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Interactive comment on "Near surface meltwater storage in low-density bare ice of the Greenland ice sheet ablation zone" *by* Matthew G. Cooper et al.

Anonymous Referee #3

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This paper reports on the findings from a field campaign on the ablation zone in south-western 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 and 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 $\sim$0.2 m is derived, suggesting that at the time of observations a water volume equivalent to 1 hour's worth of discharge from the local supraglacial catchment was essentially stored within the weathering crust.

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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? 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).

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.

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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.

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 takes 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.

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?

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.

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

P1: L1: Suggest hyphenate “near-surface” throughout. (There are some variations, e.g. P3 L13 and L15). L2: “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? L16: suggest refer to this as “specific storage”. L17: 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). L18: “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. L21: “supraglacial catchment” L25: 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. S L28: 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.

P2: L5: 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 fo-

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Discussion paper

cused on here for the less familiar reader? L6: 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. L9: what is a "seasonally temperate glacier"? Poor terminology, please revise. Seasonally temperate surface ice perhaps, but glacier thermal regime is a very different thing. L11: 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. L17: 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. L22: 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. L22: 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. L25: 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.

P3: L1: Remove "In sum" L2-L4: 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. L20: delete "mechanical" – not necessary. Be consistent with hyperlinks/formatting if used for www sources. L21: "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). L23: 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. L29: 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.

P4: L8-9: 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. L11: Issue of water content in core sections and uncertainties in density measurements remains problematic. L12-15: 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". L15: 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. L20: rationale for the change in ( ) to [ ]? L25: 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. L30: 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?

P5: L9: while it is good practice to cite, do we need more than one of two examples here? Just considering journal space. L19: “8m intervals” ? L20 &P6 L2: 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? L21: 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.

P6: L5: 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

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Discussion paper

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. L10: 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? L11: do you not just "extract" cores, rather than excavate them? L19: I think you need to better define the "potential total storage volume" (ie. 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.

P7: L24: "metre rule" ?? L32: 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?

P8: L3: "The reported pM values therefore" ? Missing word. L4: perhaps use "lens ice" to clarify your meaning here. L6: 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? L12: You have two data points above the theoretical limit,

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Discussion paper

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. L15-16: “unphysical” ? Perhaps use “physically implausible”. L15 & L18: 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. L28: This section seems a little less flowing than others, and is characterised 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.

P9: L5: so do dry holes indicate the water table is more complex and not a level surface? L7: not sure you need to use caption detail in the figure reference here. L11: 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. L21: 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. L34: further evidence for structural controls on the ice crystallography?

P10: L4: repetition of freezing leading to cessation of coring from method section, unhelpful here as a result section. L6: 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. L13: Just wondered if a clearer summary section leading to discussion might be helpful – in following with the results. For example, open with the lacking recog-

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Discussion paper

nition 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. L14: 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? L15: why the specifics on polythermal ice sheets here? The references cited discuss temperate glaciers and a polythermal glacier, respectively. L21: "stagnating" ??

P11: L1: 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. L4: see earlier comments on ice structure. L8: 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. L10: GrIS – either define and use as acronym throughout or use Greenland Ice Sheet as elsewhere. L11: Condense to a single paragraph section perhaps? L25: I'd suggest revisiting in view of the Munro (1990, AAAR) source.

P12: L24: 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).

P13: L12: 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. . ." L15: You define the symbology, no need to repeat the definition here in L16, after its use on L15. L19: For impact, suggest you rephrase as "if these ob-

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Discussion paper

servations are representative of the Greenland Ice Sheet ablation zone, then wider implications are. . ., and future work should. . .."

Fig.3.: Consistency with (..) or [..] on axes labels. The captions seems 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?

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

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?

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.

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

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