The Cryosphere
Discussions

Interactive
Comment

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

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1   General Comments

The paper of Lenaerts et al. is a valuable demonstration of how improvements in measurement, modelling and understanding of complex physical process in the cryosphere can improve our ability to model Antarctic surface mass balance (SMB). The particular focus of this paper is the contribution of drifting snow to the SMB, and controls on the contribution of drift. By focussing on an area with a long history of detailed boundary layer measurements, they have been able to apply and evaluate a complex modelling process to the SMB.

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The authors' analysis shows that snow drift is a frequent event and an important contributor to the SMB at Neumayer, East Antarctica, and that sublimation of the drifting snow is an important control on this. Furthermore, relative humidity differences drive this sublimation but their results are very sensitive to the accuracy of the input data. While their results show the potential for coupled atmospheric and boundary-layer process models, they also show that these models may have temporarily surpassed our ability to force them and evaluate the results.

2 Specific Comments

My first comment is not unique to this paper, but is a common issue when dealing with modelling or data assimilation processes; 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 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 coast line, or is it limited to regions where radiosonde forcing data is available? I suspect this is available through other work relating to RACMO2/SCM, but would appreciate its inclusion.

I am curious as to what impact the time resolution of the forcing data may have had on the results. Many studies have shown that the relation between wind and drift

transport rates are highly non/linear. Because of this, shorter integration times and higher/resolution forcing data are generally preferred as these lead to more accurate results. This is partly shown in Table 2, where an 0.5 m s^{-1} change in wind speeds leads to a marked change in results. It would also be interesting to know the mean values of the variables in Table 2, not just the adaption.

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. For example;

- The surface roughness length for momentum is taken as a constant (p. 127). The authors acknowledge that this is a representative value. However, a link has been seen before between the roughness length for momentum z_0 and the friction velocity u_* for loose surfaces (see e.g. Owen, 1964). This has been observed over snow surfaces and is even referred to in some of the references that are cited (e.g. Bintanja, 1995 or König, 1985). It may be therefore be worth adapting z_0 by ± 1 order of magnitude and including this as a sensitivity measure in Table 2.

- I am interested to see that the authors have separated the roughness lengths for momentum and heat (p. 127), and would again like to know what the influence of this was, compared to taking momentum and heat roughness lengths as equal.

- The new snow density given in Table 1 (400 kg m^{-3}) strikes me as large, although it has been observed that Antarctic surfaces can rise to this level after some time or exposure to drift; do the authors mean 'surface snow density'?

For these reasons, I would appreciate the authors providing a short discussion of the value of improved snow surface information and the potential to include more complex surface models, such as SNOWPACK or SNTHERM, amongst others.

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On page 131 and in Figure 8, the authors present a comparison with data collected at Kohnen base by Van As et al. as part of the ENABLE campaign. They show that the model significantly over-estimates 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. I applied a correction technique in analysing data from a drift sensor, reported in *Earth Surface Processes and Landforms (2008)* which may be of relevance. I would also be keen to know more about the particle size distribution in the model, as this is of general interest.

When discussing the snowdrift climate (Section 4), I would appreciate greater clarity as to what is observed or measured directly, what is modelled using the drift model and what is obtained from the RACMO2 model. For example, p.132 line 7 should (I suspect) be 'At Neumayer, *modelled* horizontal transport...'.

I would like to expand on the reasons for often asking for additions to Table 2. My motivation is to understand what is needed to better understand and model the process of drift and drift sublimation in the field. Is it improved (or more) particle sensors for model validation, better humidity measurements to bracket the sublimation or multi-level wind sensors to improve wind field data? What would assist the authors in addressing this important question?

3 Technical Corrections

p. 122 l. 7 'The site is characterised by a relatively mild, wet and windy climate, so snowdrift is a common phenomenon.' Relative to what? Also, consider clarifying that you mean that mild and wet conditions allow frequent snowfall, which is then transported by the wind. Mild and wet would not result in snow drift.

p. 123 l. 19 Why is the value for 15-20% from Bintanja 'overestimated'?

p. 126 l.6 What is the 'rescaling' of relative humidity?

p. 133 l. 6 The first sentence of section 4.4 is confusing. Please rewrite.

Table 1 'Altitude', not 'height'

Fig. 1 Please rescale to use the same colours on each plot

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