Final Response: Seasonal changes of ice surface characteristics and productivity in the ablation zone of the Greenland Ice Sheet

D. Chandler, 9 July 2014

Interesting and detailed comments were received from each of the three reviewers. Point-by-point responses to each of these comments are provided below.

Response to Anonymous Reviewer #1

The four major issues raised by the reviewer are as follows.

1. Up-scaling from measurement scale (cm to m) to satellite image scale (km).

It is true that we are correlating processes measured manually (centimetre to metre scale) with remote sensing data (kilometre scale). Contrasting scales between different data collection techniques is a common problem, and since we cannot control the resolution of the remote sensing data we have to design the field experiment accordingly. In our case, we used averages obtained from five 9 m^2 quadrats distributed within a region of c. 40 m × 50 m to reduce the impact of local effects on the observations. Frequent travel over a wider area (up to 2 km from the field site) revealed very few qualitative differences in surface characteristics and we are confident our site is representative. Despite this there were still some notable differences between quadrats; these differences are represented by the error bars used in the plots (e.g., Fig. 6). The rates of biological productivity also varied widely between ice surface types, and extrapolation to larger scales was achieved by measuring productivity within specific surface types and multiplying by the fractional coverage of that type. What we have not done is to measure productivity in a given surface type at sites separated by several kilometres. That would not have been feasible in 2012 and would require a much larger scale field campaign to achieve both the greater areal coverage at the same temporal resolution.

2. The correlation between surface albedo and productivity. The reviewer argues that it isn't physically reasonable to correlate albedo (a function of total carbon storage) with productivity (a rate of change of carbon storage).

In some ways the rate of change of carbon storage by biological processes could be of more interest than the total storage, given the climatic applications of the carbon cycle. Any correlation that leads to a predictive capability for carbon fluxes is therefore of interest. However, we note that albedo is not a measure only of the total carbon storage. Instead, it is a combined measure of many factors which do include stored carbon (e.g. organic matter in cryoconite debris) but also include inorganic material and surface type (e.g., glacier ice, snow, water etc).

Correlation statistics for productivity vs. albedo in Periods 3&4 can be provided and will help the discussion.

3. Lack of accompanying meteorological data.

We agree that more detailed meteorological data would greatly help the discussion. Short-wave radiation data used in the paper were obtained from an IMAU meteorological station close to the field site, and other relevant observations from this station (now available online) could easily be added to the revised paper.

4. Ambiguity regarding units of ablation: direct surface level change or water equivalent.

Ablation in the field was measured using change in surface level. Conversion to the more widely used water equivalent is straight forward if the ice density is known. However, the ice density at the surface is quite variable due to the effects of solar melting, which creates a low-density surface layer. Therefore, the mass ablation (mm water equivalent) is very difficult to measure accurately. This can be discussed further in the revised paper.

Other comments.

1) The correlation holds for the combined early and main melt seasons (Periods 3 and 4). Most of the changes that allow the correlation to be tested occur in the early melt season, there were only much smaller changes in albedo and cryoconite hole coverage in the main melt season. This can be made clearer in the discussion.
2) In this context we mean the productivity of that specific surface type rather than its areal contribution but yes the use of that term does need to be used more carefully.
3) Here it is the albedo of the holes that is being modified but this does of course alter the overall surface albedo, by an amount dependent on the fractional coverage of holes. This can be explained more clearly.
4) The reference list can be amended to include this citation.
5) The field site was located within the dark band, albeit quite close to its western edge, so any differences between the surface characteristics of the dark band and those of the field site are likely to be minor.
6) Yes it would be better to express as water equivalent but this would be misleading since only the surface elevation change, not the elevation change and density, was measured.
7) The bottles were completely filled each time, so the volume was always 250 ml.
8) We have used these units initially because they represent what is actually being measured in the experiment. However, we agree they are not the most useful, therefore we have converted these values from volumetric units to areal units, see pg 1347.
9) Yes we will add further meteorological data to the plots.
10) We are referring to observations of melt crust made in this study (see images in Fig 8). The weathered crust has been documented previously, as the reviewer notes, but some of the specific structures (such as the ice mushrooms) have not been mentioned in these previous studies.
11) Yes this should be storage rate.
12) This is a typo, the sentence should read "There was no significant correlation in activity between any two pairs of surface types..." which is hopefully less ambiguous.
13) Yes the hole depth could be important but in practice this is hard to measure. Holes are typically encountered in rough, sloping ices surfaces such that the hole top is difficult to identify, the hole bases are also often sloping, and the water level relative to the ice surface can vary on a day-to-day basis depending on the melt crust thickness.
14) The observation was made by the authors, this will be noted more clearly.
15) If we can obtain the precipitation data we will present it here.
16) Similarly, we will add more meteorological data to help the discussion.
17) We mean cryoconite hole coverage.
18) During sunny conditions, the hole bases melt downwards due to solar heating of the debris in the hole base. At the same time the surface is melting downwards due to solar and/or sensible heat flux. Debris in shallow holes receive more solar radiation than debris in deep holes, therefore a balance is reached in which both the surface and base melt down at similar rates, thereby maintaining the hole depth. In warm cloudy conditions there is much less solar radiation, so the rate of melting at hole bases becomes smaller. Meanwhile the surface melt continues (via sensible heat fluxes rather than radiative melting), and eventually the ice surface reaches the hole base, at which point the debris from the hole can become dispersed locally across the surface. We observed this process during our stay on the ice but are not aware of a reference.
19) We don't know this for certain, the explanation is just one possibility, we can highlight this uncertainty more clearly.

20) Yes we will add met data to support these arguments.
21) It is not just the snow melt that decreases the albedo, it is also the melting of superimposed ice. The melting snow leaves a clean, bright ice surface which takes a few weeks to evolve into the relatively dirty surface encountered in August. In some cases (pg 1356 L5) the decreasing albedo has been attributed to increasing surface water. We have shown that was not the case at this site; indeed there was a decreasing surface area of pooled water as the surface drainage system became clear of slush. This has been explained on page1356-1357.
22) Yes, hole depth needs adding to this list of other factors.
23) The AVHRR albedo responds to both snow melt and cryoconite hole exposure. The evidence is as follows, see Fig 6. In Period 2 and early in Period 3, the observed albedo decreases as snow melts, before the emergence of cryoconite holes (so AVHRR albedo decreases before any holes are exposed). Later in Period 3, there was no snow but the albedo continued to decrease; during this time the cryoconite hole coverage increased (AVHRR albedo decreases when there is no snow). In contrast there was little apparent link between albedo and DI coverage since in late July / early August the strong spikes in DI coverage did not lead to any noticeable response in the albedo. However, these periods of high DI coverage occurred mainly during cloudy conditions when AVHRR albedo is not available so there is likely some bias here that would warrant more thorough investigation in future work.
24) We already explain that any relationship between albedo and C flux would have a large uncertainty, see page 1360 L19-21. Nevertheless, this would be a worthwhile objective provided the uncertainty was properly quantified. There have been several previous attempts to estimate the ice sheet’s contribution to C fluxes based on just a few point measurements; the wide variability in fluxes presented in this study shows that such extrapolation from a small data set is not an appropriate method. Combining the existing field data with remote sensing data could, however, yield estimates with less uncertainty.
25) Yes we can revise this sentence so it is less ambiguous.
26) If the authors of the papers have stated the hole sizes we will add them to Table 1.
27) C storage can be changed to C storage rate as requested. The total C flux is calculated by integrating the time-varying areal coverages and productivities, therefore a single value of fractional coverage would not be appropriate here.
28) Fig 4 includes images annotated with the 5 surface types encountered in the quadrat survey. We could add an image of a crevasse but we don’t think this is necessary since they were not present at the field site.
29) Specific dimensions of the various features were not measured accurately. The lake bed in the foreground was approximately 5-10 m across and the spikes were of order 0.1 m in height.
30) We can add an inset to indicate the field site’s location in Greenland.
31) Approximate hole depths can be added to the caption.
32) See the above comment on water equivalent (Major comment #4)
33) Yes, CH should be CS.
34) The areal proportions are presented in Fig. 6.
35) Yes the photographs do take up some considerable space. However, since a qualitative description of the surface characteristics forms an important aspect of the paper we would prefer to keep the images in the main paper.

Response to Anonymous Reviewer #2

The reviewer suggests the conclusions are limited and firstly asks 2 questions:

(1). *“Is the key question not how evolves the dark zone over the season and should you not discuss this as such?”*

Yes, we are effectively monitoring what is happening in the so-called dark zone. In the brief field site description we do mention that our site is towards the edge of the dark zone (P1344, L4). We could reiterate this in the conclusions so it is clearer to readers where on the ice sheet our results are relevant.

(2) *"I am a little confused whether your goal is to get information about albedo characteristics or about biological production"*

Our goal was to address both issues. Also we have attempted to link albedo and productivity with the aim of finding a way of estimating productivity remotely by means of albedo, which would solve the existing problem of productivity estimates being very sparsely distributed in time and space (not that this problem has prevented previous attempts at extrapolation to ice sheet scale). The link is quite weak at present but is worth pursuing in future.

Minor remarks. Page and line numbers correspond to the list in the Reviewer's comments.

P1339 L1-10

The van de Wal and Oerlemans citation can be added here and also later in the paper where we refer to the dark band.

P1339 L15

Yes, it is true that there are now several data sets available based on AWSs and we can cite some of these in the introduction. We also intend to make further use of AWS data collected close to the field site, as outlined in the Response to Reviewer #1 above.

P1340 L3

Yes we can clarify this sentence as being the longest record of manual observations (both productivity and surface characteristics). AWSs have collected longer records of albedo and meteorological variables.

P1341 L19

Yes this should be rephrased.

P1343 L20-24

We can edit the introduction to remove repetitive sentences.

P1348

We can include AWS albedo as an interesting comparison with AVHRR albedo. However, the footprint of AWS albedo instruments is very small in comparison with that of the remote sensing instruments, which could be a significant drawback when considering the very spatially variable surface characteristics presented here.

P1349 L16-21

We intend to make greater use of the AWS6 data as indicated above and yes we can incorporate the van der Wal et al. (2012) paper regarding the exceptional summer conditions in 2012.

P1351

Figs 1,2,4,8 together illustrate the very varied surface conditions encountered on this part of the ice sheet, also they may provide clues as to the important processes occurring at the surface. If space permits we would prefer to keep these figures in the main text.

P1351

We will double check to make sure abbreviations have been properly defined.

P1352

Are you referring to figures in L3-5? If so these are for the ratio of stdev/mean, not the mean itself, so the high value quoted for CW is indicative of the high level of variability recorded relative to the mean. This primarily reflects the very small mean value.

P1355 L17-20; P1356 L6.

Yes, the site is well below the equilibrium line. When referring to superimposed ice we mean ice that has formed in the current spring as a result of downwards percolation of early melting of the snowpack. We assume that none of this ice survives longer than one season, given the site's position well into the ablation zone. Also, to avoid confusion: in our description it is the cryoconite holes that are re-emerging from under the superimposed ice, rather than the re-emergence of superimposed ice.

Subsurface radiation penetration creates the formations illustrated in Fig. 8.

P1356 L20 & L26

Yes we will add more AWS data as mentioned earlier, which could include albedo for comparison with AVHRR but noting the difference in footprint area between AWS albedo and AVHRR albedo, with the former being very small when considering the strongly variable local surface characteristics over length scales of 0.1-100 m.

P1360 L10

It isn't possible to say for certain why there was such a great difference, it could be one or more of several factors. Importantly, what we can conclude is that there is great spatial variability in measured productivity – this should be noted when attempting to extrapolate small, localised data sets up to ice-sheet scale.

Response to Reviewer #3 (A. Hodson)

1. Surface roughness

Since we did not make any measurements of surface roughness, we will reduce discussion of roughness rather than expand on it. It would make an interesting future study, since there did appear to be marked changes at a range of length scales (1 cm – 10 m) through the season; in particular the change from a smooth early season ice surface to very rough late season ice surface was observed at several sites in this part of the ice sheet during field visits in 2011 and 2012.

2. Superimposed ice as distinct surface type

From a biological aspect, yes it would be better to distinguish between superimposed ice and englacial ice since they may well contain different abundances and/or species of micro-organisms. In practice it would have been difficult to confidently classify the ice surface in the field and in some cases they may even be mixed (for example, in a layer ~10 cm thick where spring snow melt has penetrated the previous summer's permeable melt crust and then refrozen). Sometimes there are clues in the shape of the crystal remnants but again this could be uncertain/subjective. Therefore, we do not make the distinction between these ice types.

Hole depth: we did not systematically record hole depth. For small holes of a few cm diameter or smaller this would have been feasible, but for larger holes there was the problem of sloping hole bases and a rough ice surface which could potentially introduce errors of several cm. In the incubations there are actually two relevant depth measurements – one for the debris origin and one for the incubation depth. The latter was always sufficient to fully submerge the bottles, typically 15-30 cm. Again, because of the sloping hole bases, not every bottle was incubated at exactly the same depth. Also the shading by the ice walls or other bottles would have varied between bottles and between incubations. These effects will have contributed to some of the scatter and are very hard to control properly in in-situ installations. We used triplicate sampling to help reduce the effects of variance arising from uncontrolled factors, we would have liked to use more bottles but were limited by logistical constraints. We carried out additional incubations using 24 bottles of the same type (2 light & 12 dark) as explained in the paper, to provide a better estimate of variance than that obtained with just 3.

3. Hole characteristics where samples were collected

See the previous comment regarding hole depths. I am not clear how the reviewer would have used hole area to derive sediment thickness in the field even if all the sediment were to be collected from the hole. We would have had to have filtered, dried and weighed the sediment (tricky in a field camp on the ice) then assumed a sediment density representative of the sediment in its in-situ state to calculate sediment volume and subsequently thickness. We do recognise that the sediment thickness is important, which was the reason for carefully sampling the sediments such that the sampled area matched that of the incubation bottle (thereby preserving the correct sediment thickness in the bottle). So, even though we do not know the thickness, we are confident that the incubations were carried out using the correct sediment thickness. Any variations in productivity arising from thickness changes will be a further contribution to the scatter on top of other unknown factors above (e.g. hole depth) as well as environmental factors such as ice lids, water residence time, and solar radiation intensity.

4. Reliance on a single technique

Yes, we agree there are better approaches than using changes dissolved O_2 alone for assessing productivity – such as combined DIC/O_2, as suggested by the reviewer. Hopefully, future studies of this type will be able to use these multiple analysis methods to obtain a clearer picture of what is causing the variations in productivity we have observed as well as reducing potential uncertainties arising from method-related artefacts.

5. Cryoconite hole photography to estimate debris cover

Cryoconite holes become very difficult to see when not viewed from directly above, it is not clear to me how field notes could help determine debris cover from photographs. If the camera is positioned such that it is looking vertically downwards, field measurements of hole geometry (diameter and depth) could in theory enable an assessment of whereabouts in the frame the hole debris are fully visible, but in practice this would be difficult because of the great variety of hole geometries in any one frame. This is best illustrated by Fig. 4, where many hole tops can be observed but very few complete hole bases. The patchy nature of debris cover in some holes (Fig 4b, 4c) suggests that using ‘hole top’ area as an estimate of ‘debris covered hole base’ area may not be appropriate.

6. Under-saturation of O_2 in initial O_2 measurements

The melted ice samples (CI, DI) were under-saturated with O_2, while the liquid water samples (CH, CW) were close to saturation, so the reason most likely lies in the method used to melt the samples. The bags used to melt the ice samples were sealed, so perhaps there was insufficient O_2 in the bag headspace to fully saturate the water that formed from the melted ice.

7. Inter-annual persistence of cryoconite holes

It is true that we did not specifically mark holes in the autumn in order to observe their reappearance (or not) the following season. However, the thick ice lids prior to emergence of holes early in the season, and the characteristic radial crystal structure above the holes, was strongly suggestive of holes that froze in the previous autumn and were becoming reactivated. The sentence in question can be toned down ("suggests that..." instead of "shows that..").

8. Comparison with Svalbard

Yes, it is interesting to compare this site with the Reviewer's field sites in Svalbard. Our field site has a much more continental climate than Svalbard, and there was no apparent mobilisation of debris after the superimposed ice had melted – following snow melt, the superimposed ice melted to reveal cryoconite holes that were already well developed. Later in the season there were some major melt events as discussed in the paper, these were more typical of maritime conditions (warm, moist and cloudy) and did lead to mobilisation of debris from the shallower holes.

9. Incubation conditions

Use of controlled chambers would be much more preferable to using melted ice. Given the very small changes in gas concentrations, chambers would likely have introduced a different range of problems with our dissolved gas approach – for example, the release of gas from bubbles in the melting glacier ice, and the effect of disturbance needed to create a water-tight, gas-tight seal around chambers constructed on a porous, melting ice surface. To reduce uncertainties, multiple incubation and analysis techniques should be tried simultaneously to help quantify how dependent the productivity observations are on the chosen method.

10. Section 4.2

We can edit this section to make it clear that although there is a clear link between remotely sensed albedo and ground-based surface observations, the link is based only on one site and one season so therefore needs further verification.

11. Late season light levels

Yes, we can make this comparison with Hodson et al. (2010). The autumn productivity would have been great to measure for completeness, it is a shame we had to leave before the true end of the season. Activity levels in darker conditions (e.g., under snow cover or during winter darkness) are perhaps indicated by the dark bottle incubations. It is also worth noting that the 'dark' season at this site is probably relatively short, owing to the low precipitation (only a thin winter snow cover, which blocks less incoming solar radiation than a thick snow pack) and relatively low latitude (no extended periods of permanent darkness, unlike locations further north in Greenland). Leaving some radiometers measuring incoming shortwave into cryoconite holes through the winter and subsequent spring would be interesting in this respect. There are of course other considerations besides light intensity, for example the hydrological connectivity between holes and the release of nutrients from melting ice would presumably both become largely absent during winter.

12. Section 4.4

Section 4.4 considers the link between cryoconite hole coverage and albedo, not just the dirty ice coverage. Yes, the warm cloudy conditions did cause the melt-out of cryoconite holes, and it is indeed a shame that the remotely sensed albedo cannot be used during these times. These periods contributed a relatively small fraction of the melt season so the approach is still potentially useful for assessing overall seasonal productivity even if not for certain specific events.

13. Difference between first Period P1 and second Period P1

P1 was defined by its high snow cover and consequently high albedo. Therefore, the two periods identified as P1 in Fig. 6 would have been quite similar, rather than distinct (note the similar albedo values in Fig. 6g). Assuming the seasonal pattern is persistent then the first P1 and second P1 are not separate events, because P1 would extend from autumn to the subsequent spring.