Reply to J. King (Reviewer#1)

We sincerely appreciate the reviewer for taking the time to provide quite valuable comments. 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. Please also refer to the revised marked-up manuscript uploaded as a supplementary file. In the manuscript, change logs can be tracked.

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 characterized 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 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.

We appreciate for this positive evaluation. First, we summarize important revisions, which were conducted considering valuable comments from both reviewers, in the updated manuscript.

Major revisions:

First we have modified the input data for the SMAP model in terms of relative humidity. The reviewer argued the necessity to indicate the exact definition of relative humidity. Related with this, Reviewers#2 also suggested 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 Reviewer#2 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. In reference to this, scores indicating model performance, values of SEB fluxes, and some figures are changed slightly:

(**P17, L21**) 0.53 °C → 0.58 °C

(**P17, L24**) RMSE = 0.85 °C → RMSE = 0.94 °C

(**P17, L25**) ME = 0.55 °C → ME = 0.68 °C

(**P18, L16**) 0.16 → 0.17

(**P22, L3**) 15.2 → 15.3

(**P22, L3**) 11.2 → 11.3

(**P22, L3**) -13.2 → -18.0

(**P22, L3**) 17.8 → 7.2

(**P22, L4**) +31.0 → +25.2

(**P22, L5**) 102.5 → 96.7

(**P22, L6**) 55.0 → 49.1

(**P22, L6**) 24.9 to 79.9 → 20.1 to 69.2

(**P29, L22**) +31.0 → +25.2

Table1

Figures 6, 7, 8, 9, 10, 11, and 12

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, which both reviewers pointed out. 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 Reviewer#2 regarding the validity of the choice of this value. 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:

(**<u>P18, L27</u>**) Sect. 5 → Sect. 6.2

(**<u>P19, L7</u>**) Sect. 5 → Sect. 6.2

(**<u>P21, L13</u>**) 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**)

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).

Answer: Thank you very much for the very constructive comment. First of all, we think that the short-time rainfall event reported in this study is caused by a mesoscale convective system, and this kind of system is generally very difficult for hydrostatic global atmospheric models including ERA-Interim, which can calculate climatic accumulation reasonably even over the Greenland ice sheet. In order to reproduce realistic rainfall amount of this event, we think a high-resolution (in time and space) non-hydrostatic atmospheric model is necessary.

In addition, we found that ERA-Interim originally tends to underestimate annual accumulation in the area north of 68°N (Chen et al., 2011), even though ERA-Interim shows close spatial pattern of accumulation to the observations.

Based on the consideration above, we have added following discussion here: "As demonstrated by Chen et al. (2011), ERA-Interim data originally tends to underestimate annual accumulation in the area north of 68°N, even though it shows close spatial pattern of accumulation to the observations over the whole area of GrIS. In addition, it is possible that insufficient horizontal resolution (0.75°) and a hydrostatic atmospheric model, which cannot reproduce a short-time mesoscale convective system realistically in general, might have caused the large discrepancy." (**P9, L25**).

We have added the following reference related with this revision:

Chen, L., Johannessen, O. M., Huijum, W., and Ohmura, A.: Accumulation over the Greenland Ice Sheet as represented in reanalysis data, Adv. Atmos. Sci., 28, 1-9, doi:10.1007/s00376-010-0150-9, 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 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.

Answer: We would like to thank the reviewer's quite insightful suggestions. Especially, the comment that "The 2-level method involves measuring the (often small) differences in temperature and mixing ratio between two measurement levels. " touches the core of the subject. In this comment, the reviewer argued the necessity of error analysis considering the accuracy of each sensor, which the Reviewer#2 also did. Now, we completely agree with their point of view. In the reconstructed manuscript, discussion on disparities between the 1LM and 2LM methods as well as uncertainties involved in the 2LM method are placed in Sect. 6.2. Below we explain how we corresponded against the reviewer's concerns in Sect. 6.2 (originally Sect. 5).

Until the fourth paragraph, a basic flow of discussion is almost the same as the original manuscript, however, several 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 grasp 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, our discussion regarding the uncertainties of the 2LM method is now 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." (**<u>P27, L3</u>**)

As for the presentation of SEB in Sect. 5, we have unified the calculation method of turbulent heat fluxes into the 1LM method as mentioned above.

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?

<u>Answer</u>: This is a good suggestion. After we submitted the original manuscript, several papers presenting the synoptic weather pattern during July 2012 have been published. In the updated manuscript, we explained the large-scale meteorological condition during IOP referring these papers as follows: "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." (**<u>P9, L2</u>**). We intend to refer to "exceptional state" 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.

Specific comments and technical corrections

P498, L1: "However, a temperature increase is unlikely to be the only cause of surface melt, because surface melt is physically controlled by the surface energy balance (SEB)." →The occurrence of melt is controlled by temperature, its intensity is controlled by SEB

Answer: We intended that a temperature increase cannot always induce surface melt if increased air temperature is still negative. Here we have corrected as follows: "However, a temperature increase cannot induce surface melt if the surface temperature, which is physically controlled by the surface energy balance (SEB), is below 0 °C." **(P4, L4)**

P498, L7: "induces surface melt." → Only if surface temperature = 0degC

Answer: We agree with this point. We have modified the latter half of this sentence as follows ", and a positive sum of these fluxes (net energy flux) induces surface melt only if the surface temperature equals 0 °C" **(P4, L11)**

P501, L9: "The relative humidity" → Clarify whether relative humidities are reported with respect to water or ice

Answer: The values presented in Fig. 2 are relative humidity with respect to "water". Here we have indicated it is "The relative humidity with respect to water" (**P8, L17**). Also, we indicated that the AWS measured relative humidity with respect to water in beginning of the same section (**P7, L2**). In addition, we have also mentioned that the values in Fig. 2 are relative humidity with respect to water in the caption of Fig. 2 (**Fig. 2**).

Related to this point, as mentioned in the beginning of this response, we realized that our treatment of relative humidity under the condition that air temperature is negative was not sufficient at the initial manuscript, because we had not performed the general (and sometimes quite necessary) correction of relative humidity with respect to ice presented by Anderson (1994) (Comments by Referee#2 also affected this consideration). 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.

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.

P502, L6: "The comparison indicated that the accumulated precipitation obtained from the ERA-Interim reanalysis data was lower by a factor of 1/4.9. The most prevailing reason for this discrepancy was not a misrepresentation of the true area of rainfall, but just underestimation by the ERA-Interim reanalysis (Fig. 3).” → Why does ERA-Int underestimate the intensity of the precipitation so badly? Chen et al (Adv. Atmos. Sci., 2011) find that reanalyses provide a good representation of climatological accumulation across the Greenland Ice Sheet.

Answer: Please consider our answer to General comment (1) above.

P502, L22: “depth” → “thickness”

Answer: Corrected as suggested (**P10, L17**).

P504, L10: “precipitation” → Presumably the model requires the phase of the precipitation (rain or snow) as well as the amount? Does SMAP account for the heat flux associated with rain?

Answer: The phase of the precipitation is internally calculated as a function of wet bulb temperature. In the revised manuscript, we have indicated it as follows: “In default configuration, the SMAP model requires precipitation (partitioned in the model into snow and rain by using the algorithm to calculate snow:rain ratios as a function of wet bulb temperature (Yamazaki, 2001)), air pressure, wind speed, air temperature, relative humidity, downward ultraviolet (UV)-visible and near-infrared radiant fluxes, the diffuse components of UV-visible and near-infrared radiant fluxes, downward longwave radiant flux, subsurface heat flux, and the mass concentrations of snow impurities (BC and dust) (Niwano et al., 2012).” (**P12, L10**).

We have added the following reference related with this revision:

Yamazaki, T.: A one-dimensional land surface model adaptable to intensely cold regions and its applications in eastern Siberia, J. Meteorol. Soc. Jpn., 79(6), 1107–1118, doi:10.2151/jmsj.79.1107, 2001.

As for the second question, please consider our answer to the comment for (P516, L2) below.

P506, L1: “minimum” → "small but non-zero" might be clearer than "minimum"

Answer: We have rewritten as suggested (**P13, L28**).

P506, L13: “of” → “for”

Answer: Corrected as suggested (**P14, L14**).

P509, L9: “indicated that the model tended to underestimate the snow temperature profile. However, the SMAP model satisfactorily simulated the temperatures.” → Sounds a bit inconsistent. Can you clarify?

Answer: We agree with the referee’s point of view. The statement is ambiguous. Here we revised simply as follows: “indicated that the model simulated the temperatures reasonably” (**P17, L10**).

P512, L16: “the assumption of the SMAP model that the snow surface is saturated” → It would be better to say "...on the assumption that air at the surface is saturated with respect to ice at the snow surface temperature"

Answer: This is a nice suggestion. We have rewritten as suggested. Please note that the section has been moved to Sect. 6.2 as mentioned at the beginning of our response (**P24, L17**).

P513, L14: "ΨM, and ΨH" → Ψ-M, H are not "unknown parameters" but are (specified) functions of the (unknown) stability parameter, z/L, which has to be calculated iteratively. You should give a reference for your choice of psi-functions.

Answer: We agree with this point. We have revised the original explanation as follows: "Other parameters are calculated by the same method used by Niwano et al. (2012). The choice of Ψ_M and Ψ_H depends on stability conditions in the atmospheric boundary layer. When the atmosphere is stable, the SMAP model assumes that $\Psi_M = \Psi_H$ and calculates the profile functions according to Holtslag and De Bruin (1988), whereas the SMAP model carries out the calculations with functions determined by Paulson (1970) under unstable conditions." (**P25, L14**).

Related to this, the following references are now added:

Holtslag, A. A. M., and De Bruin, H. A. R.: Applied modeling of the nighttime surface energy balance over land, J. Appl. Meteorol., 27, 689-704, doi:10.1175/1520-0450(1988)027<0689:AMOTNS>2.0.CO;2, 1988.

Paulson, C. A.: The mathematical representation of wind speed and temperature profiles in the unstable atomospheric surface layer, J. Appl. Meteorol., 9, 857-861, doi: 10.1175/10.1175/1520-0450(1970)009<0857:TMROWS>2.0.CO;2, 1970.

P514, L21: "However, it is still obvious that characteristics of latent heat fluxes with perturbed z0 are significantly different from the results from OBS_2LM." → The most significant difference between 1LM and 2LM is the difference in the sign of HL and no amount of variation of Z0 is going to change this.

Answer: We agree with this point. As explained in the beginning of this response and our answer to the general comment 2), we have investigated difference between the

1LM and 2LM methods in Sect. 6.2 of the updated manuscript, although we could not determine significant difference between both methods as mentioned in our answer to the general comment 2). The discussion regarding the uncertainties in SEB caused by the choice of snow surface roughness length is now placed in Sect. 6.1 as mentioned in the beginning of this response.

P516, L2: "*H*R is the heat flux associated with rainfall," → You should state how you calculate this

Answer: We have added following description: "H_R is the heat flux associated with rainfall calculated as a function of rainfall rate and a difference in rain temperature (wet bulb temperature is assumed) and surface temperature (Niwano et al., 2012)," (**P21, L3**)

P516, L15: "it increased" → "became generally positive"

Answer: We have rewritten as suggested (**P21, L18**).

P516, L19: "while it was almost 0 Wm^{-2} in the daytime." → Presumably this indicates a near-isothermal snowpack as a result of meltwater percolation?

Answer: Yes, exactly. Additional explanation is given in the updated manuscript: "Finally, H_G showed clear diurnal variation: it heated the surface especially during the night time, while it was almost 0 Wm^{-2} in the daytime as a result of isothermal profile in the near-surface snowpack caused by meltwater percolation." (**P21, L20**).

P516, L21: "it remained positive" → "it remained positive at all times"

Answer: We have added "at all times" as suggested (**P21, L25**).

P518, L24: “comparable and reasonable.” → Not clear what you mean. "Validation results that the model was able to produce a good simulation of the evolution of snow surface temperatures and snow temperature profiles"?

Answer: We agree with point. It can cause misunderstandings. Now, we have revised simply as follows: “Validation results revealed that the RMSE for the snow temperature profile and snow surface temperature were ~~comparable and~~ reasonable.” (**P28, L25**)

P519, L13: “to obtain an accurate SEB in the validation of the snow grain size calculated with the SMAP model,” → What is the main issue here - SEB or grain size? The two are connected, of course, through the impact of grain size on albedo.

Answer: As mentioned in our response to the general comment 2), we have dampen our argument regarding the 2LM method. In this section, our conclusion regarding the calculation method of latent heat flux is 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**)

P519, L25: “we could confirm that the 2LM method was an effective way to obtain an accurate *HL*.” → I don't think you can be as definite as this without an independent measurement of HL for validation.

Answer: Thank you for this comment. As we responded to the previous comment, the statement has been removed in the updated manuscript.

P527, L4: “observed temperature” → “observed surface temperature”

Answer: Corrected as suggested. (**Table 1**)

P529, L1: “IOP” → Give dates: 1-13 July 2012?

Answer: We have given the detail of IOP in the caption of Fig. 2. (**Fig. 2**)

P529, L2: “29 June 2012” → A bit confusing (sounds as if you are only presenting temperature measurements for 29 June). Why not say "...observed at a nominal height of 3 m above the snow surface"?

Answer: It is a nice suggestion. We have revised as suggested. (**Fig. 2**)