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Interactive comment on “Numerical simulation of extreme snow melt observed at the SIGMA-A site, northwest Greenland, during summer 2012” by M. Niwano et al.

Anonymous Referee #2

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General Comment:

It is a challenge to obtain high quality meteorological observations on the Greenland ice sheet, which the authors of this paper have achieved and should be congratulated for. Surface observations and modelling are used to characterize the surface energy balance and melt at a site in north-western Greenland, at an elevation of 1490 m a.s.l. The observations described are over a two week period, including the unprecedented event where widespread melt was observed over most of the Greenland ice sheet. The research is of interest as the atmospheric processes controlling this extreme melt event have not previously been described at this site. The measurements and modelling approach used in this research are described carefully and it is the view of this reviewer

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that the manuscript should be considered for publication. The comments provided below are intended to provide the authors with some feedback that they may wish to consider should the paper be considered for publication in *The Cryosphere*.

Specific comments:

Please note that page number is referred to as (P) and line number is referred to as (L).

1. P496, L7-9 and L20-21: The abstract is well written and provides a clear framework of the paper. Two small comments that the authors may wish to consider. Firstly, the authors comment that 100 mm of rain fell during a “remarkable” melt event in the abstract. It would be of interest if the authors could provide more information in the site description (Section 2) about the long term climatology of the site, and whether “continuous” rainfall is an unusual event in summer at this location before making this statement in the abstract. Secondly, the assertion that two-level atmospheric profiles are “needed” to obtain realistic latent heat fluxes needs to be constrained if kept, to state that “in this study” it was found to be useful. Not enough evidence has been shown to suggest it should be widely adopted (further comments below).

2. P498, L1-11: The authors may wish to consider providing an additional paragraph or replace paragraph two, which is quite general, with some of the key energy balance studies that have been carried out on the Greenland ice sheet margin, and/or in the interior. This might provide further context for readers about the expected radiative forcing due to clouds and the typical direction and magnitude of the turbulent heat fluxes. The controls on melt have been studied on the western margin of the Greenland ice sheet, so further justification and importance of the proposed research could be useful here.

3. P500, L6-17: I am confident that the measurements are of a high quality but given the emphasis on determining gradients of wind speed, temperature and moisture in this paper I think it is necessary to clearly state the accuracy and/or precision of the

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RM Young (wind), HMP155 (temperature and relative humidity) instruments. Was a relative calibration of the instruments performed in the field or before or after deployment? If so, what was the precision of the instruments at the two heights? It would be useful to carefully demonstrate in this section that the instruments do allow gradients of wind speed, temperature and moisture to be resolved, before calculating turbulent heat fluxes from the two level method. Also, I would include the sampling rate of the instruments – averaging intervals are provided but sampling rates are not.

4. P498, L1-11: It is common to apply a procedure to recalculate relative humidity data to account for saturation with respect to ice rather than liquid water (e.g. Box and Steffen, 2001). Was this correction attempted? If not, the authors may wish to comment on whether they think such a correction would or wouldn't have an impact on the absolute humidity values used to calculate the latent heat flux.

5. P500, L4-24: The description of the meteorological conditions in this section is of interest, but before presenting data from the measurement period it might be of useful to have further context about the background long-term climatological conditions at the site (see point 1). Prior to the “exceptional” melt event, were conditions typical for this elevation and latitude? A climatological context for the measurements would provide a broader context for readers.

6. P502, L1-12 and L22-25: The authors should consider providing a precipitation normal for the site, which may help explain the discrepancy between the reanalysis and bucket rain gauge. The near surface layer (NSL) was 88 cm – is this the accumulation over the last 12-months? This needs clarification. Also, it is this referee's understanding that snow temperatures obtained from snow pit measurements were used to initialize and then validate SMAP. It appears that observations were taken on 12 days (June 30 to 13 July, except for 11 and 12 July). Are the authors confident that the RMSE calculated in Table 1 has sufficient samples to be meaningful? It might be useful to confirm to readers how many in situ measurements were available for model comparison.

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7. P506, L1-2: The significance of surface roughness lengths is discussed at length in this paper in relation to their control on the turbulent heat fluxes. It appears that the stability functions are calculated using a Richardson Number, and that an upper bound of 0.1 was set. How influential was this decision compared to changing the magnitude of the surface roughness lengths?

8. P508, L21-23: How was the NSL simulated by the model adjusted to the measured depth? It is not clear how this was done, and a comment on the reasons for any discrepancy might be useful to readers.

9. P509, L18-23: The emissivity chosen was 0.98, which is lower than the values chosen in other studies over the Greenland ice sheet, where unity has been assumed (e.g. van den Broeke et al., 2008). Was the same emissivity used in the model? If the measured snow surface temperature had been calculated assuming an emissivity of 1 would the offset between SMAP and observed surface temperature would have been larger? Bottom line: are the authors satisfied that the emissivity chosen is not affecting the calculation of the latent heat flux using the 1D method? Could this help explain the failure to detect deposition events (Section 4.3)? Also, Figure 7 appears to indicate that after 10 July both model and measured snow temperatures were constantly at melting point – is this the case?, the lines are hard to detect.

10. P511, L16-18 and P512, L6-12: The measurement and modelling of near-infrared radiation is very interesting and is often not explicitly treated in energy balance modelling studies. The variability of the surface albedo around 4 July and 10 July is quite significant, and it is impressive how model and measurements agree (Figure 9). The explanation for this variability is linked to near-infrared, UV-visible and diffuse fractions of downward shortwave radiation. The authors could consider placing a little more emphasis on this finding, as it is an interesting result. A more detailed explanation about the physical processes controlling changes in snow albedo on the temporal scale shown in Figure 9 would be insightful for readers.

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11. Section 5: To improve this analysis it might be useful for the authors to present temperature and moisture gradients (surface and two levels in the atmosphere) to determine from the outset what the fundamental difference is between comparing surface-atmosphere and atmosphere at two levels. This could also aid the authors in addressing point 3 – the uncertainty of the instruments. In this context it should be noted that Box and Steffen (2001) had good agreement in determining the sign and magnitude of the latent heat flux using the 1D and 2D methods at low elevations on the Greenland ice sheet but greater uncertainty existed at higher elevations (sign often changed – see Table 6; for further discussion see Cullen et al., 2014). Though the authors focus on changing the magnitude of the surface roughness values (pg. 514), this should only have the effect of increasing or decreasing the magnitude of the latent heat flux, not the sign (direction), which appears to be the issue (not resolving deposition events). The discussion on pg. 515 could be re-focused if the atmospheric controls on the gradients of moisture and temperature are resolved more clearly.

12. P516, L1-9: Please clarify how the energy available for melt is treated in the model and in equation 6. In line with point 11 it would seem more logical to calculate both the turbulent heat fluxes in a consistent manner (either 1D or 2D but not a mixture of the two).

13. P517, L7-29: The energy balance during melting resembles what has been observed in the ablation areas of Norway's glaciers, and other mid-latitude glaciers (e.g. Giesen et al., 2009; 2014). The authors might wish to make this linkage rather than just referring to the Bennartz et al. (2013) publication.

14. P518-520: The statement that the 2D method is “preferable” over the 1D method to calculate the latent heat flux (P519, L26-29, P520, L1-2) requires more evidence before it can be suggested for use more broadly (see point 1). As indicated in point 11, an analysis of temperature and moisture gradients might be a useful way to clarify to readers why the 1D method is not reproducing deposition events.

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Technical corrections

P498, L5-L6: present tense could be used – “these fluxes are defined to be positive when they are directed P501, L1: these data could be used rather than “this” data P507, L8: we calculated the temporal evolution – add “the” P507, L12: resolution for an Arctic snowpack – add “an”

References

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