indicated here. However, a stronger physical discussion of the potential formation processes for the ice lenses is needed – with comparison to ice structure and any alternative explanations too.

Author response: This paragraph has been substantially revised to emphasize the interpretation of the lenses as structural features. However we do use this paragraph as an opportunity to provide a more nuanced view of refreezing, highlighting work that suggests negligible refreezing in porous near-surface ice (Schuster, 2001; Wheler and Flowers, 2011) as well as non-negligible refreezing (Hoffman et al., 2014a).

Line 10: GrIS – either define and use as acronym throughout or use Greenland Ice Sheet as elsewhere.

Author response: GrIS has been changed to Greenland Ice Sheet, as requested.

Line 11: Condense to a single paragraph section perhaps?

Author response: The two paragraphs are condensed to a single paragraph, as requested.

Line 25: I'd suggest revisiting in view of the Munro (1990, AAAR) source.

Author response: We are not sure exactly what the reviewer is asking us to revisit in view of Munro (1990). We suspect it may be the finding that discrepancies between ablation and runoff were reconciled for that experiment by combining a detailed energy balance model with a specially designed ablatometer. If so, we agree the Munro experiment presents a useful demonstration this is possible, though we think it equally highlights the difficulty required to reconcile the effect of weathering crust on runoff and mass balance. We cite Munro (1990) throughout the revised discussion where relevant.

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Line 24: Lutz reference focuses on ice algae, not cryoconite. Suggest Wientjes and Boggild references would be more appropriate here. Similarly, L25: Fountain discussed ice-lidded cryoconite in Antarctica which may physically be a little different – suggest a more cautious use of literature which refers to the types of feature and observations that are characteristic for Greenland (e.g. the older Gribbon, 1979, J Glac. or Gadja, 1958, Can Geogr. references for cryoconite holes in Greenland).

Author response: The sentence has been revised to discuss microbial communities in general, with references added and revised as suggested.

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Line 12: Does Hoffman's study relate to a temperate or polythermal ice mass – isn't it cold? Or just remove the thermal regime aspect here – "supraglacial environments elsewhere. . ."

Author response: The reference to thermal regime has been removed and replaced with "supraglacial environments worldwide", as suggested.

Line 15: You define the symbology, no need to repeat the definition here in L16, after its use on L15.

Author response: The symbology has been removed, as requested.

Line 19: For impact, suggest you rephrase as "if these observations are representative of the Greenland Ice Sheet ablation zone, then wider implications are. . ., and future work should. . .."

Author response: The conclusion has been rephrased as suggested: "If these findings are representative of broader areas of the Greenland Ice Sheet ablation zone, they suggest the potential for substantial sub-seasonal meltwater storage within porous low density ice on the Greenland Ice Sheet ablation zone bare ice surface. Future work should examine how the weathering crust evolves in response to spatio-temporal changes in the surface energy balance, and quantify potential errors in sub-seasonal SMB and surface elevation change estimates derived from surface energy balance models and altimetry, as most currently neglect removal of mass via subsurface melting in the weathering crust."

Fig.3.: Consistency with (..) or [..] on axes labels. The captions seem to be overly long – focus on the content and remove superfluous text. Label 1 -10 in the figure. Hatched areas are “no data” not “core depth” are they not? Can you not include the snow- shovel data here for the uppermost 20cm – albeit in a different colour, for comparison and completeness? Might inclusion of potential ablation here be helpful given from the field campaign description, the cores were collected over one week during which time ablation would take place – and such that (for example) a refreezing event (if this is what the lenses are) might be more clearly identified if lenses appear at the same depth relative to a zero set for the period of coring?

Author response: The cores were collected on 11-12 July. We state this clearly in the revised methods. We have updated axis labels with consistent use of (), labeled the cores 1-10 in Figure 3, changed ‘core depth’ to ‘no data’, and gap filled the upper 20 cm with the snow cutter density measurements, as requested. Revised figures are included at the end of this documented.

Fig.4.: Is the lower image for the core in the upper? Perhaps use arrows to indicate where ice lenses are on the core.

Author response: The photos were taken from different cores but are the best photos we have. The caption has been updated to note the core location.

Fig.5.: y-axis should be phi-eff. The equation given should be phi-hat-eff (inconsistent symbology). Surely “observations” not “data”? Caption – is “measured data” needed here?

Author response: The equation has been corrected to read phi-hat-eff, ‘data’ has been changed to ‘observations’, and ‘measured data’ has been removed from the caption, as requested. Regarding the y-axis label, since the axis is used for phi-hat-eff, measured phi-eff, and phi-total, we think it is better to leave the label as generic phi [-] and let the legend distinguish, though if requested we are happy to change to all three relevant symbols.

Fig.6.: (b) there is a lot of information here, and I just wonder if two panels here would be helpful – one to give clearer indication of the water level in holes with a simple zero as ice surface, and then the detrended plot with the unsaturated crust estimate? The two grey tones are hard to differentiate. If detrended, surely the data should be scattered around zero – so did you offset this to a maximum positive deviation - one presumes so, but clarification would be appropriate? Have you compared distance or elevation against any of the variables – are there any other patterns to explore – as these don’t seem to have been mentioned in the main text – even if to confirm there is no elevation dependency.

Author response: The grey shaded area has been removed to improve the clarity, as requested. We also added some empty space at either end and added the depths of the shallow cores as per a request from another reviewer.

Regarding the offset, yes, they were all offset to the maximum positive deviation such that the datum is 0. This was done for consistency with the conceptual diagrams of Muller and Keeler (1969) and Irvine-Fynn and Edwards (2014).

Regarding distance relations, as noted elsewhere we did find a slight trend toward shallower holes (-0.014 cm m^{-1}) but no significant trend (or apparent trend whatsoever) in depth to water below the surface.

Table 1: could you include a column of mean phi-eff for each core here, for ease of direct comparison?

Author response: The mean phi-eff has been added as well as mean density. The porous ice and lens ice depths have been removed, but can be added back if requested.

Updated figures/table:

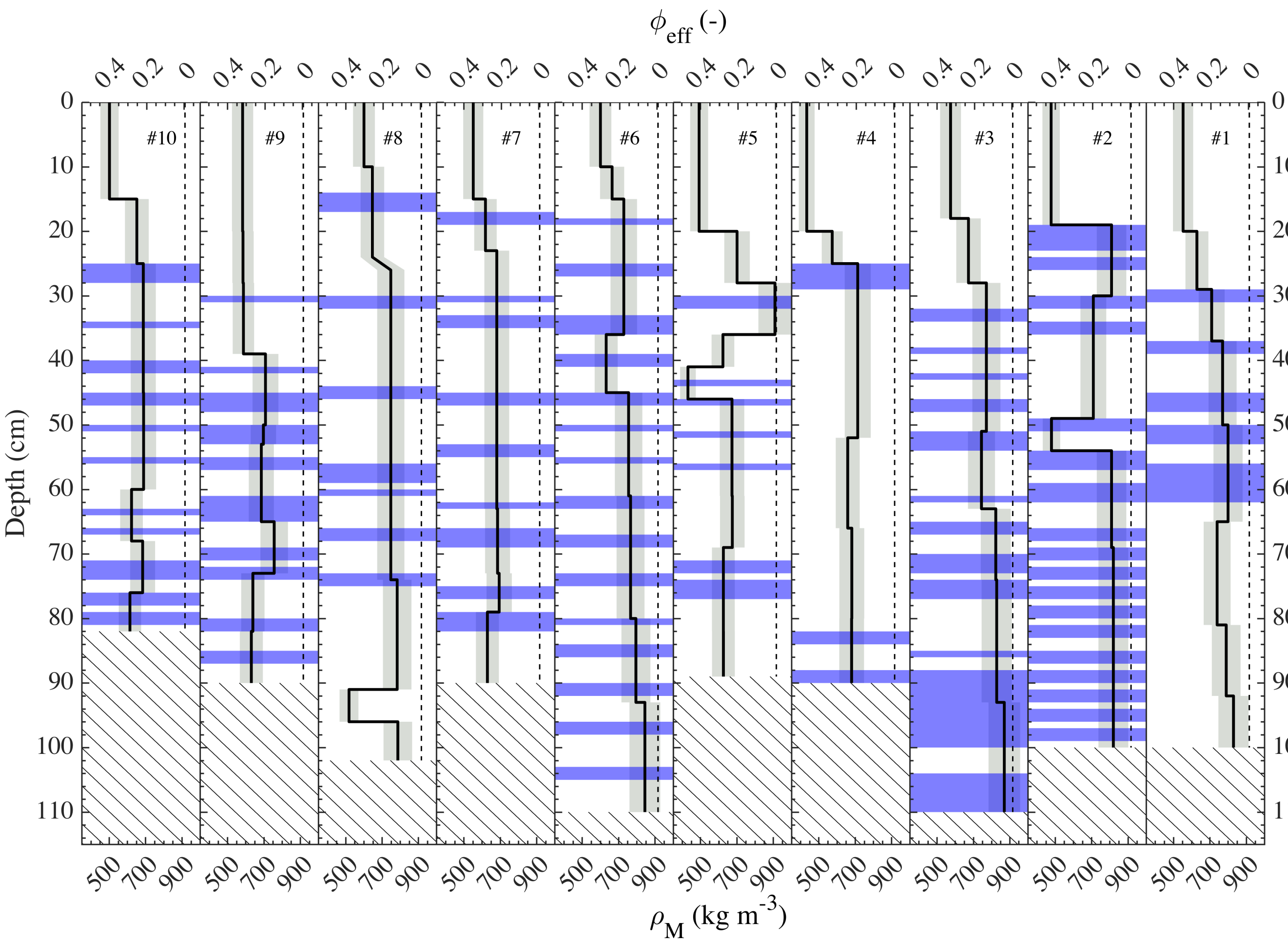

Figure 3: Subsurface measured ice density (ρ_M) and corresponding calculated effective porosity (ϕ_{eff}), and stratigraphy profiles from 10 shallow ice cores (#10-1, left to right) extracted at 80 m postings along the study transect (see Figure 1 for ice core locations). Horizontal blue shading represents 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.

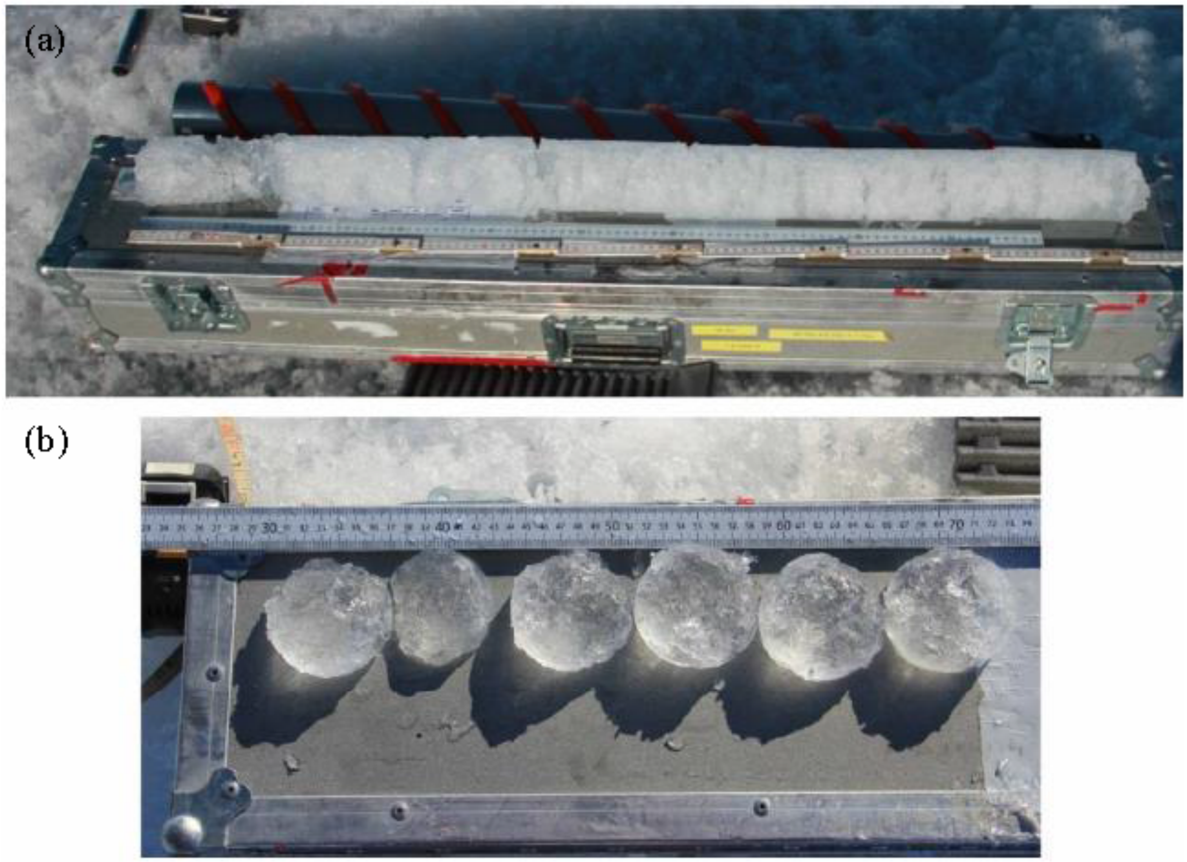

Figure 4: (a) Typical near-surface shallow ice core (core #6) prior to in situ analysis of density and stratigraphy. Clear, solid ice lenses alternate with granular, fractured ice. (b) Ice lenses removed and confirmed after completed core analysis (core #1).

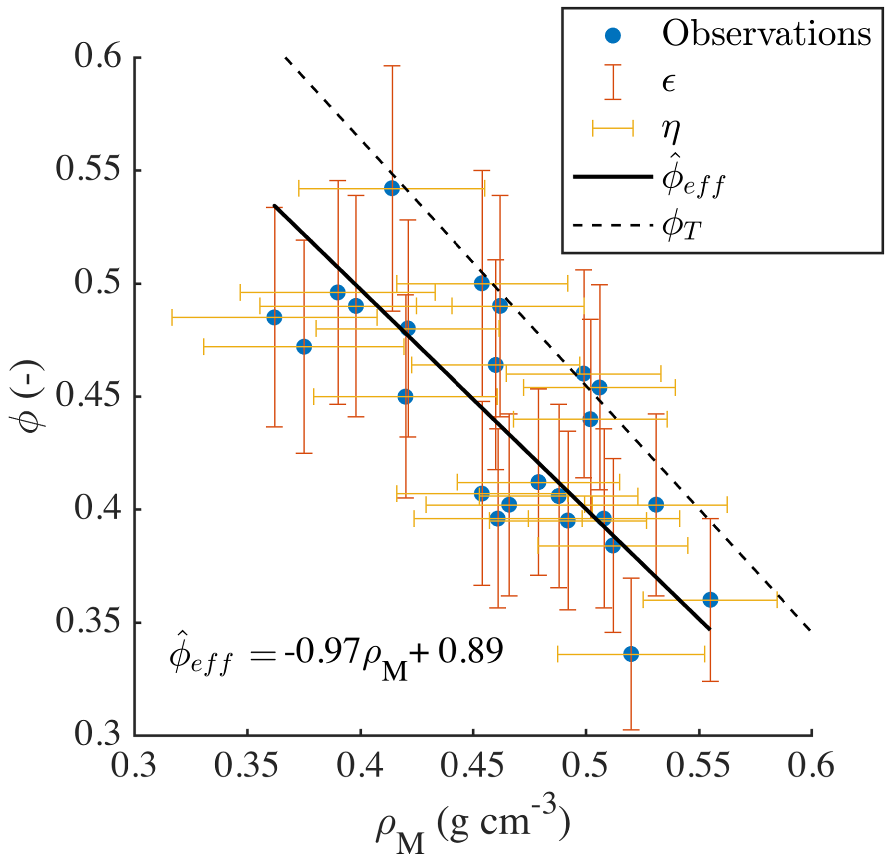

Fig. 5: Linear relationship ($\widehat{\phi}_{eff}$, solid line) between measured ice density (ρ_M) and effective porosity (ϕ_{eff}) and assumed ±10% measurement error (whiskers). Dashed line is theoretical upper limit where effective porosity equals total porosity (i.e. $\phi_T = \rho_M/\rho_T$).

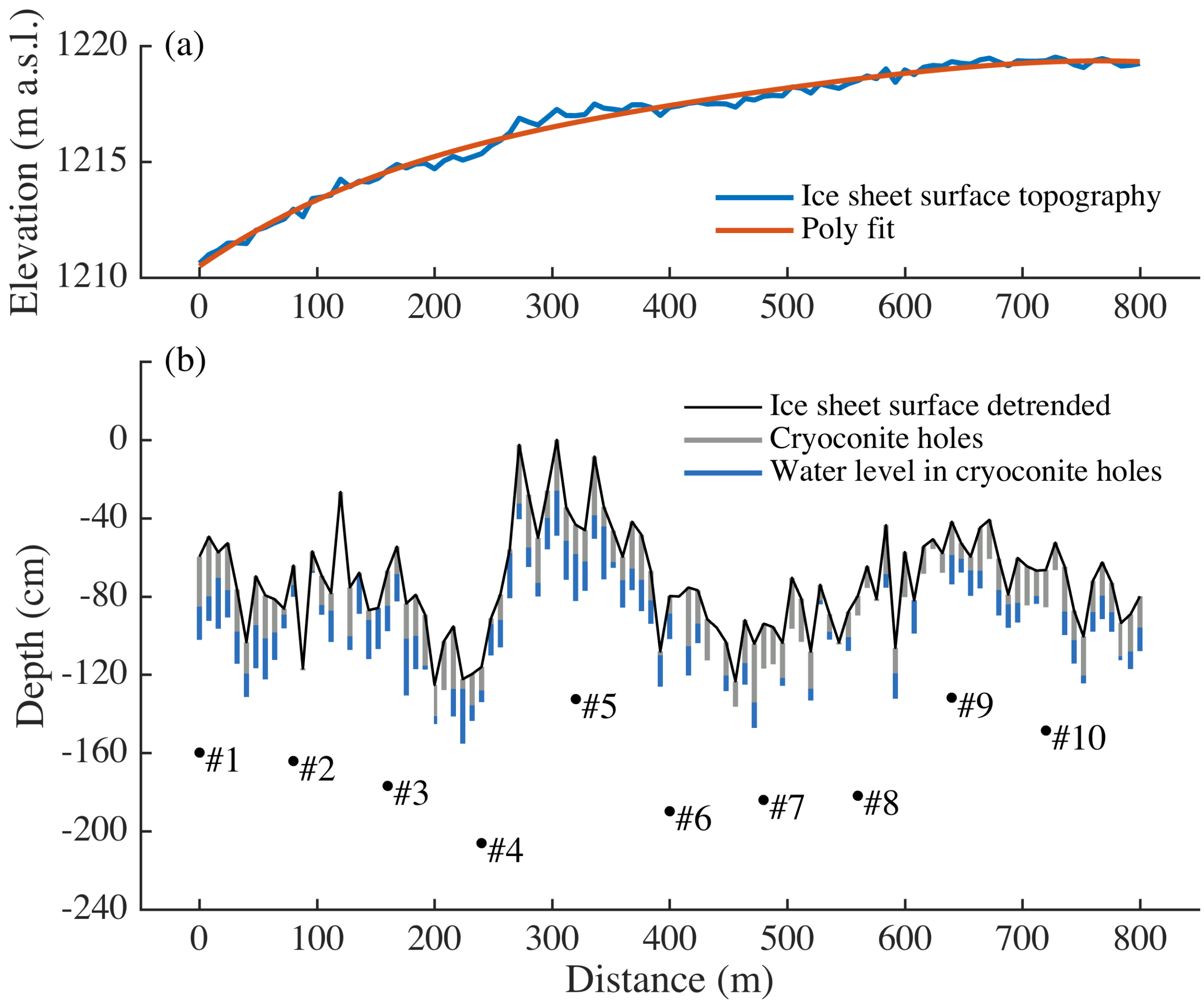

Fig. 6: (a) Ice sheet surface topography along the 800 m study transect extracted from a 6 cm posting stereo-photogrammetric digital elevation model derived from RGB imagery collected 10 July 2016 from a quad-copter drone and the 2nd-order polynomial best fit. (b) Ice sheet surface topography detrended with the polynomial best fit, cryoconite 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.

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

Core	Ice Core Depth (cm)	Mean Core Density (g cm^{-3})	Mean Core Porosity (-)	S_P (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**

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