Dear reviewer,

Thank you for the thorough and supportive comments. The suggestion to contextualize the study in terms of the seasonal cycle and to quantify the effect of sub-surface porosity on ice mass were particularly fruitful. 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 #1

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The paper presents the results of a recent field survey conducted to assess the potential for subsurface meltwater storage in porous ice in the ablation zone of the Greenland ice sheet. Focusing on a small, internally draining, hydrological catchment in the much-studied South West, the authors find that the subsurface 'reservoir' consists of two layers. These consist of a thin, light, unsaturated layer atop a thicker (⇠1 m), denser, saturated layer. Between them, these layers provide storage potential for up to 20 cm of meltwater which, integrated over the catchment, is the equivalent of one hour's proglacial discharge from this sector.

The methods employed in the study are sensible, and their results are very interesting, however the manuscript is a little confused in places and I would have liked to see the field data discussed in more detail. That said, this is a good paper and I expect will make a solid contribution to the literature subject to editing as follows.

Major comments

1. The paper is lacking in analysis of spatial variability along the transect studied. For example, Fig 3 reveals that ice lenses are not always common between adjacent cores. Further investigation into these features and why they arise would be both interesting and serve to strengthen the manuscript.

Author response: We have added substantial interpretation of the ice lenses that will help explain the lack of consistent stratigraphy in adjacent cores. We believe the ice lenses are structural ice features, not refrozen meltwater. For the crust to develop, the ice must be temperate, which should prevent substantial refreezing. 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. We think it is more likely the lenses are remnant solid ice that has undergone less weathering owing to structural heterogeneity in ice grain size, bubble size/content, and impurities. We also speculate that meltwater advection along micro seams or cracks in the ice may promote differential weathering, similar to joint block weathering of terrestrial lithology. Finally, 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), thus spatial analysis of lens features is unlikely to prove fruitful. Each of these would suggest lenses are local features, helping to explain the lack of consistent stratigraphy among cores. In the revised, we have added substantive descriptions of these ideas to support our hypothesis that the lenses are structural features, not refrozen meltwater.

To address spatial variability in cryoconite holes along the transect we tested for statistically significant linear relationships between distance 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, along with a strengthened discussion of variability along the transect.

2. While you do not have seasonal data, you should contextualize your findings in terms of the seasonal cycle. For example, what is the ablation rate in this location?

Author response: Thank you for the excellent suggestion to contextualize our study in terms of the seasonal cycle. 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 dark ice dynamics 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 (see Figure 3 in Tedstone et al., 2017), consistent with the AWS data. 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.

2. (continued) Is your weathering crust likely to be the product of one, or more melt seasons?

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. 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 likely substantial and we prefer not to comment on interannual carryover without a thorough analysis. That said, we have added a note in the revised that the mean annual ablation rate is ~1.25 m a^{-1} at KAN-M, thus given that we document weathered ice at depths > 1.5 m it is conceivable that near-surface, low density ice persists on interannual timescales.

2. (continued) You attribute ice lenses to remnant glacier ice but to me it seems that these are evidence of meltwater refreezing at depth. Can you correlate the incidences of ice lenses to e.g. the annual cycle or specific weather events?

Author response: As noted above, we do not think the ice lenses are refrozen meltwater but instead we think they are structural features, as suggested by reviewer #3. Further, based on similar analyses of meltwater lenses in firn we do not think the annual cycle or specific weather events will prove fruitful (Brown et al., 2011; Machguth et al., 2016).

3. While it is good that you discuss the implications of your findings for SMB and surface hydrology studies, I would like to see a bit more qualitative information here. For example, what would the difference in ice mass be in your catchment a) at the density of ice and b) when you account for sub-surface porosity?

Author response: The difference in ice mass in the catchment can be evaluated from the difference between solid ice density and in situ measured density integrated across the depth of porous ice:

$$\Delta m = \int_0^{h_o} \rho(h, t_o) \cdot dh \ - \ \int_0^{h_o} \rho(h, t_1) \cdot dh \qquad (1)$$

where m is mass, h is ice thickness, ρ is ice density, and t is time. Assuming $\rho(t_o)$ = 917 kg m^{-3}, integrating Eq. (1) across the depth-density profiles from our shallow cores yields Δm = 254 kg m^{-2} or 25.4 cm water equivalent. Integrating this across the 63.1 km^2 catchment yields 1.6×10^{10} kg or 0.016 Gt.

However, the mass of stored water must also be accounted for. Subtracting the 17.3±4.3 cm mass of stored water we document from the above estimate yields Δm = 81±43 kg m^{-2} or 8.1±4.3 cm water equivalent. Integrating this across the 63.1 km^2 catchment yields 5.11×10^9 kg or 0.0051±0.0027 Gt. The first estimate (0.016 Gt) can be considered a maximum plausible mass difference as it assumes there is no subsurface stored meltwater, whereas the latter (0.0051 Gt) estimate is a minimum based on the water storage we document. In reality the mass of stored meltwater is time-variant, thus we can provide a snapshot estimate at best.

The effect of subsurface porosity on mass, therefore, depends on the timescale considered, the initial conditions, and, critically, the role of meltwater drainage. We omitted this discussion from the original manuscript to keep it focused on the instantaneous characterization of density, porosity, storage, and weathering crust structure, owing to incomplete knowledge of the initial ice density profile, the density profile below the depth of the shallow cores, and the fate of the stored meltwater. We are happy to include the above exercise in the revised if requested.

2. (continued) Similarly, hydrological studies use estimates of snow permeability for water routing according to Darcy's Law. Can you provide an updated estimate of sub-surface permeability in your study area for future use by such studies?

Author response: We agree the results of our study point to the importance of sub-surface permeability but unfortunately, we cannot provide an estimate from our data. We do, however, discuss the topic in the discussion where we cite the four studies we are aware of that provide estimates of ice permeability, and we compare these to estimates of supraglacial channel flow velocities. Our hope is that an interested reader will use these citations as a resource for this topic.

Minor comments:

Page 1

Line 29-30: I'm not sure that meltwater throughput 'reinforces concerns about ... sea level rise'

Author response: The statement is removed, as requested.

Line 29: Greenland ice sheet not Greenland Ice Sheet.

Author response: We respectfully submit that our capitalization is correct for a proper noun referring to a geographical place name. Reviewer 3 also expressed a preference for Greenland Ice Sheet.

Page 2

Line 3: Mention that these models assume that runoff is instantaneously lost to sea here.

Author response: As requested, we have mentioned that these models assume runoff is instantaneously lost to sea. To supplement this, we have added reference to several works that demonstrate substantial time lags and possible meltwater retention in the ablation zone, motivating the study of ablation zone hydrologic processes and near-surface porous ice.

Line 8: Sentence structure is odd here; implies that the stored meltwater is the substrate. Is that what you mean?

Author response: Thank you for catching this error. The "weathering crust" is the substrate. As requested, "meltwater storage" has been deleted.

Line 16: Maybe mention melting due to friction from the flow of meltwater.

Author response: As requested, this additional source of melting has been noted, but we added this to the interpretation of the ice lenses in results Sect. 3.1.

Author changes in manuscript:

Line 31: ‘could potentially’ rather than ‘would’

Author response: ‘would’ has been changed to ‘could’, as requested.

<u>Page 3</u>

Line 2: Logical disconnect here, add an explanatory line.

Author response: We are not sure what logical disconnect the reviewer is referring to but the introduction has been substantially revised and we hope the problem has been corrected.

Line 6: Phrasing of ‘near surface ablating’ seems strange.

Author response: We have hyphenated ‘near-surface’ which we hope improves the phrasing. The phrasing is used to emphasize that the study is focused on bare, ablating ice, to avoid possible confusion about firn, snow, or superimposed ice.

Line 12: Add melt zones onto map in Fig 1

Author response: The entire area in Fig 1 was actively melting during the study period.

Line 16: Delete ‘study area’

Author response: deleted, as requested

Line 27: Example of logical jump; you assign and uncertainty then say where you got it. It would be better to say, ‘we consider 1.3 cm (10%) accuracy to be conservative’.

Author response: We have revised the sentence to read as requested.

Line 29: Just measurement uncertainty or a combination of measurement and instrument uncertainty?

Author response: Just measurement uncertainty. We used a pair of calipers and digital scale to make the measurements and we assume the instrumental uncertainty is substantially less than the measurement uncertainty. We have, however, improved our discussion of measurement uncertainty, specifically describing the 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 to an unknown extent. Estimates of temperate ice water content range from 0-9%, though most estimates (including all based on in situ calorimetric methods) are in the range 0-3% (Pettersson et al., 2004). Recognizing that both sources of error are poorly constrained, we think our original 10% estimate is sufficiently conservative without giving undue confidence to either the measurements or the error estimate. We have also added a physical constraint that density cannot exceed solid ice density (917 kg m^{-3}) and effective porosity cannot exceed total porosity $(1 - \rho_{\mathrm{m}}/\rho_{ice})$. These issues are presented in the revised methods Sect. 2.1 paragraph 3.

Page 4

Line 12: What determined the maximum depth?

Author response: The maximum depth was determined based on the 1 m drill barrel length. A drill extension is required to retrieve cores deeper than 1 m. We did not expect weathered ice to extend below 1 m depth, so we designed our field methods to remove 1 m cores using the standard drill barrel without any extensions. After drilling the first few cores, we realized the ice was weathered to at least 1 m depth, but owing to time limitations, and for consistency with the first few cores, we chose to drill each of the ten cores to 1 m depth. For additional context, we drilled two 1.8 m cores near camp (described in the text), but we were not able to systematically return to each core site and drill deeper.

Line 28: Be consistent with units; you used cm3 before.

Author response: The units have been changed, as requested.

Page 5

Line 6: Delete ‘these’

Author response: ‘these’ has been deleted, as requested

Line 7: Justify this given the difference in structure between the two layers.

Author response: Thank you for pointing out this important caveat to our study. We have added substantial discussion of structural differences between the two layers in the revised results Sect. 3.3 paragraphs 3 and 4.

Line 20: Nearest cryoconite hole? Nearest x cryoconite holes?

Author response: The nearest cryoconite hole within a 1 m radius of the posting. We have moved this description further up so the methods are clear to the reader right away.

Line 27: Another logical jump re: transition!

Author response: Thank you for pointing out the logical disconnect with respect to the unsaturated/saturated layer transition. 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. 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. We hope this addresses the logical disconnect.

We are also working with a graphics specialist in our department to design a conceptual diagram for the introduction that we hope will improve the visual communication of the weathering crust structure to the reader. The diagram will merge the conceptual diagram from Müller and Keeler, (1969) and the characteristic subsurface depth-density profile for weathering crust from LaChapelle, (1959):

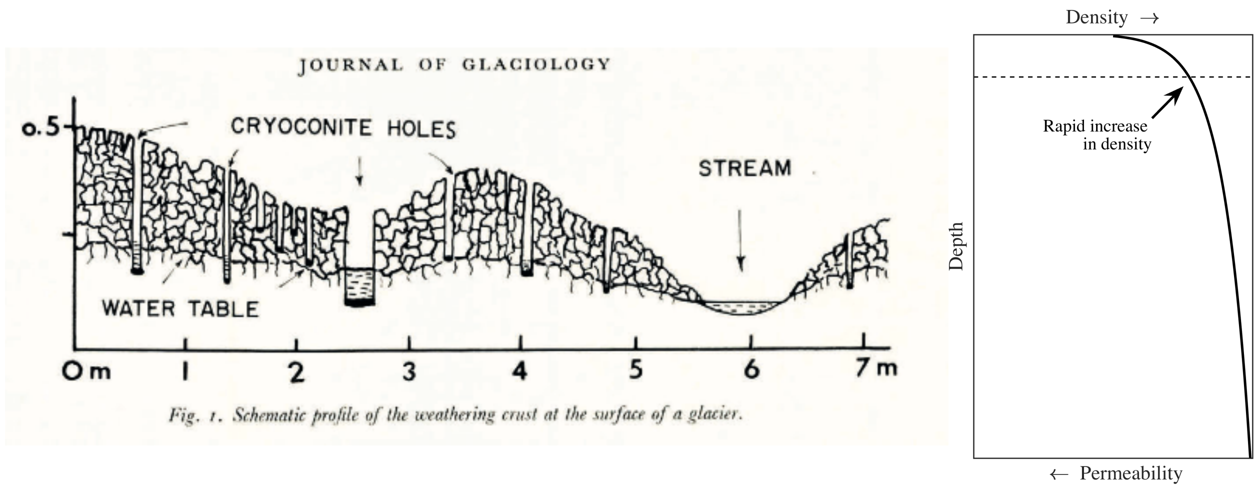

Figure 1: Conceptual schematic of the weathering crust (Müller and Keeler, 1969) and the ideal subsurface ice density curve for the near-surface of an ablating glacier, adapted from La Chapelle (1959).

In the text, we have clarified the following:

1) The unsaturated depth is inferred from the depth to water in cryoconite holes
2) The depth probe measurements are used as a qualitative description of weathering crust structure with reference to the sub-surface density profile and conceptual schematic
3) The apparent transition from very low density to higher density material is reported in the results

Line 30: To what height? It would have been good to measure the water table at the drilled holes as well as at Cryoconite holes.

Author response: We agree these measurements would have been good to measure, but we think the cryoconite hole water levels provide a better (non-destructive, equilibrium) estimate of the water table height. We were also severely limited by time. For example, it wasn't clear how much time was required for the water levels in the drilled holes to equilibrate. We planned to measure the water levels after all other science priorities were completed, but returning to (and locating) all 100 drilled holes was infeasible. Instead, we use the refilling of the drilled holes as a proxy for water saturation, and use the depth to water in the cryoconite holes as a measure of the water table height.

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Line 8: Impermeable yes, but how continuous? Are these highly localized features?

Author response: As noted above, we have substantially revised our interpretation of the ice lenses to suggest they are highly localized structural features.

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Line 7: I think the value below snow surface would be a better one to quote.

Author response: We agree and have added this value, as requested.

Line 10: Again, to what level? Completely full?

Author response: As with the 1 m drilled holes (N=100), the water levels are used here as an indication of sub surface saturation, and the depth to water in cryoconite holes is used as an estimate of the depth to saturation.

Line 17: Did you see any evidence of flow in any of the holes?

Author response: We did not test for evidence of flow in the holes, other than our observations of refilling, which implies subsurface permeability and flow.

Line 25: Quantify ‘often’

Author response: We agree this sentence is somewhat vague and unqualified and we have removed the sentence altogether. The observations were not systematic and hence should not be reported.

Line 29: Ok so the water table is ⇠20 cm below the surface yes? Which is consistent with your statement that the bottom layer, from 20cm down is saturated.

Author response: The water levels in cryoconite holes were on average 15 cm below the ice sheet surface, which is our best estimate of the water table height. We apologize for the confusion in the original manuscript, resulting from the depth probe measurements. We use the average 15 cm depth as an estimate of the depth to saturation.

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Line 29-30: This is speculative without seasonal data. If the aquifer is perennially saturated then this is not necessarily the case.

Author response: The average annual maximum snow depth in this region of Greenland is <1.0 m thus we think it is highly unlikely the aquifer is perennially saturated.

Fig 3, reorient so #10 is on the left as in fig1.

Author response: We have revised Figure 3 such that #10 is on the left, as requested. Additionally, as per a request from Reviewer 3, we have constructed continuous depth-density profiles by substituting the snow-cutter density measurements for the upper 20 cm at cores #1, 2, 4, 5, 9, and 10, and used linear interpolation to gap fill missing data between 20–30 cm depth for cores #1, 4, 5, and 9. The new figure is included below.

Fig 6 b add core depth.

Author response: Core depths have been added, as requested. The new figure is included below.

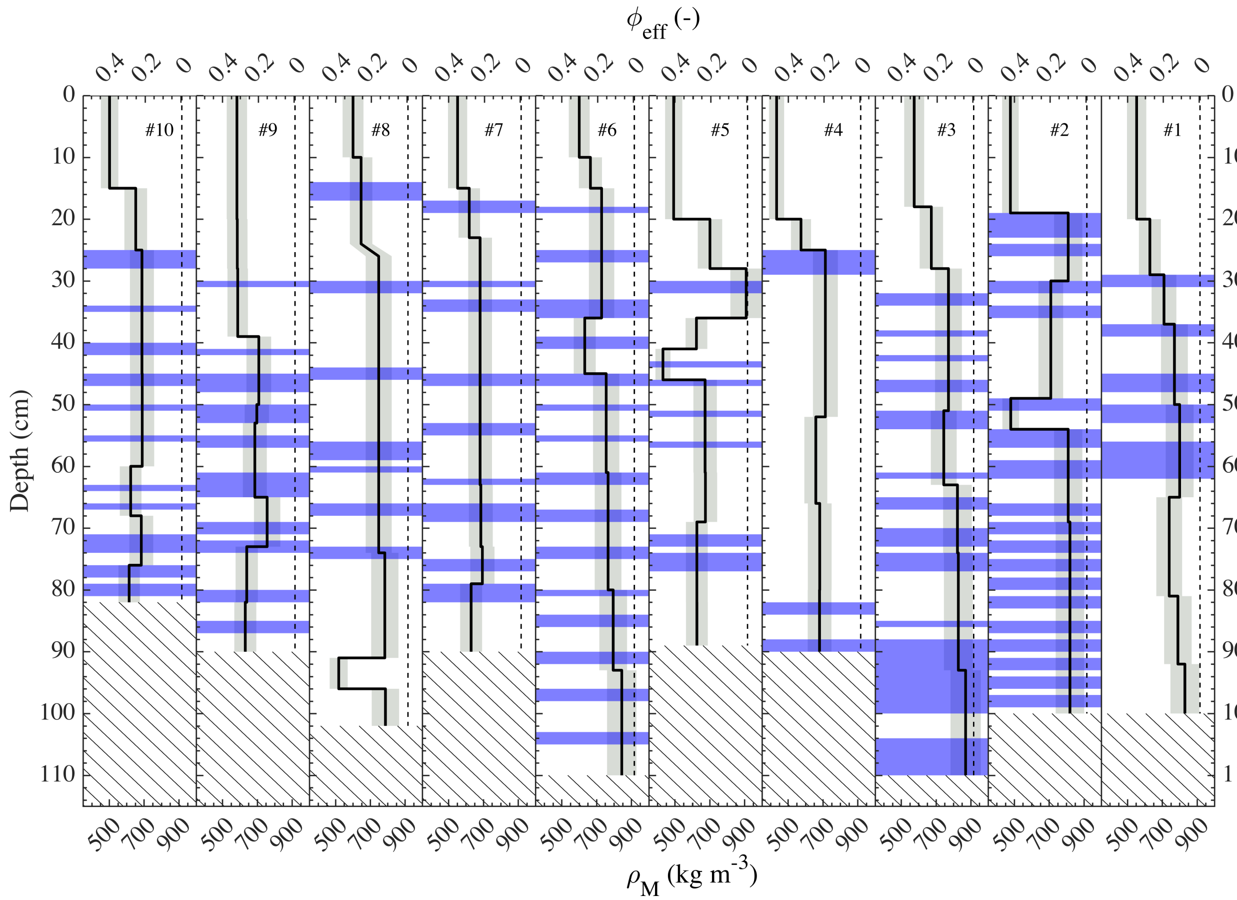

Figure 3: Subsurface measured ice density (ρ_M) and corresponding calculated effective porosity (ϕ_{eff}), and stratigraphy profiles from 10 shallow ice cores (#10-1, left to right) extracted at 80 m postings along the study transect (see Figure 1 for ice core locations). Horizontal blue shading represents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

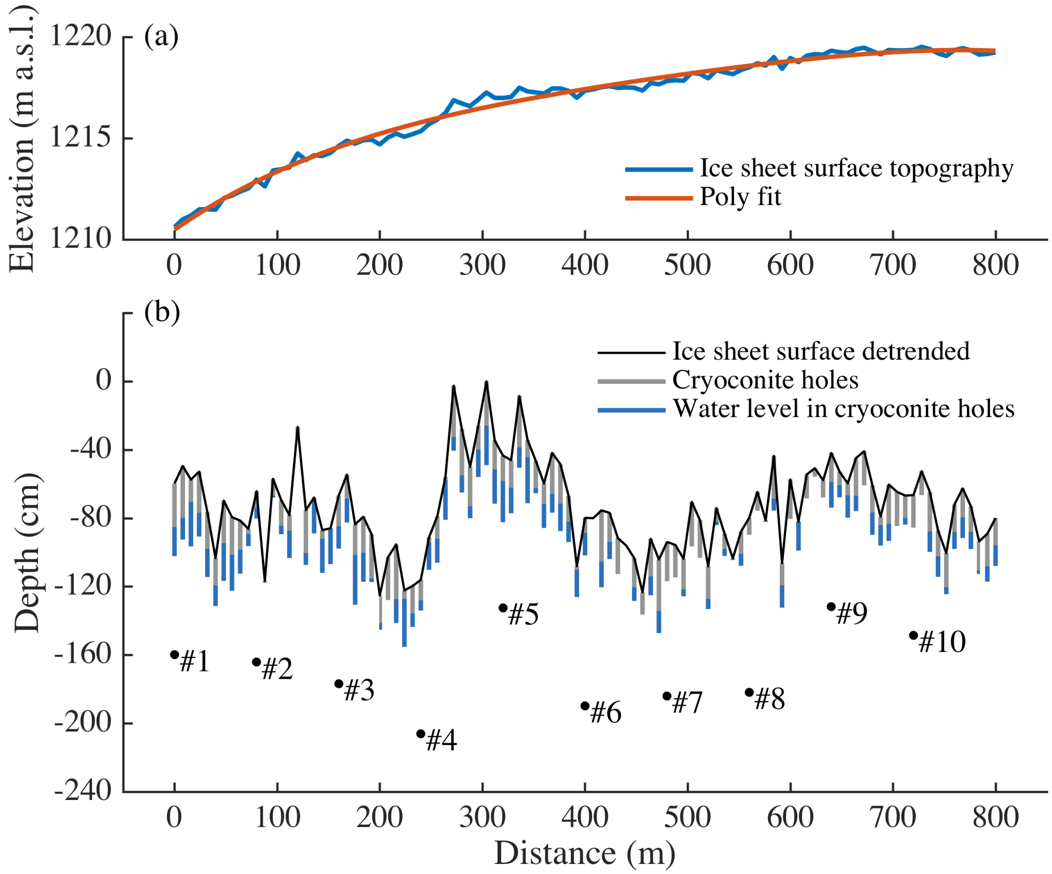

Fig. 6: (a) Ice sheet surface topography along the 800 m study transect extracted from a 6 cm posting stereo-photogrammetric digital elevation model derived from RGB imagery collected 10 July 2016 from a quad-copter drone and the 2nd-order polynomial best fit. (b) Ice sheet surface topography detrended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

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