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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.

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General Comments

This paper studies surface energy balance and melt at a site in northwest Greenland at a time (July 2012) when record levels of surface melt were occurring across the Greenland Ice Sheet. The surface energy balance (SEB) at the site is characterised using measurements from an automatic weather station (AWS) and corresponding surface melt rates are then calculated. SEB and melt rates are contrasted between the early part of the study period and the latter part, when higher temperatures and enhanced melt were observed. Analysis of the SEB component by component shows

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that the enhanced melt was associated with increased downwelling longwave radiation (associated with warm, cloudy conditions) together with enhanced downward turbulent fluxes of sensible and latent heat. The measurements made are clearly of good quality and they have been analysed in an appropriate manner, providing significant insight into the drivers of enhanced surface melt at this site. I believe that the paper is suitable for publication in The Cryosphere once the authors have attended to the general comments given below and the specific comments that I have made on an annotated copy of the manuscript.

1.) Manuscript p.8. There is a large discrepancy (nearly a factor of 5) between the precipitation recorded at the site by bucket measurement and the precipitation diagnosed from the reanalysis. In the paper this is simply accounted for by applying a scale factor to the reanalysis precipitation. While one could argue that going further than this is outside the scope of the paper, I think that the authors should discuss possible reasons for the discrepancy. Is this a very local effect (in space and/or time)? I suspect that it may be, because reanalyses appear to reproduce observations of surface mass balance across the Greenland Ice Sheet quite well on longer timescales (e.g. Chen et al., Adv. Atmos. Sci., 2011).

2.) The authors argue strongly in favour of using 2-level measurements instead of single-level (plus surface) measurements for calculating latent heat flux. The basis of their argument seems to be that single-level measurements fail to generate the large, downward latent heat fluxes that would be associated with surface hoar deposition. This leads them to claim (MS. p. 25) that "...the 2LM method was an effective way to obtain an accurate HL.". Without an independent (eddy covariance) measurement of HL, it is not possible to substantiate this statement. Furthermore, the single-level method was used for sensible heat flux so the values for the two fluxes will be inconsistent. The advantage of the single-level method is that it uses a minimal set of measurements and produces a consistent set of calculated turbulent fluxes and surface temperature. The 2-level method involves measuring the (often small) differences in temperature and

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mixing ratio between two measurement levels. There will be measurement uncertainties in both temperature (at least 0.1K, probably greater) and relative humidity (at least 5%) at both levels, leading to a large relative uncertainty in the differences and hence the calculated fluxes. The authors should carry out an error analysis to determine the impact of realistic measurement uncertainties on their calculated fluxes. They could then formally determine whether the results of the 2-level calculation differed significantly from those of the single-level calculation. Some discussion of why the two methods produce HL values of opposite signs would also be useful.

3.) It would be useful to put the observations into the context of the prevailing synoptic meteorology during the observing period. What were the meteorological conditions that led to the higher temperatures and enhanced cloud cover during the latter part of the IOP? Were these conditions exceptional in the context of the long-term climatology?

Specific comments and technical corrections

Please see the attached annotated version of the MS.

Please also note the supplement to this comment:

<http://www.the-cryosphere-discuss.net/9/C107/2015/tcd-9-C107-2015-supplement.pdf>

Interactive comment on The Cryosphere Discuss., 9, 495, 2015.

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