Review of

A Triple-Moment Blowing Snow-Atmospheric Model and Its Application in Computing the Seasonal Wintertime Snow Mass Budget
(hess-2010-8)

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Overview

This manuscript combines a blowing snow model, PIEKTUK-T, with a mesoscale model, the Canadian Mesoscale Compressible Community Model (MC2), to simulate terms in the snow mass balance in the Northern Hemisphere. The model simulates precipitation, surface sublimation from the snowpack, sublimation from blowing snow, and transport. The manuscript reaches conclusion about the regions in the Northern Hemisphere where these processes are important and where they are negligible in the snow mass budget. The maps of these terms for the Northern Hemisphere are especially important.

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.

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.

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.

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.

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 page 932) 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 near-surface 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.

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.

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.)

6. The discussion about trees and vegetation in the middle of page 945 is pretty much hand-waving. 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.

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.

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.

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.

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 . . . .

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

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.

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 . . . .

Page 931, line 13: Male and Granger (1979) showed . . . surface sublimation was smaller than 0.2 mm day^{-1}(,) because sublimation during the day . . . .

Page 936, line 3: We emphasize that[,] computationally, PIEKTUK-T is . . . model (Dery et al. 1998)(,) and is therefore . . . .

etc., etc.

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.

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.

5. In the caption for Table 3, define what "Percentage" is. Add degree symbols to the latitude bands.

6. In the captions for Figures 1 and 4, mention that the relative humidity is with respect to saturation over ice.

7. In Figure 3, both lines are too thin—especially the dotted line.

8. Likewise, in Figure 12, the points need to be bigger—especially the blue points.

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