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The Cryosphere Discussions

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

Anonymous Referee #2

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General comments

The authors address the issue of snowdrift sublimation on two locations of the Antarctic continent, with a specific focus on the Neumayer site. Snowdrift sublimation is one of the terms determining the Antarctic surface mass balance (SMB), which is poorly known. Reducing the uncertainty on snowdrift sublimation will benefit to estimation of the SMB. The authors use a surface snowdrift model forced by analyzed data at a relatively low level (7m) over a long period. They made a significant effort for the building of a relevant atmospheric data base. They found that the snowdrift sublimation correspond to 16

The paper and the conclusions are interesting. My main recommendation is to strengthen the parts 2 and 3, which are not very straightforward for the reader. Some

hypotheses, such as the fact that the snowdrift layer is only 7m high must be supported by a proper discussion. One option for a reorganization of this paper could be to clearly separate the work done to obtain the near surface forcing (including validation), then present validations with snowdrift, climatological results and finally and enhanced discussion on uncertainties and sensitivities to key parameters.

Specific comments

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. Results are shown in Section 4.1 (Fig 6.). Fig4. shows the climate of the model up to 5 km, which is not very useful. I think a discussion on the uncertainty associated to the forcing data must be included in section 2 or 3. The discussion on sensitivity to measurement errors (section 4.6) can be improved based on error estimates obtained at this stage.

I understand that the atmospheric model does not simulates snowdrift. Although it is said that the forcing data contains implicitly the effect of snowdrift, this is not the case in the model, in particular in the exchange coefficients between the surface and the first layer in the model. It is a possible source of discrepancy in the first level data, I invite the authors to comment on that.

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

Page 126, first line, u*,t is assumed constant. 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. As shown in table 2 u*,t has a quite strong influence

on the results

Interannual varaibility (section 4.4) : the figures given in the text could be gathered in a Table, in order to facilitate comparisons.

Technical issues :

Table 1 : add “at Neumayer” in the legend.

Fig 2 : please clarify the location of the lowest model level, is it identical to the top of the surface layer ?

Fig 7 : 4th line of the legend : B: Trds : replace by D: Trds.

Check all Figures legends in order to explicitly state the full name and abbreviations in the legend

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