# Author's response to "Review" by dr. Nishumura

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The authors would like to thank dr. Nishimura for the time devoted to review the manuscript and for his useful and constructive comments. All comments by the referee were carefully addressed and the manuscript has substantially benefited from the proposed changes. We would like to clarify our changes regarding each of the 10 comments:

> **General Comment 1:** "Needless to say, I do not deny the meaning of the precise calculations and sensitivity evaluations on this manuscript, but authors would like to conclude just the Princess Elisabeth station (AWS 16) is located on a unique place? Preferably, not only such local issue but also more universal one which is applicable to other places is introduced. For instance, many attempts have been made to estimate the sublimation rate in the Antarctica, as is also shown in the manuscript. The methodologies are different, thus errors included may differ more or less. Nevertheless, I believe it is worth comparing each other. It would be also useful to attest that the sublimation amount obtained this study is reasonable. Takahashi estimated the sublimation rates over -200mm w.e. yr-1, whereas this study area was only 18 to 52 w.e. yr-1. Such difference can be explained reasonably with the meteorological factors? Discussions like this will be useful for the next stage, such as the total estimates of sublimation from all over the Antarctica."

We improved the manuscript by comparing computed sublimation amounts to other studies, and explain – in more detail – the underlying reasons for the differences between the sites. In Table 1, sublimation estimates from 9 different studies are compiled. However, already among each other they show strong variability. Moreover, great caution is needed when comparison is made between our results and extrapolated values from studies with an observation period less than one year. Mostly, observations in these studies are performed during the summer season, and hence generate too high annual sublimation rates when being extrapolated. Comparison to multiyear studies, on the other hand, shows that sublimation amounts presented in the manuscript generally are of the same order of magnitude. Although, as expected, sublimation rates at AWS 16 are lower compared to estimates found in literature, for AWS 5, 6 and 9, reasonable agreement is found with stations located in the same geographical zone of the ice sheet. Hence, the first paragraph of Section 3.2. has been extended with the comparison to Frezotti et al. (2004) and Déry and

Yau (2002) for $SU_s$, whereas comparison to Bintanja (1998), Déry and Yau (2002) and Lenaerts and Van den Broeke (2012) was included in paragraph 3 for $SU_{ds}$.

| **Estimate** | **Period** | **Method** | **Location** | $SU_s$ | $SU_{ds}$ |
|---|---|---|---|---|---|
| Fujii and Kusunoki (1982) | 1 Jul '77 -31 Jan '78 | Pan | Mizuho (ice) | -85* | / |
| Fujii and Kusunoki (1982) | 24 Nov '77 -11 Jan '78 | Stakes | Mizuho (ice) | -224* | / |
| Fujii and Kusunoki (1982) | 24 Nov '77 -11 Jan '78 | MOST | Mizuho (ice) | -192* | / |
| Bintanja and VdBr (1995) | 28 Aug '92 - 10 Feb '93 | MOST | Svea Cross | -245* | / |
| King et al. (1996) | 1 Mar '91 - 31 Aug '91 | MOST | Halley | 8.72* | -7.44* |
| Frezotti et al. (1997) | 1998-2000 | Stakes | Terre Adélie | -3 to -156 | / |
| VdBr (1997) | 5-yr GCM climatology | NM | Sør Rondane | -150 to -200 | / |
| Bintanja (1998) | 1987 | B98 | Dumont d'Urville | / | -170 |
| Bintanja (1998) | 1987 | B98 | D47 (1560m a.s.l.) | / | -120 |
| Bintanja (1998) | 1987 | B98 | D57 (2105m a.s.l.) | / | -60 |
| Bintanja (1998) | 1987 | B98 | D80 (2500m a.s.l.) | / | -8 |
| Déry and Yau (2002) | 1979-1993 | NM | Sør Rondane | -50 to -75 | -40 to -60 |
| Lenaerts et al. (2010) | 1997 | NM | Neumayer | -22 | -80 |
| Lenaerts and VdBr (2012) | 1989-2009 | NM | Sør Rondane | / | -40 to -70 |

Table 1: Annual sublimation from other studies as estimated over snow surfaces, with values of $SU_s$ and $SU_{ds}$ given in mm w.e. yr$^{-1}$. MOST stands for Monin-Obukhov similarity theory, whereas NM stands for numerical model and VdBr for Van den Broeke. For studies covering the entire ice sheet, contours closest to the Sør Rondane mountains were used. *: values for measurement periods shorter than one year are extrapolated. Note that these measurement periods usually are summer months (except King et al., 1996), during which $SU_s$ peaks.

Only a limited number of previous studies already estimated $SU_s$ based on Monin-Obukhov similarity theory for different study sites in Antarctica. However, studies of this kind generally use observations no longer than one or a few months. Other known methods to calculate $SU_s$ are pan sublimation measurements (e.g. Fujii and Kusunoki, 1982) and estimates based on the stake method (e.g. Fujii and Kusunoki, 1982; Frezotti et al., 1997). Pan sublimation has the advantage of observing directly $SU_s$ by measuring mass changes of frozen water in a glass dish. However, it requires manned observations at the site and only works for an ice surface. The stake method, on the other hand, has the drawback that the snow density of the upper snow layer must be monitored accurately and, more critically, that other surface processes such as snowdrift (both erosion/deposition and sublimation) and precipitation (synoptic or diamond dust) might contaminate the measurements. $SU_{ds}$, on the other hand, can only be derived from numerical models including blowing snow physics (NM). As mentioned in the manuscript (Sect. 1 and 3.3), there are few examples of application of these models to estimate $SU_{ds}$ locally (e.g. Lenaerts et al., 2010) or across the entire ice sheet (e.g. Déry and Yau, 2002; Lenaerts et al., 2012).

Finally, it is important to note that the high $SU_s$ rates computed by Takahashi et al. (1992), to which both the referee and the introduction of the manuscript refer, are for a blue ice area downstream of Seal Rock (53 km north of AWS 16). Blue ice areas generally have a lower albedo (~0.56) than snow surfaces (~0.80), generating significantly higher turbulent fluxes to compensate for the enhanced solar radiation absorption (Bintanja and Van den Broeke, 1995). Besides surface albedo, also differences in surface roughness and near-surface meteorology were found to affect sublimation rates over blue ice areas compared to snow surfaces, as discussed in detail by Van den Broeke and Bintanja (1995). We note that the estimate of Takashi reasonably agrees with the estimated annual sublimation mass flux of 350 mm w.e. $yr^{-1}$ computed by Van Den Broeke et al. (2005) for a blue ice field upstream of the Sør Rondane mountains and with Estimates at Mizuho by Fujii and Kusunoki (1982) shown in Table 1. However, since all estimates presented in our study are for snow surfaces only, they cannot be compared to results from Takahashi et al. (1992).

> **General Comment 2**: "Further, in Fig. 1, it looks like AWS 6 locates on the shoulder of the plateau and AWS 5 is further downstream. I am anxious that Coriolis force may change the wind direction. However, if we can assume the air at AWS 6 flows down to AWS 5, it is worth comparing wind speeds, air temperatures, RHs and the evaluated sublimation amounts at both stations. It may reveal how the sublimation progresses along the katabatic flow."

Mean values for air temperature, relative humidity with respect to ice and wind speed for each station were included in Table 1 of the manuscript. It shows that both stations are equally exposed to wind, while AWS 5 experiences higher air temperatures and relative humidity with respect to ice. The higher $T$ at AWS 5 seem to generate higher $SU_s$ amounts, whereas slightly more frequent extreme winds (15-20 m $s^{-1}$, see Fig. 8 and Fig. 11a) are responsible for the higher $SU_{ds}$ amounts. However, unfortunately such a comparison cannot not reveal how sublimation progresses along the katabatic flow, as explained hereafter.

AWS 5 is situated on the coastal slopes of the ice sheet (Van Den Broeke et al., 2004), with the dominant wind regime being the katabatic (ENE) (Fig. 9b). AWS 6 is located at an altitude of 1160 m a.s.l. and about 10 km to the north of the steep transition towards the plateau (the Heimefrontfjella, with height differences exceeding 1000 m in less than 3 km). Most of the time, it experiences katabatic winds of easterly direction, and since the synoptic forcing is associated with (south)easterly surface winds, they tend to enhance the katabatic flow at this location (Bintanja, 2000; Jonsson, 1995) (Fig. 9c).

For the winds to flow down from AWS 5 to 6, this would require S or SSE wind directions at both stations. However, this is not observed, and at AWS 6 this has been ascribed to two main causes (Bintanja, 2000). (i) First, as also indicated by the referee, the low surface inclination at this location (1.5 °) increases the importance of Coriolis effect relative to that of the katabatic force, leading to an eastward shift of the katabatic wind vector. (ii) Second, the boundary layer flow at AWS 6 is influenced by the presence of the steep topography of the Heimefrontfjella, an obstacle to the flow. As a consequence, at AWS 6 the predominant katabatic wind direction is even further shifted in the NE direction (Bintanja, 2000). At AWS 5, again the low surface inclination (1.3 °) and associated strong Coriolis effect explains the wind veering.

> **Specific comment 1**: "Page 1495, line 3: "SEB" should be shown as "surface energy balance" here."

The considered sentence was adapted.

> **Specific comment 2**: "Page 1498, line 23: I am not so sure that the sublimation from the snow surface never happens in the drifting case."

To answer this question, one has to look into the thermodynamic feedbacks associated with snowdrift sublimation. For the sublimation of drifting snow particles both causes the temperature to decrease and relative humidity with respect to ice of the near surface layer to increase (Dover, 1993; Déry et al., 1998; Mann, 1998; Bintanja, 2001).

The relative importance of these two thermodynamic feedbacks at AWS 16 was verified by applying the numerical blowing snow model SNOWSTORM (Bintanja, 2000) to a test case at AWS 16 (11 February 2009, 10-12 GMT) and subsequently comparing it to a simulation using the default model settings ("Idealised" run). The latter is characterised by significantly higher wind velocities. It was found that (i) the temperature change is very small (Fig. 1c), too little to have an effect upon $SU_s$, whereas (ii) the increase in relative humidity with respect to ice ($RH_i$) is indeed significant at AWS 16, even though it is smaller than the idealised high wind speed event (Fig. 1b).

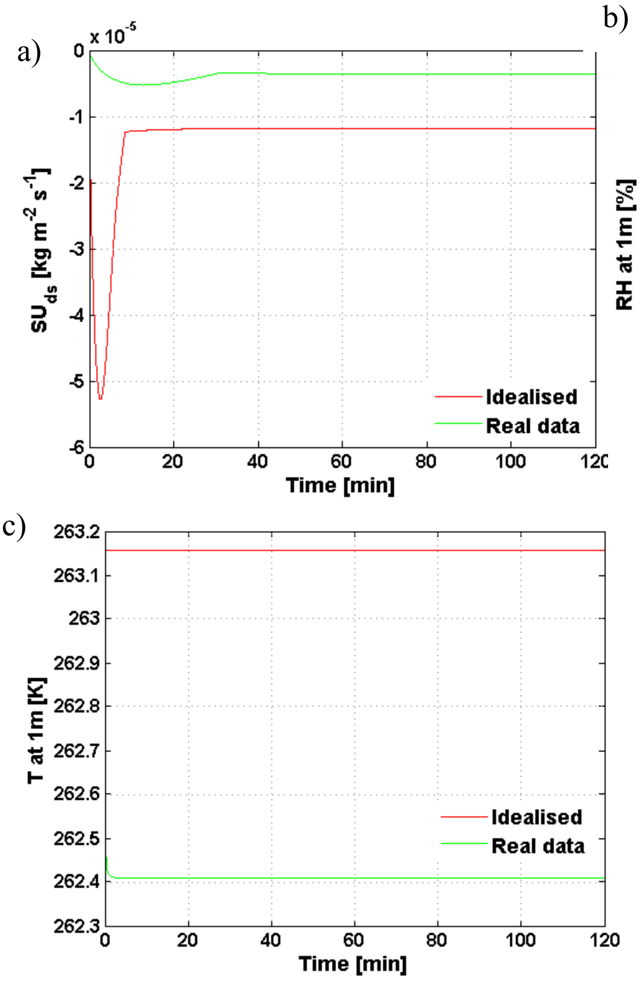


Figure 1: Evolution of (a) column integrated snowdrift sublimation $SU_{ds}$, (b) relative humidity with respect to ice at approximately 1 m above the surface and (c) air temperature at approximately 1 m above the surface for a 2h simulation with the idealised theoretical model version and the model applied to the case of 11 February 2009 (10-12 GMT) at AWS 16 - Princess Elisabeth.

Given the increase in $RH_i$ at AWS 16 during snowdrift, it is likely that $SU_s$ is, at least, strongly dampened during snowdrift. Unfortunately, with the AWSs used in this study, it is difficult to catch this effect, since the drifting snow layer usually stays below the sensor level. Therefore, we cannot prove but only assume that $SU_s$ vanishes at the onset of snowdrift. The considered sentence was rephrased to: "In that case, $SU_s$ is assumed to vanish and column-integrated snowdrift sublimation $SU_{ds}$ is estimated as the average of three parameterisations: ...".

> **Specific comment 3**: "Page 1499, line 15: Please explain why the roughness length was set to 1 mm; 0.1 mm was used for the surface roughness in the SMB calculations (page 1497, line 20)."

Roughness lengths are known to increase at the onset of snowdrift due to enhanced momentum dissipation, lower temperature and increased saturation levels (page 1499, lines 1-4). Consequently, when calculating the 10 m wind speed during snowdrift (a required input for the DY01 parameterisation), it is advisable to use a momentum roughness length ($z_{0,V}$) adapted to snowdrift conditions instead of the default $z_{0,V}$. A momentum roughness length of 1 mm is adopted because (i) it is the value used by DY01 in their parameterisation, and (ii) this value reasonably agrees with the mean increased roughness lengths parameterised by BR01 for each station (0.64 mm, 0.87 mm, 1.2 mm and 1.8 mm at AWS 9, 16, 6 and 5, respectively). Section 2.2 (paragraph 5) was adapted to clarify this issue.

> **Specific comment 4**: "Page 1504, line 14: Assuming that the ERds is negligible, is the precipitation amount obtained as the residual processes reasonable? Since the precipitation is hard to measure directly, particularly in the drifting, comparisons with the other studies and detailed discussions are significant."

As the referee correctly points out, solid precipitation is difficult to measure directly in Antarctica given the horizontal snow transport. Modelling studies using regional climate models, global circulation models or reanalysis data can provide an outcome to this problem by directly simulating moisture transport and sedimentation across the Antarctic ice sheet. Note however that associated model uncertainties need to be taken into account when model output is used for comparison. Table 2 depicts annual solid precipitation estimates near AWS 16 derived from several studies covering the entire ice sheet. Only one study was found where corrected rain gauge data were spatially interpolated (Legates and Wilcott, 1990), however, due to the scarcity of stations Antarctica, the predicted values strongly deviate from other predictions, especially in Dronning Maud Land where they are believed to be unreliable.

It is found that the annual solid precipitation calculated at AWS 16 for the 2-year measurement period (166 mm w.e. $yr^{-1}$) reasonably agrees with the estimates found in other studies. Also, solid precipitation estimates during 2009 (261 mm w.e.) and 2010 (71 mm w.e.) generally agree with these studies, although the solid precipitation for 2010 appears below the lower end of the estimates for the area. Note however that the microtopography around AWS 16 is not accounted for in these studies, due to their low spatial resolution. A detailed discussion on the accumulation regime at AWS 16 is provided in Gorodetskaya et al. (in review). Section 3.3 (paragraph 3) was extended as to include this reasoning, and references to Jaeger (1976), Bromwich et al. (2004) and Van den Broeke and Van Lipzig (2004) were added.

| Estimate | Period | *PR* (mm w.e. $yr^{-1}$) | Source |
|---|---|---|---|
| Jaeger | 1931-60 | 200-300 | (Jaeger, 1976) |
| Legates and Wilcott | various | 1000-1500* | (Legates and Wilcott, 1990) |
| NCEP2 | 1979-99 | ~200 | (Bromwich et al., 2004) |
| ECT | 1991-99 | 100-150 | (Bromwich et al., 2004) |
| Bromwich et al. | 1985-99 | ~150 | (Bromwich et al., 2004) |
| Van den Broeke and Van Lipzig | 1980-93 | ~200 | (Van den Broeke and Van Lipzig, 2004) |

Table 2: Annual solid precipitation estimates for the region of AWS 16. *: Estimate based on spatial interpolation of corrected rain gauge data.

**Specific comment 5**: "Page 1504, line 23: Sum of SUs + Suds increased much from 2009 to 2010 in AWS 16. This trend was also shown at other AWS stations?"

Table 3 shows the relevant mass fluxes calculated for 2009 and 2010 at AWS 5, 9 and 16. Indeed, at AWS 16 the sum of $SU_s$ and $SU_{ds}$ doubled in 2010 compared to 2009. The fraction of solid precipitation removed by sublimation increases even more dramatically from 4% in 2009 to 31% in 2010 owing to strong decrease in solid precipitation (being the residual if $ER_{ds}$ is assumed negligible). To highlight this dominance of reduced solid precipitation, the sentence was rephrased to: "The fractional removal increased from 4% in 2009 to 31% in 2010, indicating enhanced (summer) sublimation but especially limited accumulation during the second year at this location."

Contrary to AWS 16, no significant difference can be noted for total sublimation during 2009 and 2010 at AWS 5 and 9 (at AWS 6 no data is available during this period). Moreover, whereas solid precipitation only slightly decreased at AWS 9, it nearly doubled at AWS 5 (Table 3). Consequently, the difference at AWS 16 is not found for other stations, and it is not possible to claim that one of both years was exceptional in terms of sublimation or accumulation for whole Dronning Maud Land. This statement was added to the manuscript (Sect. 3.3, paragraph 4).

| | AWS 16 | AWS 5 | AWS 6 | AWS 9 |
|---|---|---|---|---|
| **Sublimation 2009 (mm w.e. yr$^{-1}$)** | -9 | -46 | / | -2 |
| SUs (mm w.e. yr$^{-1}$) | -3 | -15 | / | -1 |
| SUds (mm w.e. yr$^{-1}$) | -6 | -30 | / | -1 |
| **Sublimation 2010 (mm w.e. yr$^{-1}$)** | -18 | -43 | / | -1 |
| SUs (mm w.e. yr$^{-1}$) | -11 | -15 | / | 0 |
| SUds (mm w.e. yr$^{-1}$) | -8 | -28 | / | 0 |
| **Residual 2009 (mm w.e. yr$^{-1}$)** | 261 | 151 | / | 88 |
| **Residual 2010 (mm w.e. yr$^{-1}$)** | 71 | 300 | / | 75 |

Table 3: sublimation and residual mass fluxes during 2009 and 2010.

**Specific comment 6**: "Page 1501, line 21 & Fig. 2: Although the SEB model does a good job in general, there exists a trend that Tmodel becomes higher than the Tobserved, particularly at low temperatures. Do you have any explanations?"

The overestimation by the SEB model of $T_{s,obs}$ for very cold conditions was observed for the first time by Kuipers Munneke et al. (2009) when applying the same SEB model to Summit, Greenland. It has been noted that the deviation of the SEB model from observations is stronger for clear-skies compared to cloudy conditions. For instance, during the coldest conditions at AWS 16 ($T_{s,obs}$ < -50 °C), observed $LW_{in}$ rank among the lowest values of the whole measurement period ($LW_{in} < 95$ W m$^{-2}$), pointing to the absence of clouds. On the one hand, this could mean that the observations of certain variables are biased under very cold, clear-sky conditions. It is known that such conditions present a huge challenge to most AWS equipment. On the other hand, it is also possible that the model performs less well during cold, clear-sky conditions. Possibly, this could be due to the typical, decoupled surface layer which one can observe in reality during these conditions. If the standard Monin-Obukhov similarity theory is used to represent this situation, the downward $SHF$ tends to be overestimated, causing the additional warming of the snow surface in the SEB model. The considered paragraph was extended to include this reasoning.

**Specific comment 7**: "Table 1: I suppose the snow density shown here is the measurements at a specific time and place. It doesn't change much during the observation period and its variation does not give large effect on the following calculations?"

The snow density ($\rho_{snow}$) at AWS 16 was retrieved from the first 85 cm of a 4 m deep ice core drilled in February 2009 at a distance of 1 km East of Utsteinen ridge (Table 4). The sudden increase in snow density around 85 cm suggests the presence of a more compact snow layer, presumably accumulated the previous year. Unfortunately, the temporal variation of the $\rho_{snow}$ during the observation period cannot be assessed since no follow-up cores were drilled in the subsequent years. Fortunately, neither the calculation of $SU_s$ , nor the computation of $SU_{ds}$ depend on $\rho_{snow}$. On the other hand, the instantaneous surface mass balance linearly scales with $\rho_{snow}$, since $SMB = \rho_{snow} \Delta H_{snow} / \Delta t$ (Page 1944, lines 25-26). This way, indirectly, $\rho_{snow}$ also influences the residual mass flux. In agreement with Van den Broeke et al. (2004), and given the density fluctuations observed in the upper snow layers at AWS 16, we estimate that this leads to an uncertainty of ~15% in the obtained instantaneous surface mass balance values. This uncertainty estimate was added to section 2.2 (last paragraph).

| **Depth [m]** | **Density [kg m$^{-3}$]** |
|---|---|
| 0 – 0.17 | 307 |
| 0.18 – 0.34 | 339 |
| 0.35 – 0.42 | - |
| 0.43 – 0.64 | 359 |
| 0.65 – 0.84 | 333 |
| 0.85 – 1.04 | 411 |
| 1.05 – 1.16 | 391 |

Table 4: snow density profile sampled in February 2009, 1 km East of Utsteinen ridge. The density value for 0.35 - 0.42 m depth was unreliable and thus removed from the profile.

**Specific comment 8**: "Fig. 10: Although the wind directions are shown with different colors in addition to the notices with arrows, distinction between the katabatic and the synoptic is not clear."

We admit that there is no clear subdivision of the data cloud into a katabatic and a synoptic cloud. Rather, a general transition zone can be noted. However, despite of the presence of this transition zone, a cluster of winds of easterly directions (katabatic regime) is clearly visible for wind speeds ranging from 3 to 5 m s$^{-1}$ and air temperatures from 225 to 250 K (or $RH_i$ from 15 to 45 %, respectively), while a cluster of southerly winds (synoptic regime) appears for wind speeds ranging from 5 to 15 m s$^{-1}$ and air temperatures from 255 to 267 K (or $RH_i$ from 80 to 100 %, respectively). As for extreme winds (> 15 m s$^{-1}$), the demarcation is clear (above versus below 252 K, saturated versus undersaturated). Section 3.5. (paragraph 6) was adapted to clarify this issue.

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