Reply to Reviewer #2

We are deeply grateful to the reviewer for taking the time to provide quite valuable comments and suggestions. Below we describe our responses (in black text) point-by-point to each comment (in blue text). In addition we indicate revisions in the updated manuscript by a yellow highlighter together with line numbers in the manuscript. Please also refer to the revised marked-up manuscript uploaded as a supplementary file. In the manuscript, change logs can be tracked.

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 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.

We would like to thank the reviewer for this encouraging evaluation. First, we summarize important revisions, which were conducted considering valuable comments from both reviewers, in the updated manuscript.

Major revisions:

It was pointed out that observed relative humidity with respect to water should be corrected after it is converted to relative humidity with respect to ice. We agree with this point, and performed the correction that the reviewer suggested. Accordingly, input data for the SMAP model was modified and recalculations were performed. However, the results of this paper did not change significantly, because the values of relative humidity with respect to water and ice are almost the same under the condition that air temperature is around 0 °C. Related with this, scores indicating model performance, values of SEB fluxes, and some figures are changed slightly:

(**<u>P17, L21</u>**) 0.53 °C → 0.58 °C

(**<u>P17, L24</u>**) RMSE = 0.85 °C → RMSE = 0.94 °C

(**<u>P17, L25</u>**) ME = 0.55 °C → ME = 0.68 °C

(**<u>P18, L16</u>**) 0.16 → 0.17

(**<u>P22, L3</u>**) 15.2 → 15.3

(**<u>P22, L3</u>**) 11.2 → 11.3

(**<u>P22, L3</u>**) -13.2 → -18.0

(**<u>P22, L3</u>**) 17.8 → 7.2

(**<u>P22, L4</u>**) +31.0 → +25.2

(**<u>P22, L5</u>**) 102.5 → 96.7

(**<u>P22, L6</u>**) 55.0 → 49.1

(**<u>P22, L6</u>**) 24.9 to 79.9 → 20.1 to 69.2

(**<u>P29, L22</u>**) +31.0 → +25.2

<u>Table1</u>

<u>Figures 6, 7, 8, 9, 10, 11, and 12</u>

Both reviewers pointed out that error analysis investigating the significance of latent heat flux calculated from the 2LM method (air temperature, relative humidity, and wind speed at two profiles are employed to calculate turbulent heat flux at the surface) is necessary. It is because accuracy of air temperature, relative humidity, and wind speed sensors affect calculated latent heat flux with the 2LM method. In the original manuscript, we only investigated uncertainties induced by the snow surface roughness length for momentum. Now, we have completely understood and agreed with that it is insufficient, because it does not change the sign of latent heat flux but only modified its order. In addition, both reviewers commented that the turbulent heat fluxes in the presentation of SEB should be calculated in a consistent manner (only 1LM or 2LM). Based on this consideration, we have reconstructed the paper especially after model evaluation section. Until Sect. 4, basic construction is as same as the original manuscript. After Sect. 5, we first present calculated SEB characteristics, where the turbulent heat fluxes are calculated employing only the 1LM method. Next, we discuss validity of the obtained SEB from various aspects in Sect. 6 “Discussion”. In Sect. 6, we begin with investigating the effects of model setting on the SEB calculation (Sect. 6.1). In this subsection, we discussed impacts of the choices of snow surface roughness length and emissivity on the calculated SEB characteristics. The original discussion on the effects of snow surface roughness length has been reconstructed there (basic flow of discussion is as same as the original manuscript, but several corrections of sentences are performed). The motivation of investigating effects of emissivity on the SEB calculation was the comment by the reviewer regarding the validity of the choice of this value (comment on “9. P509, L18-23”). Finally, in Sect. 6.2 (reconstructed from the original Sect. 5 with some minor corrections of sentences), we discussed uncertainties in the 2LM method referring to valuable comments by both reviewers. In conclusion, we could only say that the 1LM method calculated latent heat flux could be underestimated and the 2LM method calculated latent heat flux seemed to be plausible; however, uncertainty involved in the 2LM method was so large that we could not confirm its significance, because gradients of air temperature, vapor pressure, and wind speed between two measurement heights (they were used for the 2LM calculation) were very small. Details of each relevant correction are explained in our responses to all related comments below. Technical corrections related to the reconstruction are as follows:

(**P18, L27**) Sect. 5 → Sect. 6.2

(**P19, L7**) Sect. 5 → Sect. 6.2

(**P21, L13**) Figure 11 → Figure 10

(**P21, L28**) Fig. 12 → Fig. 11

(**P22, L1**) Figure 12 → Figure 11

(**P25, L20**) Figure 10a → Figure 12a

(**P26, L17**) Figure 10c → Figure 12b

(**P26, L20**)Fig. 10c → Fig. 12b
(**P26, L23**) Fig. 10a → Fig. 12a
(**P26, L29**) Fig. 10c → Fig. 12b

Minor revisions:

During revision process, we found a typo at the beginning of Sect. 2 in the original manuscript: "An automated weather station (AWS) was newly installed at site SIGMA-A on 29 ~~July~~ June 2012 (Aoki et al., 2014a)." It is corrected in the updated manuscript. (**P6, L16**)

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).

Answer: We would like to thank the reviewer for these constructive comments. Related to the first comment, the Reviewer#1 also suggested we should give more information regarding the specificity of the meteorological condition during IOP in the context of long-term climatology. We agree with their point of view. As for the rainfall amount, we would like to answer in more detail at our feedback against the comment "6. P502, L1-12 and L22-25" below. Regarding the long term climatology of the site, our more detailed answer is presented at our correspondence against the comment “5. P500, L4-24”. Please consider our answer there.

For the question whether “continuous” rainfall is an unusual event in summer at this location or not?, we do not have any information. Although it is not a direct answer to the reviewer’s comment, we would like to say that at least atmospheric condition was “unusual” at that time. To support this, we have added following discussion at Sect. 2.1: “Neff et al. (2014) examined synoptic-scale atmospheric conditions over the GrIS during July 2012 from various aspects and summarized notable features as follows: (1) warm air originating from a record North American heat wave (the North American drought of 2012 was the worst since 1895), (2) transitions in the Arctic Oscillation, (3) transport of water vapor via an Atmospheric River over the Atlantic to Greenland, and (4) the presence of warm ocean waters south of Greenland. Bonne et al. (2015) clearly showed that moist air mass was advected northward following a narrow band reaching southern Greenland and then it moved northward along the western Greenland coast around 9 July. Observed features of above mentioned meteorological properties during the IOP at the SIGMA-A site are consistent with these large-scale atmospheric conditions.” (**P9, L2**). Here we intend to refer to “unusual condition” in the context of the long-term climatology by the following part of the revision above: “(1) warm air originating from a record North American heat wave (the North American drought of 2012 was the worst since 1895)”.

Added references are:

Neff, W., Compo, G. P., Ralph, F. M., and Shupe, M. D.: Continental heat anomalies and the extreme melting of the Greenland ice surface in 2012 and 1889, J. Geophys. Res. Atmos., 119, 6520-6536, 10.1002/2014JD021470, 2014.

Bonne, J.-L., Steen-Larsen, H. C., Risi, C., Werner, M., Sodemann, H., Lacour, J.-L., Fettweis, X., Cesana, G., Delmotte, M., Cattani, O., Vallelonga, P., Kjær, H. A., Clerbaux, C., Sveinbjörnsdóttir, Á., E., and Masson-Delmotte, V.: The summer 2012 Greenland heat wave: In situ and remote sensing observations of water vapor isotopic composition during an atmospheric river event, J. Geophys. Res. Atmos., 120, 10.1002/2014JD022602, 2015.

The second point is quite important. As mentioned above, we have dampen our argument regarding the 2LM method, and related information have been removed from abstract. For more detail, please consider our answer to the comment “11. Section 5” 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.

Answer: Thank you very much for the constructive comment. We agree with the point that we should summarize key energy balance studies previously performed over the GrIS in the “Introduction” section. In this study, we focus on summer SEB characteristics at the SIGMA-A site. Thus, we have reviewed some important studies investigating summer (sometimes melting period) SEB features over GrIS, and added the following paragraph:

“Several attempts that focus on the summer GrIS SEB characteristics have been performed. Presented results show that the net shortwave radiant flux is the main contributor for the surface heating in general; however, detailed characteristics vary from place to place, and differ from year to year. Greuell and Konzelmann (1994) unveiled temporal changes in SEB at the ETH Camp (69°34'N, 49°18'W, 1155 m a.s.l.), west Greenland, during the 1990 summer (June, July, and August). During this summer, average net shortwave radiant flux (82 W m^{-2}) and sensible heat flux (34 W m^{-2}) acted to heat the surface, while average net longwave radiant flux (-54 W m^{-2}), latent heat flux (-28 W m^{-2}) played a role in cooling the surface. SEB characteristics during the

1991 summer at the same place was presented by Ohmura et al. (1994). According to their results, the absolute values of each dominant SEB component averaged in this summer decreased obviously from the 1990 summer (net shortwave radiant flux was 65 W m^{-2}, net longwave radiant flux was -44 W m^{-2}, sensible heat flux was 16 W m^{-2}, and latent heat flux was -6 W m^{-2}, respectively). Summer SEB characteristics at the higher place on the GrIS was described by Cullen and Steffen (2001). They demonstrated that average turbulent (sensible and latent) heat fluxes at Summit (72°58'N, 38°51'W, 3203 m a.s.l.) during June 21 to July 6, 2000 were small (4 W m^{-2} and 3 W m^{-2}, respectively), while average net shortwave radiant flux (82 W m^{-2}) and net longwave radiant flux (-68 W m^{-2}) were comparable with previous results at ETH Camp. van den Broeke et al. (2011) presented long-term records of SEB at three AWSs situated along the K-transect, a stake array in southwest Greenland that extends from the ice margin to 1850 m a.s.l. They demonstrated that the temperature and moisture contrasts between ambient atmosphere and (melting) ice surface are less pronounced higher on the ice sheet, resulting in smaller summertime values of turbulent heat fluxes and net longwave radiant flux at the higher elevations." (**P4, L16**).

Here we have added the following four references related with this revision:

Cullen, N. J., and Steffen, K.: Unstable near-surface boundary conditions in summer on top of the Greenland Ice Sheet, Geophys. Res. Lett., 28, 4491-4493, doi: 10.1029/2001GL013417, 2001.

Ohmura, A., Konzelmann, T., Rotach, M., Forrer, J., Wild, M., Abe-Ouchi, A., and Toritani, H.: Energy balance for the Greenland ice sheet by observation and model computation, in: Snow and Ice Covers; Interaction With the Atmosphere and Ecosystems, Jones, H. G., Davies, T. D., Ohmura, A., Morris, E. M. (Eds.), IAHS, Gentbrugge, Belgium, 85-94, 1994.

van den Broeke, M. R., Smeets, C. J. P. P., and van de Wal, R. S. W.: The seasonal cycle and interannual variability of surface energy balance and melt in the ablation zone of the west Greenland ice sheet, The Cryosphere, 5, 377-390, doi:10.5194/tc-5-377-2011, 2011.

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 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.

Answer: We completely agree with the reviewer's point of view that the accuracy of sensors affect turbulent heat fluxes from the 2LM method. In the revised manuscript, we decided to list up the absolute accuracy of each sensor as well as results of relative calibrations in newly reconstructed Sect. 6.2 as follows:
"In fact, numerical sensitivity studies with perturbed input parameters considering absolute accuracy of temperature, relative humidity, and wind speed sensors (±0.2 °C, ±2 %, and ±0.3 m s^{-1}, respectively) in the 2LM calculation modified the picture of calculated turbulent heat fluxes drastically in any calculations. Even when relative differences in the accuracy of two sensors at the lower and upper measurement heights were considered (according to our relative calibration of the instruments performed in advance, air temperature and wind speed sensors at two levels showed no significant difference; however, as for relative humidity, the upper sensor tended to be lower by 1.2 % compared to the lower sensor), differences in calculated latent heat flux with perturbed input parameters were quite large." (**P27, L19**)

The reviewer also noted the necessity of indicating sampling rate. Related to this, the original description regarding the data storing procedure ("Measured data were averaged every 1 min and stored in a data logger (CR1000, Campbell, USA)." P500, L14 in the original manuscript) was not accurate. Now we have revised as follows:

"Measured data were sampled and stored in a data logger (CR1000, Campbell, USA) every 1 min." (**P7, L20**).

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.

Answer: Thank you very much for the very important information. We investigated related previous studies (including Box and Steffen (2001)) again and recognized that it is quite popular to correct relative humidity with respect to ice using the method presented by Anderson (1994), which is employed by Box and Steffen (2001). In the revised manuscript, we have performed the correction against relative humidity with respect to ice converted from observed relative humidity with respect to water. It is now stated in the revised version as follows: "As for relative humidity with respect to water, we converted it into relative humidity with respect ice when air temperature was below 0 °C, and performed the correction presented by Anderson (1994)." (**P7, L9**).

It means that the input data for the SMAP model were modified, and we performed recalculation accordingly as mentioned above. However, relative humidities with respect to ice and water are almost on the same level under the condition that air temperature is near 0 °C, and main conclusions of this study is not affected by this modification.

We have added the following reference related with this revision:

Anderson, P. S.: A method for rescaling humidity sensors at temperatures well below freezing, J. Atmos. Oceanic Technol., 11, 1388-1391, doi:10.1175/1520-0426(1994)011<1388:AMFRHS>2.0.CO;2, 1994.

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.

Answer: It is a nice suggestion. However, because the SIGMA-A AWS is a newly installed one, we estimated "possible" climate condition at the site referring two GC-Net AWS data (Humboldt and GITS) (Steffen and Box., 2001) located in the northwest GrIS at the beginning of Sect. 2 as follows:

"In the northwest GrIS, two GC-Net AWS sites exist (Steffen and Box., 2001); one is the Humboldt site (78°32'N, 56°50'W, 1995 m a.s.l.) and the other is the GITS site (77°08'N, 61°02'W, 1887 m a.s.l.). Steffen and Box (2001) presented monthly mean temperature at these stations and also demonstrated that the mean temperature lapse rate over GrIS in summer to be 0.4°C per 100 m. These information allow us to estimate possible average near-surface air temperature during July at the SIGMA-A site, and it was around -6.5°C." (**P6, L16**)

These discussion has allowed us to give the wider interpretation of measured air temperature at the SIGMA-A site (Sect. 2.1):

"Figure 2 presents time series of meteorological conditions measured with the AWS during the IOP. Until 9 July, air temperature at 3.0 m above the surface was already high and often exceeded 0 °C in the daytime. The time interval from 10 July until the end of the IOP coincided with the record near-surface melt event period reported by Nghiem et al. (2012); during that time air temperature increased slightly and remained above 0 °C, which is much larger than the estimated possible average air temperature at the SIGMA-A site: -6.5 °C (Sect. 2), continuously (Fig. 2a)." (**P8, L11**)

Added reference:

Steffen, K., and Box, J.: Surface climatology of the Greenland Ice Sheet: Greenland Climate Network 1995-1999, J. Geophys. Res., 106(D24), 33951--33964, doi:

10.1029/2001JD900161, 2001.

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.

Answer: Firstly, we have mentioned a precipitation normal in SIGMA-A referring Ohmura and Reeh (1991)'s result as follows: "According to Ohmura and Reeh (1991), annual total precipitation near the SIGMA-A site is extrapolated to be around 200 – 300 mm w.e. The estimated total precipitation during this event can account for more than 30 – 50 % of the annual total precipitation." (**P10, L5**)

Regarding the next comment, it has not been determined whether the NSL is the latest annual layer or not, because of the lack of justification. Further information from chemical analysis, for example, would be necessary (this attempt is ongoing). In the revised manuscript, we have indicated it as follows: "At present, the NSL has not been determined to be the latest annual layer, because the lack of justification." (**P10, L18**)

The last question is related to the significance of RMSE and ME of calculated snow temperature. During IOP, total 221 profiles (after rejecting strange data) were available for the validation of snow temperature profile calculated by the SMAP model. We believe it is sufficient for this kind of assessment. In the revised manuscript, it is indicated as follows: "In this comparison, total 221 profiles (after rejecting strange data) were available." (**P11, L6**)

Added reference:

Ohmura, A., and Reeh, N.: New precipitation and accumulation maps for Greenland, J. Glaciol., 37, 140-148, 1991.

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?

<u>Answer</u>: The upper bound of Richardson number (0.1) ensures small but non-zero exchange of turbulent heat flux even under strongly stable condition. During IOP, low wind speed (lower than 1.0 m s^{-1}) were rarely observed at the SIGMA-A site. As a result, such a strongly stable condition was not observed on-site, and we could not confirm the effect of this setting. It is stated at the end of Sect. 3.1 of the updated manuscript as follows: "During IOP, low wind speed were rarely observed at the SIGMA-A site as mentioned in Sect. 2.1. As a result, preliminary numerical simulation revealed that the impact of the upper bound was not clear during IOP; however, it is still set in this study." (**<u>P14, L1</u>**)

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.

<u>Answer</u>: In this procedure, model simulated NSL thickness (the SMAP model tended to underestimate by -2.0 cm, which may be sufficiently enough, during IOP compared to the snow pit measurements) was simply stretched to adjust the snow-pit measurements. Here, model simulated internal properties were not modified at all, implying that the mass in the NSL is not conserved "only during this process"; however, this is completely a post-process management and does not affect the model simulation itself. The small error of -2.0 cm makes it difficult to look at meaningful possible causes of the discrepancy. Thus, we have revised as follows: "When a measured snow temperature profile was compared against simulation results, the depth of the NSL simulated by the model (the SMAP model tended to underestimate the NSL's depth by -2.0 cm compared to the snow pit measurements during the IOP) was adjusted to the measured depth as a post-process, where model simulated internal properties were not modified at all." (**P16, L19**)

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.

Answer: First of all, we should comment that the same emissivity (0.98) was used in the process of obtaining observed snow surface temperature, as well as the SMAP model calculation. In order not to cause misunderstanding to readers, we have added an explanation that emissivity of 0.98 was employed by the SMAP model in the model description section as follows: "In addition, the emissivity of the snow surface ε was assumed to be 0.98 (Armstrong and Brun, 2008; van As, 2011) throughout this study." (**P15, L22**). Related with this revision, we have removed the original description introducing emissivity in Sect. 4. In the added description highlighted above, we have cited van As (2011) who employed the value of 0.98 in the GrIS. Basically, we think that there are no good grounds for both values of emissivity (0.98 or 1), which may cause uncertainties in SEB calculations. Therefore, we discussed uncertainties induced by the choice of this value in the newly added discussion section (Sect. 6.1) as follows: "Secondly, we investigated effects of ε_s introduced in Sect. 3.3 on SEB calculations. In this sensitivity test, ε_s was set to be 1.0 and surface temperature (to be input to the SMAP model) was calculated only from observed $L^{\uparrow}$. The result indicated that mean differences of turbulent heat fluxes against the original SEB calculation were 1.1 w m^{-2} and 0.9 W m^{-2} for H_S and H_L, respectively. This result implies that the sensitivity of SEB calculation to the choice of ε_s is small, and SEB characteristics during IOP at the SIGMA-A site presented in Sect. 5 is still valid. van As (2011) also performed this type of sensitivity test and demonstrated ε_s 's small impact on the SEB calculation." (**P24, L6**)

As for Fig. 7, its presentation has been modified.

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.

Answer: We would like to thank this encouraging comment. As a matter of fact, the SMAP model owes much of the "impressive" result to the PBSAM developed by Aoki et al. (2011) that explicitly treats the physical processes both in the atmosphere and the snowpack, which are explained in the manuscript. We considered what should be specially mentioned more, and decided to discuss as follows: "Therefore, once the SMAP model or the PBSAM are coupled with atmospheric models, it is necessary for such host atmospheric models to simulate the presence or absence of cloud realistically. King et al. (2014) also argued that efforts to improve model simulations of surface energy balance and melt in the polar region should concentrate initially on reducing biases in modeled shortwave and longwave radiation, which are caused by deficiencies in the representation of cloud properties." (**P19, L20**). It is also stated in the conclusion section as "These physical processes are explicitly taken into account by the PBSAM, an important component of the SMAP model, highlighting that an advantage of PBSAM." (**P29, L14)**, and in the abstract as "Above all, the fact that the SMAP model successfully reproduced frequently observed rapid increases in snow albedo under cloudy-conditions highlights the advantage of the Physically Based Snow Albedo Model (PBSAM) incorporated in the SMAP model." (**Abstract**).

Added reference:

King, J. C., Gadian, A., Kirchgaessner, A., Kuipers Munneke, P., Lachlan-Cope, T. A., Orr, A., Reijmer, C., van den Broeke, M. R., van Wessem, J. M., and Weeks, M.: Validation of the summertime surface energy budget of Larsen C Ice Shelf (Antarctica) as represented in three high-resolution atmospheric models. J. Geophys. Res. Atmos., 120, 1335-1347, doi:10.1002/2014JD022604, 2015.

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.

Answer: Very insightful comments that we should consider without fail. In the reconstructed manuscript, discussion on disparities between the 1LM and 2LM methods are placed in Sect. 6.2, while sensitivity studies investigating effects of the snow surface roughness length was conducted in Sect. 6.1 as mentioned above.

First, we have mentioned important results by Box and Steffen (2001) and Cullen et al. (2014), which we agree with that we should refer to, in the third paragraph of Sect. 6.2 (**P25, L23~**), where we compared latent heat fluxes from the 1LM and 2LM methods at the SIGMA-A site during the period:

"Obviously, the result indicated that the latent heat flux from OBS_1LM was almost compatible with the heat flux from SMAP_1LM, implying that the OBS_1LM is also likely to underestimate the latent heat flux. Comparison between OBS_2LM and OBS_1LM shows that the former obviously tends to be higher than the latter, and sometimes the signs of the fluxes are different. According to previous studies (Box and Steffen, 2001; Cullen et al., 2014), the sign and magnitude of the latent heat fluxes from the 1LM and 2LM methods agree reasonably at low elevations on the GrIS, whereas they often differ from each other at the higher elevations. Measurements conducted previously in the northwest GrIS (the Humboldt and GITS sites) showed that the net annual sublimation from the 1LM and 2LM methods did not agree sufficiently at both sites (Box and Steffen, 2001). In the former site, the sign was contrasting, while the magnitude was remarkably different at the latter site."

In the next (fourth) paragraph of Sect. 6.2 (**P26, L12**), we demonstrated that the SMAP model forced by two level measurements succeeded in reproducing the surface hoar, which discussion is basically as same as the original manuscript. However, several minor revisions were performed in order to maintain consistency in the manuscript.

Then, in the last paragraph of Sect. 6.2, we calculated wind speed, temperature, and moisture gradients (surface and two levels in the atmosphere) as suggested. Obtained gradients for wind speed, air temperature, and vapor pressure between the surface and the lower measurement height (positive downward) were 1.6 s^{-1}, 0.3 °C m^{-1}, and -0.15 hPa m^{-1}, respectively during the IOP. However, those values between the lower and upper measurement heights were nearly 0, except for the case of wind speed: 0.2 s^{-1}. If we focused on the period from 00:10 to 00:20 UTC on 4 July when SMAP_2LM detected the surface hoar, we found that vapor pressure gradients showed opposite signs: -0.13 hPa m^{-1} for the 1LM method and 0.01 hPa m^{-1} for the 2LM method, respectively. This result explains why the 1LM method failed to detect the surface hoar, whereas the 2LM method succeeded in that. However, those gradients between the lower and upper sensors are too low to detect significant uncertainties caused by the sensor accuracy. The accuracy of each sensor and results from relative calibration between the lower and upper sensors are presented here. As a result, we decided to discuss the uncertainties of the 2LM method as follows:

"According to Box and Steffen (2001), the uncertainty of the 2LM method increases as the temperature, humidity, and wind speed differences between two measurement heights decrease. These motivated us to investigate the significance of the 2LM method calculated latent heat flux during IOP. In this inquiry, gradients (positive downward) of wind speed, temperature, and vapor pressure between the surface and the lower measurement height, as well as those between the lower and upper measurement heights were investigated at first. Averaged gradients between the surface and the lower measurement height during the IOP were 1.6 s^{-1}, 0.3 °C m^{-1}, and -0.15 hPa m^{-1}, respectively. The value for vapor pressure is very close to that obtained at Summit during 2000–2002 reported by Cullen et al. (2014). On the other hand, averaged gradients between the lower and upper measurement heights were nearly 0, except for the case of wind speed: 0.2 s^{-1}. Focusing on the period from 00:10 to 00:20 UTC on 4 July when SMAP_2LM detected the surface hoar, vapor pressure gradients showed opposite signs: -0.13 hPa m^{-1} for the 1LM method and 0.01 hPa m^{-1} for the 2LM method, respectively. Although this result explains the reason why only the 2LM method succeeded in the surface hoar detection, the latter value is still very small. These make it difficult to assess uncertainties of the 2LM method caused by each sensor as expected. In fact, numerical sensitivity studies with perturbed input parameters considering absolute accuracy of temperature, relative humidity, and wind speed sensors (±0.2 °C, ±2 %, and ±0.3 m s^{-1}, respectively) in the 2LM calculation modified the picture of calculated turbulent heat fluxes drastically in any calculations. Even when relative differences in the accuracy of two sensors at the lower and upper measurement heights were considered (according to our relative calibration of the instruments performed in advance, air temperature and wind speed sensors at two levels showed no significant difference; however, as for relative humidity, the upper sensor tended to be lower by 1.2 % compared to the lower sensor), differences in calculated latent heat flux with perturbed input parameters were quite large. Therefore, we should conclude that underestimation of the latent heat flux calculated with the 1LM method could be plausible, although the exact order of underestimation was quite hard to detect during this study period." (**P27, L3**)

We have added following references in the updated manuscript:

Cullen, N. J., Mölg, T., Conway, J., and Steffen, K.: Assessing the role of sublimation in the dry snow zone of the Greenland ice sheet in a warming world, J. Geophys. Res. Atmos., 119, 6563–6577, doi:10.1002/2014JD021557, 2014.

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).

Answer: For the first point, we have mentioned the detail as follows: "The snow surface is heated when the flux is positive and surface melt is occurred if the surface temperature is 0 °C, whereas it is cooled if the flux is negative." (**P21, L7**)

As for the next point, we have unified the calculation method for turbulent heat flux (only the 1LM method was used) as explained at the beginning of this response. Related with this, the statement: "However, based on the discussion in Sect. 5, only *H*L was calculated by using the 2LM approach." has been removed. (**P21, L11**)

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.

Answer: Thank you very much for this interesting information. We checked these references, and decided to cite the information in this section as follows: "These values, calculated during Period-1, are almost equal to the surface fluxes from June to August averaged during the summers of 2000-2011 over the GrIS accumulation area based on the MAR regional climate model (Fettweis et al., 2011) and MODIS data presented by Box et al. (2012). On the other hand, the SEB characteristics during Period-2 (signs and orders) resemble those obtained at Langfjordjøkelen, Norway (Giesen et al., 2014). (**P22, L7**)

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.

Answer: Based on our response against the comment 11, we have reconstructed the conclusion section. In the revised manuscript, we have removed the last paragraph of the "Conclusion" section in the original manuscript, and we only mentioned as follows "In order to confirm the validity of SEB characteristics during IOP, additional error analyses were conducted. During this process, it was turned out that the sign of latent heat fluxes from the 1LM and 2LM methods differed especially when the surface hoar was observed (3-5 July). The former showed negative, while the latter turned positive and designated the surface hoar formation. Therefore, the 2LM method calculated latent heat flux seemed to be plausible; however, uncertainty involved in the 2LM method was so large that we could not confirm its significance." (**P30, L3**)

Technical corrections

P498, L5-L6: present tense could be used – "these fluxes are defined to be positive when they are directed

Answer: Corrected as suggested (**P4, L9**).

P501, L1: these data could be used rather than “this” data

<u>Answer</u>: Revised as suggested (**<u>P8, L8</u>**).

P507, L8: we calculated the temporal evolution – add “the”

<u>Answer</u>: We have added “the” as suggested (**<u>P15, L7</u>**)

P507, L12: resolution for an Arctic snowpack – add “an”

<u>Answer</u>: Revised as suggested (**<u>P15, L11</u>**).

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