The Cryosphere Discuss., 4, C124–C134, 2010
www.the-cryosphere-discuss.net/4/C124/2010/

The Cryosphere Discussions

Interactive comment on "Modelling snowdrift sublimation on an Antarctic ice shelf" *by* J. T. M. Lenaerts et al.

J. T. M. Lenaerts et al.

j.lenaerts@uu.nl

Received and published: 16 April 2010

First of all, we would like to thank Andy Clifton, Richard Essery and the anonymous reviewer for their constructive and valuable comments, which will certainly improve the manuscript. In this response, we address their comments point by point.

Referee 1: A. Clifton

1. *I would like to see a flow chart describing exactly what data is obtained, from where and how it is moved between the tools used to generate the data. By tools I mean field observations, radiosondes, RACMO2/SCM and the drift model (presumably a version of Pietkuk, though not explicitly named). I feel that this would allow others to better understand what has been implemented, and how it might be applied using similar*

models.

A new flow chart (see below) will be added as new Figure 2B.

2. *A second comment is that the authors may wish to consider making available online some of the code they used to generate these results. If the drift model can accept input data from other numerical weather models or reanalysis data it could form a useful research tool to be applied to much of the Antarctic coastline. In this respect, a short discussion of the limitations of this approach would also add to the paper; can the approach be applied over sections of coastline, or is it limited to regions where radiosonde forcing data is available?*

Stephen Déry has developed the code (PIEKTUK-B), which is available on request. A technical description is available on his website (http://nhg.unbc.ca/). The adaptations that are made to the code are clearly indicated in the text, so in our opinion the reader has all necessary info and tools to reconstruct how we have implemented the code. The technique described in this paper is not solely applicable to places where measurements are available. To better explain this, we have added this line to the conclusions: "This technique is not only applicable to regions where radiosonde measurements are available: the input for the snowdrift routine can also come from a 3D meteorological model or reanalysis data, although this will introduce an additional uncertainty and does not allow feedbacks into the model."

3. *I am curious as to what impact the time resolution of the forcing data may have had on the results.*

Figure 7A shows the impact of the time resolution on the results. Here we have performed a test with the snowdrift parameterization of Stephen Déry (Déry and Yau, 2001, Bound. Lay. Meteor.), (a) using hourly observations, and (b) using the 12-hourly model input. The results show that the difference is relatively small (2%), which gives confidence that we can use these 12-hourly forcing data without losing too much contributions of the extremes. In addition, Figure 6 illustrates that the scatter of (a) and (b) is low (R^2=0.87) and relatively few extreme wind speeds are missed.

4. *I am also interested in the treatment of the snow surface, which is not as complex as the treatment of the boundary layer. This may be model constraints or may result from the lack of forcing data, and I would be interested to know more about the reasons behind this.*

The treatment of the snow surface indeed is an important issue. In the 1D model, the surface density is prescribed (estimated on the basis of the expression of Kaspers and others (2004, Atm. Chem. and Phys.), and after that is held constant. That implies that we also use a constant value for the threshold friction velocity $u_{*,t}$. Lacking information on temporal variations of surface snow density or $u_{*,t}$ from observations, we feel that the choice for a constant $u_{*,t}$ value is justified in this case. To make this clear, we have added the following line to the conclusions: "Further work will also include a dynamic surface density evolution to explicitly calculate the feedback between snowdrift and the surface snow structure. However, this will only be useful when more detailed observations of surface snow density and $u_{*,t}$ become available."

The apparent increase of z_0 with increasing u_* in observations is due to the fact that snowdrift extracts momentum from the near-surface air to keep snow particles in suspension. The real z_0, however, represents the shape and size of the roughness elements of the surface. Nonetheless, we have tested the sensitivity of the results to the inclusion of a dependency of z_0 to friction velocity. We have performed a 1-year simulation with a varying z_0 according to Bintanja and Reijmer, 2001, JGR, who proposed the relation:

$z_0 = 0.0039202.u_*^{2.1968}$

Including this relation yields a one-year (1997) average z_0 value of 0.00034 m (originally 0.00025 m) and an average u_* of 0.326 m/s, (originally 0.343 m/s). The resulting annual snowdrift sublimation decreased by only 2 mm, which is equivalent to 1.5% compared to the original value. Therefore, the influence of introducing the snowdrift-dependent

roughness length is only marginal. We have made this clear in the MS by including the following sentence: "Including a dependency of z_0 to friction velocity following Bintanja and Reijmer (2001) did not significantly change the results. For a 1-year simulation (1997), the average z_0 value increased from 0.00025 to 0.00034 m and the snowdrift sublimation decreased with 2 mm, which is only 1.5% of the original value."

Following the suggestion of the reviewer, we have also performed a sensitivity test in which the value of z_0 was changed permanently. Of course this directly leads to a change in friction velocity, (see table 2) and hence the snowdrift frequency and sublimation rates. However, if we compare the snowdrift frequency in these experiments to the observations, we find that agreement does not improve considerably compared to the original experiment. In the case of a lower z_0 value (0.000125 m), the frequency of snowdrift events decreases from 39% to 37%. The fact that the introduction of a snowdrift-dependent roughness length does not considerably change the result, gives additional confidence that we chose an appropriate roughness length.

5. *...the model significantly overestimates the horizontal mass flux, compared to the observations. I would appreciate the authors expanding on this, particularly the spectra of particle size in the model compared to the sensitivity of whatever measurement devices were used to quantify the drifting snow.*

A comparison in terms of particle size distributions between different numerical snowdrift models is given in detail in Xiao et al. (2000). Their results show that the particle mean radius in the model used here (PIEKTUK-B) is somewhat underestimated compared to the analytical solution, especially above the first meter above the surface. Xiao et al. (2000) also found a relatively high transport rate in PIEKTUK-B compared to other models, which may in part explain the overestimation seen in Figure 8. Yang and Yau (2007, Bound. Lay. Meteor.) compared modelled and measured particle spectra at Byrd station, which showed that the overall that the overall pattern of the distribution is well represented, but that there are discrepancies between model and measurements. We have added this paper in the references and the above line to the paper. Another possible reason for differences between model and observations could be the assumption that the wind speed follows a logarithmic function with height. As stability is not considered, transport rates could be overestimated in the model. The data from the particle impact sensor that are shown in the paper are described in Van As et al. (2007), and have an accuracy of 10% (Bintanja et al. 2001). Different quality checks have been performed on these data to eliminate e.g. periods with ice on the sensor (Van As et al., 2007). Unfortunately, the particle impact sensors do not provide data on particle size spectra. Applying a smaller, more realistic value of z_0 (0.0001 m) for the Kohnen run did not give a better result: the transport rate increased even more to maximally 0.2 $kgm^{-1}s^{-1}$. This demonstrates the sensitivity of the results to the value of z_0.

6. *My motivation is to understand what is needed to better understand and model the process of drift and drift sublimation in the field.*

We have made a thorough re-check on the exact formulation of measured and modeled and we have adapted the text where necessary. We have added the following text to the conclusions: "In view of the demonstrated sensitivity of the results to near-surface data, future field campaigns would benefit from accurate measurements of near-surface variables. Especially reducing the uncertainty in the relative humidity measurements would greatly improve snowdrift sublimation estimates. Additionally, direct validation data are needed, such as impact particle and sublimation sensors as well as optical particle counters to improve model estimates of snowdrift transport and sublimation."

7. *Technical corrections*

The proposed technical corrections have been changed in the text. Following the suggestion of the reviewer, "rescaling" is replaced by "corrected for a bias at low temperatures". Rescaling the colours in figure 1 would reduce the colour differences at the right side of the plot, which contains valuable information. Therefore, we would like to keep the present colour labels.

Referee 2: Anonymous referee

1. *I had difficulties in section 2 and 3 in the presentation of the tools and understanding well the strategy and the assumption made. In particular, there is no discussion on the quality of the results of the atmospheric model (used as physical interpolator) by comparison to the observations (also used as forcing data for the model), especially near the surface.*

We have now included a flow chart, which was also proposed by referee 1 (see above), to facilitate the understanding of how measurements and modelling were combined. We have added a comprehensive analysis of the quality of the interpolated results (see section 4.1, figure 6, figure 7A), which shows that the interpolated results match the original hourly observations very well. See also comment 3 of Referee 1.

2. *The assumption of a snowdrift layer of 7m must be more supported, for instance by observations, previous detailed model runs ...*

A 7 m high snowdrift layer is not assumed in this study. Rather, the lowest model level in RACMO2/SCM is at 7 m above the surface. The measured variables are linearly interpolated to model levels. The snowdrift routine uses a logarithmic vertical grid of 24 levels up to 100 m above the surface. This allows snowdrift to occur also above 7 m.

3. *I agree with the fact that the near surface wind speed forcing take into account the retroaction of snow drift on wind speed. But assuming $u_{*,t}$ constant means that there is no impact of the snow quality on snow drift, which is quite surprising (no feed back during snowdrift events). This leads to neglect an important effect of snowdrift.*

See comment 4 of Referee 1.

4. *Inter-annual variability (section 4.4): the figures given in the text could be gathered in a Table, in order to facilitate comparisons.*

We have chosen to illustrate the inter-annual variability using a figure rather than a table, to better visualize the year-to-year variations. We feel that a table would contain too much information so as to deteriorate readability.

5. *Technical comments*

The technical comments have all been taken into account in the revised manuscript. In the caption of figure 2, we have added that the lowest model level is situated at 7 m.

Referee 3: R.L.H. Essery

1. *I guessed immediately that the divergence of blowing snow transport would not be discussed in this paper, but I had to read on to realize that precipitation and runoff are not addressed either. These should be acknowledged in the discussion of SMB here.*

In the paper, the focus is on snowdrift sublimation, but surface sublimation is also explicitly quantified. Indirectly we can also quantify the other components of the surface mass balance. We use the accumulation of Schlosser et al. (2002), derived from stake measurements. The contribution of runoff due to melt is insignificant at Neumayer, where temperatures rarely reach the melting point. If we furthermore neglect horizontal divergence of drifting snow, we can reconstruct the precipitation amount at Neumayer (P = Acc+SU+SUds). We have added this information to the introduction and conclusions.

We have also added "modelled" before "SMB" on line 3, which was proposed by the referee.

2. *Does interpolating the 24-hour observations to 12 hour forcing not suppress any diurnal variation that the model would generate itself?*

The daily cycle is most important near the surface. The input at the two lowest model levels (7 m and 18 m) comes from hourly mast measurements. The model calculates the incoming shortwave radiation at the surface, which ensures that the daily cycle is well represented (see also figure 6). To make this more clear, the following sentence has been added to section 4.1: "The input at the lowest levels comes from hourly

mast measurements, and the surface energy balance is calculated in the model, which ensures that the daily cycle is well represented."

3. *Does the modelís momentum roughness length depend on snowdrift?*

See comment 4 of Referee 1.

4. *The "measured" energy balance components in Figure 5 are actually modelled.*

True, and we have replaced "measured SEB components" by "modeled SEB components using observations".

5. *What are the 3-h observations referred to here?*

The 3-hourly snowdrift observations are "synops", i.e. official WMO measurements performed at Neumayer (WMO, 1995). See König-Langlo and Loose (2007, Polarforschung) for further details. To make this clearer, the following sentence has been added to section 4.1: "The 3-hourly snowdrift observations are "synops", i.e. measurements performed at Neumayer according to WMO standards."

6. *In reality, the latent heat required for sublimation of drifting snow cools the air, not the surface. Does this not decrease the temperature gradient between the air and the surface and act to decrease the sensible heat flux into the surface, not increase it as stated here?*

In reality, sublimation cools the surface of the snow particles, setting up a temperature gradient between the snow particles and the air. This in turn sets up a sensible heat flux from the air to the snow particles, cooling the air and keeping the snow particles in thermal equilibrium with their surroundings. In the model, this cooling effect of snowdrift sublimation is prescribed to be extracted from the snow surface in the model. As a result, the skin temperature in the model decreases, the temperature gradient between the air and the surface will increase, causing a larger sensible heat flux into the surface, cooling the air throughout the boundary layer, which is realistic. To make this clearer, the following sentence has been added to section 2.3: "In reality, sublimation cools the surface of the snow particles, setting up a temperature gradient between the snow particles and the air. This in turn sets up a sensible heat flux from the air to the snow particles, cooling the air and keeping the snow particles in approximate thermal equilibrium with their surroundings. In the model, this cooling effect of snowdrift sublimation is simulated as follows: the heat needed for snowdrift sublimation is prescribed to be extracted from the model snow surface. As a result, the skin temperature in the model decreases, the temperature gradient between the air and the surface increases, enhancing the downward sensible heat flux, heating the surface and cooling the near-surface air."

For Kohnen we have used the same threshold friction velocity to make a fair comparison with Neumayer. There is almost no snowdrift sublimation simulated because, although the wind speed was high enough to generate snowdrift, the thermodynamic conditions for snowdrift sublimation were not favourable: during the strong snowdrift event the relative humidity was 100%. The model generates negligible amounts of snowdrift sublimation during this event. To clarify this, the following sentence has been added to section 4.2: "During the snowdrift event, relative humidity was 100%, so the amount of snowdrift sublimation was negligible".

7. *400* kg/m^3 *is very high for fresh snow density*

In fact, this is not the fresh snow density, but the density of the surface snow. This has been changed this in the text.

8. *Are what are marked as Obs snowdrift in B actually the predictions of snowdrift produced by the model driven with hourly observations, and not observations at all?*

No, these are the actual 3-hourly WMO observations of snowdrift which are also discussed in point 5) above.

9. *Technical corrections*

The proposed technical corrections are introduced into the revised paper. Figure 11f

has been changed according to the referee's proposition.

Interactive comment on The Cryosphere Discuss., 4, 121, 2010.

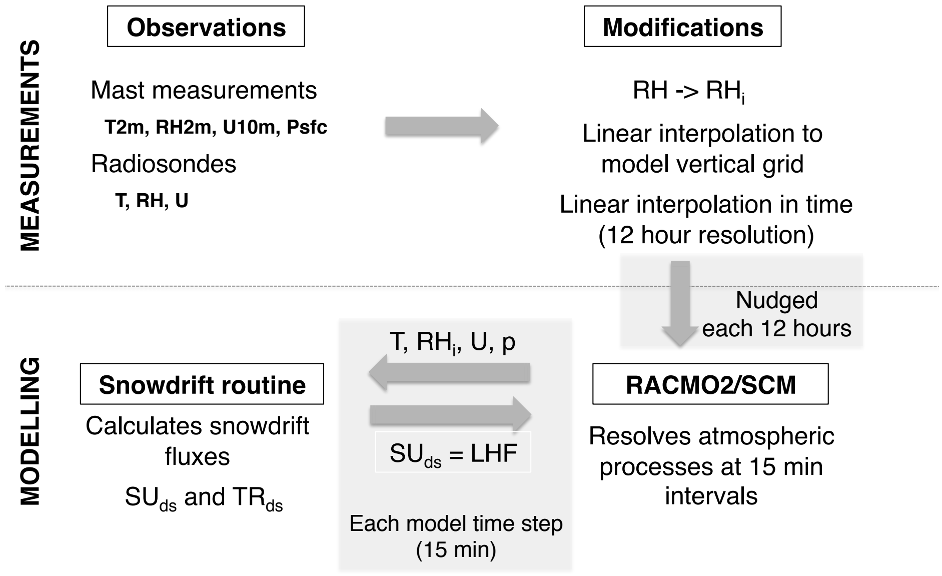
MEASUREMENTS
Observations
Mast measurements
T2m, RH2m, U10m, Psfc
Radiosondes
T, RH, U
Modifications
RH -> RHi
Linear interpolation to model vertical grid
Linear interpolation in time (12 hour resolution)
Nudged each 12 hours
MODELLING
Snowdrift routine
Calculates snowdrift fluxes
SUds and TRds
T, RHi, U, p
SUds = LHF
Each model time step (15 min)
RACMO2/SCM
Resolves atmospheric processes at 15 min intervals

Fig. 1.