Responses to comments of Reviewer A (E.L. Andreas)

The authors wish to thank E.L. Andreas for constructive comments which improve the manuscript. In this response, the original comments are in italics.

General Comments:

1) Scientifically, the manuscript is authoritative and has a fairly comprehensive review of relevant literature over snow-covered ground but ignores the equally relevant literature on snow processes over sea ice. The sublimation results must be taken on faith because I do not believe that any models of snow sublimation have been fully validated with data. The authors briefly mention one such attempt here but have only seven data points. As a result, the authors need to discuss the uncertainty in their results and place some error bars or other such uncertainty limits on their calculations.

A: We have added some relevant references on snow processes over sea ice in the revised manuscript. We agreed that models of snow sublimation have not been fully validated with data because of the paucity of observations. In principle, mesoscale ensemble seasonal simulations using our coupled blowing snow-atmospheric model can be performed to obtain uncertainties on the sublimation results. However, this has not been done because of the limitation in our computing resources. Instead, we added a new Section 6 to caution the readers on some of the limitations and uncertainties of our results.

2) The manuscript is a bit weak grammatically. The proofreading is good—I found few typographical errors. But punctuation, paragraph structure, syntax, and consistency in spelling could be improved. The authors might want to solicit help from a competent grammarian.

A: We have carefully checked the grammar and have improved the punctuation, paragraph structure, syntax, and consistency in spelling.

Specific Comments:

1) In the paragraph on page 931 that begins with line 12, the authors make some general statements about turbulent exchange over snow that tend to be simplistic and inaccurate. Contrary to implications here, we know quite a bit about parameterizing the turbulent exchange of heat and moisture over snow-covered surfaces from measurements over snow-covered sea ice in winter. See the following papers for discussions, data, and analyses pertinent to estimating surface sublimation from a snowpack:

*Andreas, E. L, 2002: Parameterizing scalar transfer over snow and ice: A review. Journal of Hydrometeorology, **3**, 417–432.*

*Andreas, E. L, R. E. Jordan, and A. P. Makshtas, 2005: Parameterizing turbulent exchange over sea ice: The Ice Station Weddell results. Boundary-Layer Meteorology, **114**, 439–460.*

Andreas, E. L, P. O. G. Persson, R. E. Jordan, T. W. Horst, P. S. Guest, A. A. Grachev, and C.W. Fairall, 2008: Parameterizing turbulent exchange at a snow-covered surface. Proceedings, 65th Eastern Snow Conference, Fairlee, VT, 28–30 May 2008, 65–72.

*Andreas, E. L, P. O. G. Persson, R. E. Jordan, T. W. Horst, P. S. Guest, A. A. Grachev, and C.W. Fairall, 2010: Parameterizing turbulent exchange over sea ice in winter. Journal of Hydrometeorology, **11**, 87–104.*

A: We added some references which demonstrated that turbulent exchange parameterizations have good properties under stable stratification conditions. The content between line 12 and 21 of paragraph 2 on page 931 was modified as the following:

Over snowpacks, very few direct measurements of turbulent fluxes exist for validation of snow sublimation estimates. There are indirect estimates of turbulent fluxes and parameterization using bulk transfer and flux-gradient techniques, especially over snow-covered sea ice in winter (Andrea 2002; Andrea et al. 2005; 2008; 2010). Those factors contributing to the difficulties and problems in estimating turbulent exchange from bulk transfer and flux-gradient techniques include stability and small, uncertain exchange coefficients. Typically, snowcovers have low thermal conductivities and high albedos and emissivities, and a snow surface can be very cold, especially at night. This results in increased stability and hence a reduction of turbulent mixing (Male, 1980).

The Monin-Obukhov similarity theory is generally used in stably stratified conditions. The roughness lengths for wind speed, temperature, humidity, and the stability function are thus important factors in estimating the turbulent fluxes. Several stability functions have been proposed (Webb, 1970; Lettau, 1979; Holtslag and de Bruin, 1988; Launianien, 1995; Vihma, 1995; King et al., 1996; Jordan et al., 1999; Andreas et al., 2005; Grachev et al., 2007) and Andreas (2002) found that the one developed by Holtslag and de Bruin (1988) yields the best results in terms of the calculation of the Richardson, Deacon, and turbulent Prandtl numbers in the limit of extreme stable stratification. Nevertheless, radiative flux divergence in the atmospheric surface layer may cause problem in the application of the Monin-Obukhov similarity theory and limit the universal application of the Holtslag and de Bruin (1988) stability function. More field measurements and further study on turbulent fluxes are therefore warranted.

2) A related issue here is a confusing statement about eddy diffusivities:
"Uncertainty in the exchange coefficients is further complicated by the inequality of eddy diffusivities for latent and sensible energy and momentum . . . ."
I read this to mean that the eddy diffusivity for water vapor is different than for temperature, and both are different than the diffusivity for momentum. Most authorities use the same diffusivities for water vapor and temperature in both stable and unstable stratification. This scalar diffusivity is, however, generally taken to be different than the diffusivity for momentum. Andreas (2002; listed above), for example, reviews formulations for the diffusivities in stable stratification and assumes the similarity of the diffusivities for temperature and water vapor. The following is a recent analysis, based on a very large data set, of the turbulent diffusivities in stable stratification:
*Grachev, A. A., E. L Andreas, C. W. Fairall, P. S. Guest, and P. O. G. Persson, 2007: SHEBA flux-profile relationships in the stable atmospheric boundary layer. Boundary-Layer Meteorology, **124**, 315–333.*

A: The original statement was confusing and we have made it clear that the diffusivities for temperature and water vapor are the same, and the same diffusion coefficient temperature and water vapor is used in our model. The modified sentence now reads as follows.

The uncertainty in the exchange coefficients is further complicated by the fact that the eddy diffusivities for temperature and water vapour are the same but are different from that for momentum, and by low turbulence due to the extreme aerodynamic smoothness of snow surfaces (Male and Granger, 1979). Grachev et al. (2007) give a comprehensive review of the diffusivities in stable stratification based on a very large data set.

3) Still on page 931 and the next page.

Start a new paragraph at line 24 with the sentence that begins "Surface sublimation may contribute . . . ."The three paragraphs that start with this sentence (and continue through line 22 on page932) are quite jumbled. The examples of surface sublimation, blowing snow sublimation, and snow transport in the three paragraphs, respectively, seem to be a potpourri of random facts with little coherence. A strong topic sentence at the beginning of each paragraph and organization that does not mix Arctic observations, Antarctic observations, and prairie observations in adjacent sentences would aid understanding. These two references might also provide some additional values for the sublimation from snow:

*Andreas, E. L, R. E. Jordan, and A. P. Makshtas, 2004: Simulations of snow, ice, and nearsurface atmospheric processes on Ice Station Weddell. Journal of Hydrometeorology, **5**, 611–624.*

*Persson, P. O. G., C. W. Fairall, E. L Andreas, P. S. Guest, and D. K. Perovich, 2002: Measurements near the Atmospheric Surface Flux Group tower at SHEBA: Near-surface conditions and surface energy budget. Journal of Geophysical Research, **107** (C10), SHE 21-1–SHE 21-35. (DOI: 10.1029/2000JC000705).*

Both report the latent heat flux rather than the sublimation rate, but you can easily convert these measurements to sublimation rate. Moreover, the Persson et al. reference describes a year of data and also includes Table 6, which summarizes estimates of annual latent heat flux from several other Arctic sea ice sites.

A: We have reorganized these three paragraphs as suggested, and added some related references regarding to surface sublimation. The modification has been made from line 24 on page 931 to line 22 on page 932 as following:

Surface sublimation may contribute either positively or negatively to the mass budget, which depends on the humidity gradient between surface and the air above. Male and Granger (1979) showed with lysimeter and profile observations over continuous open snowfields in the Canadian Prairies that surface sublimation was smaller than 0.2 mm day^{-1}. Such small values resulted because sublimation during the day was compensated by condensation at night. Some studies showed that surface sublimation can return a significant amount of the snow mass to the air over high altitude regions. For example, Hood et al. (1999) reported that 15% of the precipitation at an alpine site in the Colorado Rocky Mountains was lost to surface sublimation over the winter season. Over the Greenland ice sheet, the total annual sublimation was estimated to be about 1.85×10^{14} kg yr^{-1}, corresponding to 23% of the annual precipitation (Box and Steffen, 2001). Note, however, that this is due to a combination of surface and blowing snow sublimation. On the other hand, studies over high latitude regions revealed that surface sublimation has small values, and negative sublimation (hereafter deposition) is observed to occur. For example, over Arctic pack ice, the annual surface sublimation was showed to have small values around 0.03 mm SWE from various studies (Ebert and Curry, 1993; Lindsay, 1998; Persson et al., 2002). Deposition was found to occur in winter and early spring, whereas sublimation was showed to occur in the summer season (Persson et al., 2002). Over the Antarctic, Kameda et al. (1997) reported a downward water vapor flux onto the surface of 5.5 kg m^{-2} at the Dome Fuji observation site. Similarly, King et al. (2001) observed small amounts of deposition during the winter at Halley, Antarctica; whereas surface sublimation can remove around 10% of the precipitation at the same location in summer. Andreas et al. (2004) also demonstrated that deposition dominated most of the time during days 56-150 in 1992 at Ice Station Weddell in Antarctica, with the latent heat flux of 1-3 Wm^{-2}, equivalent to a daily sublimation of 0.03-0.09 mm SWE.

When surface wind speeds surpass a certain threshold, blowing snow may occur. Blowing snow particles undergo a phase change from ice to water vapor if the air is sub-saturated with respect to ice. Some of the snow mass on the ground can be returned back to the air through blowing snow sublimation, which has been studied at various sites over the Canadian Prairies, and various high altitude and high latitude regions. In the Canadian Prairies, blowing snow sublimation can amount to 29% of the solid precipitation and the measured sublimation rate can be as high as 1.2-1.8 mm day^{-1} during blowing snow events (Pomeroy and Essery, 1999). At a high terrain region in southeastern Wyoming, Schmidt (1982) estimated that 39% of transported snow will sublimate. Over an Arctic site, Pomeroy and Li (2000) reported that, on average, 22% of the solid precipitation will be eroded by blowing snow sublimation. In Antarctic coastal regions, observations indicated that the annual blowing snow sublimation can be as much as 170 mm SWE (Snow Water Equivalent) (Bintanja, 1998). All these studies indicated that blowing snow sublimation is a non-negligible process in the winter water mass budget.

In blowing snow transport, surface inhomogeneity and wind speed accelerations can redistribute snow on different spatiotemporal scales. The surface snow mass change due to blowing snow transport thus has different contributions to the hydrology and climatology, depending on the scales considered. Over the Canadian Prairies, redistributed snow has been shown to be important for fresh water management (Pomeroy and Essery, 1999). In alpine regions, small scale snow redistribution plays an important role in snow packing and the formation of avalanches. At high latitude regions, the mass balance from snow transport over the Greenland and the Antarctic ice sheets can be vital in the study of global sea level changes (Cuffey and Marshall, 2000; Alley et al., 2005).

4) Line 3 on page 936 makes brief reference to a wind speed threshold for blowing snow and directs readers to Li and Pomeroy (1997) for the parameterization for this threshold. A brief description of this parameterization here would make the current paper more complete.

A: Modified as suggested. A brief description to the parameterization for wind threshold has been added to line 3 on page 936. Since we added the new equation in the article, the subsequent equation numbers have been modified correspondingly.

$$U_t = 6.98 + 0.0033(T_a + 27.3)^2 \qquad (1)$$

Here, T_a is the 2-m air temperature and U_t is a threshold for the wind speed at 10 m height. This empirical equation was developed from six years of hourly meteorological data from 16 Canadian prairies stations. It demonstrates that the threshold wind speed has a close relationship with the surface air temperature. If the temperature is relatively warm, say greater than -10ºC, cohesive forces increase dramatically with increasing temperature due to the increase of liquid water surrounding the snow crystals. If the temperature is very cold, elasticity and kinetic friction increase with the decreasing temperature, resulting in increased shear stress to initiate snow transport. Therefore, the intermediate temperature range -30ºC<T_a<-10ºC would provide favorable conditions for snow transport. We emphasize that, computationally, PIEKTUK-T is much more efficient than the spectral PIEKTUK model (Déry et al., 1998) and is therefore the preferred choice for coupling to the atmospheric model MC2.

5) In many places, the paper describes changes in snow water equivalent (SWE), but the time interval over which the change occurs is not obvious. This imprecision limits the usefulness of the results. The first paragraph on page 944 is one instance with several numbers for the change in SWE but not a statement of the period over which the change is occurring. Figures 8, 9, 11, and 13 are other cases that report a change in SWE, but the only stated period for the change is the "winter season." Including simulation dates in the figure captions or other information about the length of the "winter season" would allow readers to estimate the sublimation rate, which, for me at least, is a quantity I am more comfortable with. (It translates to a latent heat flux, for example.)

A: The time interval for Simulation 2 is DJF 2006/2007, which has been added to the text and the corresponding figure captions (Figures 6, 7, 8, 9, 11 and 13).

6) The discussion about trees and vegetation in the middle of page 945 is pretty much handwaving. The conclusion seems to be that the larger roughness over forests limits the wind speed, and these lower speeds lead to reduced sublimation from blowing snow. I agree that trees are large roughness elements and, therefore, have large roughness lengths and lower wind speeds above them as a result. This is not the point, though. The snow doesn't sublimate as effectively from such regions because the snow is on the ground within the forest canopy, and the winds are greatly reduced within the canopy. That is, the winds don't reach the blowing snow threshold at the snow surface because of the protection given by the trees.

A: Yes, we agreed that the forest canopy will protect against reaching the blowing snow threshold. This is the main reason to reduce the blowing snow sublimation. The spatial distribution of vegetations and roughness lengths was added as Figure 9, and the subsequent figure numbers have been changed correspondingly. The text between line 11 and line 20 on page 945 has been modified as following:

Vegetation data from the US Geological Survey (USGS 2002), which are used in our simulation, indicate largely evergreen needle-leaf trees and mixed wood forests over the Canadian boreal forests, and deciduous needle-leaf trees over Siberia (Fig. 9a-c). These tall trees result in larger roughness lengths over forested area above the trees than over the tundra and prairies (Fig. 9d). Within the forest canopy, winds are greatly reduced and do not reach the wind threshold for blowing snow for surface snow within the forest canopy. Indeed, if a fully operational land surface scheme such as CLASS (Verseghy, 2000) is used, the presence of vegetation would cause sub-canopy wind speeds to be extremely low resulting in almost no blowing snow transport or sublimation where there are forests (Pomeroy et al., 1999).

7) The results rely heavily on the blowing-snow sublimation model within PIEKTUK. Measuring sublimation from blowing snow is very difficult; hence, the parameterization for it in PIEKTUK is largely theoretical. If any validation of this parameterization exists, please describe it. Page 949 mentions one such comparison; but there are only seven data points, so the results are not very compelling. In light of the faith we must thus place in an untested theoretical model, the authors need to discuss the uncertainty in their results. Can they place error bars on any of their simulated results. In Table 3, for example, they report simulation results to 5–6 significant figures. There is no way they can have confidence in their results to 1 part in 10,000; including uncertainties with these calculations would give readers a better appreciation for their meaning.

A: We agreed that there should be more comparison of model results with observations but there are very limited blowing snow sublimation measurements. In this article, we compared the simulated blowing snow sublimation from PIEKTUK-T against observation at Wyoming site with good results. As mentioned in the response to comment #1, a new Section 6 has been added to caution the readers on some of the limitations and uncertainties of our results. Specifically, the new section is as follows:

6 Discussion

Our three-month simulation provides spatial distribution of surface sublimation and blowing snow related processes and their contributions to the surface mass budget over the northern hemisphere. Because there are few blowing snow measurements for validation, we mention here some limitation and uncertainties in our results.

(1) The wind speed threshold to initiate blowing snow is calculated from a relatively simple expression. This formula is derived from the data over the Canadian Prairies, and is found to be a function of the temperature. However, there are other factors which affect blowing snow initiation and termination. For example, the character of the snow, such as snow age and snow compactness can also play a role, and the general applicability of the threshold expression to other sites still needs to be validated.

(2) The negative thermodynamic feedbacks in our model limit further blowing snow sublimation, resulting in relatively low sublimation rates of blowing snow compared to other snowdrift models.

(3) The horizontal grid resolution of the current simulation is 18 km, within which homogeneity is assumed. As such, fine scale variation in topography and surface characteristics are not resolved, as well as the micro-relief over sea ice associated with snowdrifts and pressure ridges. The relatively coarse resolution may also reduce the wind speed, and modify the snow transport and concurrent sublimation.

(4) In our current model, we neglected forest canopy effects and vegetation interception in snow transport. If these two factors were taken into account, the snow on the ground will be sheltered from transport and the transported snow will be partly intercepted by the forest. These factors will probably reduce the surface and blowing snow sublimation.

(5) Although lead fraction is crudely taken account into in the coupled model by its effect on the surface turbulent fluxes via a weighting factor, the role of sea ice leads as a direct sink of blowing snow transport is neglected in the model. Consideration of this subgrid scale process will decrease blowing snow transport.

(6) A stability function was used in the boundary layer scheme of MC2 to deal with stratification effects. However, in the blowing snow module, neutral stability is assumed in extrapolation of the wind profiles below first MC2 level. Additionally, the vertical turbulent transport of blowing snow follows the relatively simple representation appropriate for neutral conditions.

These uncertainties may change the magnitude of the simulated surface sublimation and blowing snow related processes, and modify their contributions to the snow mass budget. However, it is our belief that the overall characteristics of the spatial patterns would not be overly sensitive to these uncertainties. Since this experiment is the first to explicitly simulate blowing snow processes covering the entire northern hemisphere, it provides a useful reference for comparison with future studies of the snow mass distribution over large spatial and temporal scales.

8) The discussion of surface sublimation at the bottom of page 945 and the top of page 946 is simplistic and ignores modern results. The references listed above describe the current state of the art.

A: We described the expression for surface sublimation used in MC2, and some detailed explanation has been added to line 1 on page 946.

C_D is an integrated bulk transfer coefficient over the surface layer, determined by the surface roughness and a stability function

$$C_D = \frac{1}{k}\int_{z_0}^{z_a+z_0}\left(1-\frac{z}{h_e}\right)j\left(\frac{z}{L}\right)\frac{dz}{z} \qquad (8)$$

where k is Von Kármán constant and L is the Obukhov length, and z_0, z_a and h_e indicate the roughness lengths for moisture, surface height and boundary layer height for the stable case, respectively. In the coupled model, the stability function is calculated using a linear relationship with the local Richardson number for statically stable conditions. The term ($1-z/h_e$) is introduced to include the variation of fluxes with height within the surface layer, i.e., because the surface layer in atmospheric model often exceeds the lowest one tenth of the boundary layer (i.e, the constant flux layer). Note that positive/negative values of *E* indicate sublimation/deposition.

The modern results have been added to line 9 on page 946 as following:

The indicated deposition over Arctic Ocean is in agreement with the previous studies. Based on 45 years of surface meteorological observations from the drifting ice stations in the Beaufort and Chukchi Seas from 73 to 90ºN, the calculated latent heat flux indicated deposition for winter months, with the monthly averaged values around 1.1 Wm^{-2} (Lindsay, 1998). According to this latent heat flux, the accumulated surface deposition would be around 3 mm SWE for the three months DJF. Our simulated seasonal deposition over the Arctic Ocean has a similar value. Other studies also showed deposition over Arctic ice in the winter months (Maykut, 1982; Ebert and Curry, 1993; Persson et al., 2002; Andreas et al., 2010) with smaller values but similar order of magnitude.

9) The discussion at the bottom of page 946 and the top of page 947 explains that surface sublimation increases with latitude and that there can actually be deposition at very high latitudes. The explanation given—without reference—is that the near-surface air is near ice saturation and the air is very cold. The following reference establishes that, over sea ice at least, relative humidity with respect to ice is always very near 100%:

*Andreas, E. L, P. S. Guest, P. O. G. Persson, C. W. Fairall, T. W. Horst, R. E. Moritz, and S. R. Semmer, 2002: Near-surface water vapor over polar sea ice is always near ice saturation. Journal of Geophysical Research, **107** (C10), SHE 8-1–SHE 8-15. (DOI: 10.1029/2000JC000411).*

The "cold" is not a very good explanation, though. The above reference shows near-saturation for all temperatures between –40° and 0°C. Over sea ice, the saturation occurs because even a little open water (i.e., leads) loses enough vapor to saturate the boundary layer with respect to ice.

A: We thank the reviewer for this suggestion, and have included the reference for the explanation. The text between line 27 on page 946 and line 3 on page 947 has been modified as following:

The reason for the different behaviour of surface and blowing snow sublimation is that at very high latitudes, the near surface air is usually saturated with respect to ice though the moisture content is very low. Andreas et al. (2002) demonstrated that over sea ice, even a small fraction of leads and polynyas can provide enough water vapour to saturate the atmospheric boundary layer, resulting in the surface air near ice saturation or supersaturation for temperatures between -40ºC and 0ºC. The relative humidity with respect to ice was usually around 100% in winter, with much less variability than in summer. As a result, over the entire Arctic Ocean, the water vapor flux is downward leading to surface deposition.

Editorial Issues

1) The punctuation is a bit shaky. The authors often fail to separate independent clauses with a comma or semicolon. Here are some examples, where I have noted the required punctuation with square brackets.

Page 930, line 13: Blowing snow sublimation was found to return up to 50 mm SWE back to the atmosphere over the Arctic Ocean[,] while the divergence . . . .

A: Modified as suggested.

Page 931, line 18: Typically, snowcovers have low thermal conductivities and high albedos and emissivities[,] and a snow surface . . . .

A: Modified as suggested.

Page 932, line 29: As a result, numerical modeling has become a useful tool to complement field measurements in the study of the surface water mass budget[,] and a number of . . . etc, etc.
A: Modified as suggested.

2) Alternatively, the manuscript has many instances of punctuation that does not belong. In the following examples, I enclose in parentheses punctuation that should be removed.
Page 930, line 2: Many field studies have shown that surface sublimation(,) and blowing snow transport and sublimation . . . .
A: Corrected.
Page 931, line 13: Male and Granger (1979) showed . . . surface sublimation was smaller than 0.2 mm day–1(,) because sublimation during the day . . . .
A: Corrected.
Page 936, line 3: We emphasize that[,] computationally, PIEKTUK-T is . . . model (Dery et al. 1998)(,) and is therefore . . . . etc., etc.
A: Corrected.

3) The authors often misplace "only." Here are examples where I note the incorrect (i.e., current) position in parentheses and the correct position in square brackets.
Page 932, line 24: At high latitudes and remote regions, such as over Northern (sic) Canada, field observations are (only) available [only] infrequently . . . .
Page 933, line 28: Because version 3.2 of MC2 is not a parallel code, they were (only) able to perform a simulation of [only] 48 h duration . . . .
Page 936, line 8: Since blowing snow (only) reaches altitudes of [only] tens to a few hundred metres . . . . etc.
A: Corrected, and the same corrections have been made in other similar unlisted misplacements of "only".

4) Usage is at times inconsistent. For example, SWE is sometimes defined as Snow Water Equivalent and other times as snow water equivalent. Standalone is sometimes "stand alone" and other times "stand-alone." For example, see the caption and the legend in Figure 2.
A: Modification has been made to have the consistent usage of these terms.

5) In the caption for Table 3, define what "Percentage" is. Add degree symbols to the latitude bands.
A: Modified. The following has been added to the end of the caption of Table 3.
and Percentage is defined as the ratio of Sum to the total solid precipitation (Precip.).

6) In the captions for Figures 1 and 4, mention that the relative humidity is with respect to saturation over ice.
A: Modified as suggested.

7) In Figure 3, both lines are too thin—especially the dotted line.
A: We re-plotted this figure with thick lines in Figure 3.

8) Likewise, in Figure 12, the points need to be bigger—especially the blue points.
A: We re-plotted the figure with thicker marks in Figure 12.