Author reply to the comments of E. Morris on "Event-driven deposition: a new paradigm for snow-cover modelling in Antarctica based on surface measurements"

C. D. Groot Zwaaftink, A. Cagnati, A. Crepaz, C. Fierz, M. Lehning, G. Macelloni, and M. Valt

We thank Elizabeth Morris for her constructive review and pertinent comments that greatly help improve our manuscript.

General comments

This paper describes modifications made to the SNOWPACK model to enable it to be used in on the Antarctic Plateau where wind-transport modifies the timing and characteristics of new snow inputs to the snow pack. The revised model has been tested using data from Dome C. The modifications are interesting and potentially useful. In summary the authors propose: (1) a time delay between precipitation and input of snow to the snow pack depending on wind speed. (2) a new snow density at the upper boundary of the snow pack depending on wind speed (3) a revised expression for wind-compaction in the upper 7 cm of the snow pack (4) a revised expression for the temperature dependence of snow viscosity. With these changes the model produces good simulations of surface temperature and of temperature at 10 cm depth over a 6-week period. The simulated surface height over 3 years is somewhat under-estimated, although the changes in height follow the changes measured at a nearby stake farm reasonably well. Simulations of snow stratigraphy produce depth hoar layers, as would be expected, although such layers were not actually recorded in the observed stratigraphy.

Authors: Depth hoar was observed in snow profiles, we will show this by adding further observed profiles to Fig. 7.

The authors show clearly that it is not a good idea to use a constant density of 83 kg m^{-3} for the new snow input at the upper boundary over a 3-year period. This is hardly surprising, since periods of calm when snow is added to the pack at this density are limited. Previous studies have used new snow densities of 300-400 kg m-3. The authors really need to show that equation (1) produces better simulations than an optimised constant density – which will probably lie in the 300-400 kg m-3 range. The paper would be much improved if the effect of each modification to the basic "Alpine" SNOWPACK model was demonstrated in turn, and quantified using an expression for goodness-of-fit of the simulated to the observed data. In particular, it seems important to separate modifications (1) and (2) – is there really evidence that the time delay is important? Or is the correct choice of input density the critical factor in improving simulations? How important is the new expression for viscosity compared to the other changes?

Authors: We have done several simulations to distinguish the effects of each modification. A goodness-of-fit for each version is not that simple because the observed snow profiles present a rather coarse resolution regarding density and cannot easily be compared to the modelled snow cover. Quantitative results could be obtained from surface temperature, albedo and 10 cm snow temperature comparisons. Temperatures at 10 cm and at the surface are slightly better represented in case the solid deposits are added to the snow cover in events, as will be shown in our revised manuscript.

Structure It would help the reader to follow the argument if observations made by previous workers appeared either in the introduction or as part of a discussion section and observations made at Dome C were described all together in a "field observations" section. Descriptions of processes crop up in different sections and would be better gathered together in one place – the Introduction is an obvious choice. The main difficulty in following the paper arises because the authors spend a lot of time discussing the surface-board measurements, which, in the end, are not used in the modelling. Clearly the measurements were made in the hope that they would show the input to the snow surface. Did they? If they are not useful to drive the model, did they at least contribute to deriving equation (1)? The reader needs to be guided through a structured exposition that explains why each bit of data is introduced.

Authors: The surface boards were one element in the derivation of equation 1. We now already refer to these in combination with equation 1. We give so much attention to these as they show the need for and the difficulty of such measurements. By describing the current measurement setup and discussing the results we provide a foundation for possible follow-up studies by us or others.

Nomenclature The paper would greatly benefit from a revision in which key terms (accumulation, deposition, precipitation etc) were defined and used precisely and consistently. For example, in the Abstract the authors refer to "precipitation measured 1 m above the surface" whereas later (section 4.1) they state " After subtracting the amount of hoar deposition from the observed daily total deposition on a table 1 m above the surface one obtains the estimated precipitation". In other words, precipitation is derived from measurements of snow depth on the table. Another example comes later in the Abstract where the authors write "We then used the snow cover model SNOWPACK to simulate the snow cover evolution for different deposition parameterisations. The main adaptation of the model described here is a new event-driven accumulation scheme." The reader will suppose that "deposition" and "accumulation" are different – but it is not clear how. An extra difficulty arises in that "deposition" is commonly used to mean the opposite of "sublimation" whereas in this paper the term seems to be reserved for advection of ice particles to the upper surface of the snow pack.

Authors: We now use 'solid deposits' to refer to the total deposition on the table 1 m above the surface, be it snowfall/precipitation, diamond dust or hoar.

Specific Comments

Title I think it would be helpful to re-think the title of the paper: (1) I do not really think that the authors are introducing a new paradigm or that the model necessarily applies to the whole of Antarctica. (2) The phrase "event-driven" is defined to have a specific meaning in the text, but for the reader who just reads the title the meaning is not at all clear and could refer for example to precipitation events.

Authors: The model does not necessarily apply to the whole of Antarctica, we will change the title accordingly. However, we do not know of any other snow cover model that adds snow to the snow cover in events and therefore used the wording 'new paradigm'. 'Event-driven deposition' may indeed not be that clear on itself, but we think that it is appropriate nonetheless. Further explanation would result in a too long title.

p.3577 l.1 If the conditions are found across the whole Plateau they are "extreme" rather than "extraordinary"

Authors: We change this to extreme.

l.4 If the paragraph is about the whole Plateau area then you need to say "for example . . ... at Dome C "

Authors: We added the location of the measurements.

p.3578 l.25 why "snow grains" here? Do you mean snow flakes?

Authors: We use snow grains according to WMO terminology, as the paper we refer to does.

p.3582 l.7 what is meant by "if necessary " here?

Authors: When there was too little snow for a density measurement and an estimate was needed. We changed the text to make this clear.

p.3583 1.11 how about using the term stratigraphic profile to make clear what is meant?

Authors: This is not necessary as this is described in line 13 and defined in the international snow classification.

l.23 snow structure is not the same as metamorphism

Authors: We now mention both snow microstructure and metamorphism.

l.27 Antarctic Plateau environment

Authors: We added 'Plateau'.

p.3584 l.6 Why are the observations not shown?

Authors: Because it is not the focus of this study. Also note that SNOWPACK does not distinguish between different types of new snow except for the effect of wind on dendricity and sphericity.

l.10 dune formation does not necessarily mean snow is immobile

Authors: You are right, this is not necessarily so. The paper we refer to however describes the dunes as of high density and mechanically stable. Thus we think that these are events during which snow can be immobilized.

l.20 Presumably the "original mechanism" is a reference to the original version of SNOWPACK, but this makes no sense to the reader at this stage

Authors: 'Original' was used to underline the novelty of this mechanism. To avoid confusion we omit it.

p.3585 l.5 How is this calculated?

Authors: Using a logarithmic wind profile assuming z_0=1 mm.

l.7 which daily average?

Authors: We added "over 4 m/s"

l.9 What is the justification for using a 100 hour moving average?

Authors: The observers on site estimated relevant erosion and deposition events to last about this length.

l.10 "lies in the range 4-7 m s-1 " implies 7 m s-1 is the upper bound. Do you mean "exceeds 4-7 m s-1"?

Authors: We changed this to 'exceeds 4 m s^{-1}'. (7 m s^{-1} is the upper bound of our observations)

p.3586 Equation (1).The variable Uevent should be made dimensionless. log (x) has a singularity at x=0 so the lower limit on Uevent needs to be discussed.

Authors: We changed equation (1) to: $\rho = 361 \cdot \log\left(\frac{U_{event}}{U_0}\right) + 250$. The lower limit of U_{event} is 4 m s^{-1} and log(0) therefore does not occur.

p.3588 l.9 Is "toughness" the right word here? Equation (4): need to show how f(T) relates to strain rate (as you do with equation (2)) .

Authors: Toughness is the mechanical property measured by Schweizer et al. (2004). We omitted to show the relation to strain rate to avoid going into too much detail here. However, we will add a one in the final version.

p.3589 l.21 Strainrate at 10m can be estimated if you know the mean annual accumulation and the density

Authors: You are right and we will consider adding it to the text.

p.3591 l.17 This is rather confusing. Why not say that the effects of wind transport are larger at the surface than at 1 m height?

Authors: We changed this to: This is mainly due to the larger effect of drifting and blowing snow at the surface.

l.25 Large spatial heterogeneity in what?

Authors: Of deposition.

p.3592 l.5 Need to stae error isn 13.7 kg m-2 This is significant since densit is only estimated from snow crystal form. Note that you have not explained how Frezzoti et al determined the mean annual accumulation or what the variability might be. This whole section needs clarification – and it is worth considering why it is included.

Authors: The mean density used here is based on measured values only. We will make this clear.
In line 1 on page 3581 we stated that the mean annual accumulation was inferred from stake measurements between 1996 (and not 1969 as stated erroneously in a table of Frezzotti et al., 2005) and 1999. We now added the standard deviation (mean annual accumulation is 39 ± 14 kg m^{-2} a^{-1}) and revised this section.

p.3593 l.25 Last three sentences are misplaced here

Authors: We will consider either moving these sentences to the conclusions or rewording them appropriately.

p.3954 l.12 Would it not be better to compare equations than to compare your simulated results with filed results from areas with different climatological condition?

Authors: We don't think so as the locations the measurements are taken from show some similarity to Dome C. However, we will make a note on this in the revised version. On the other hand, comparing our results to equations that are not based on

measurements taken at Dome C may not lead to much more convincing conclusions either.

p.3595 l.8 Need to be careful here. The surface temperature is determined via the longwave energy, and if this also plays a major part in the energy budget the predicted surface temperature may not be independent of the measured value.

Authors: Note that measured outgoing longwave radiation is not used as an input for SNOWPACK simulations. We will carefully revise this sentence to avoid confusion.

Figures A location map of stations referred to in the text would be useful.

Authors: All locations are shown in Figure 2.

Generally the figures could do with better titles with more explanation of what is shown.

Authors: We will change the titles where appropriate.

Contours and prevailing wind would be useful in Fig 2

Authors: Contours are not useful as the area around Dome C is almost flat; we will mention the prevailing wind direction.

Why not use conventional symbols in Fig. 7?

Authors: We will use conventional symbols for field data and colours for modelled results. Note that the model cannot reproduce the full variety of shapes as found in the ICSSG.

Technical corrections:

Authors: Thank you for the suggested revisions to the English. We changed all sentences accordingly or revised complete sections. We only keep "zastrugi" instead of "sastrugi" to be consistent with the ICSSG.

References

ICSSG:
Fierz, C., Armstrong, R. L., Durand, Y., Etchevers, P., Greene, E., McClung, D. M., Nishimura, K., Satyawali, P. K., and Sokratov, S. A.: The International Classification for Seasonal Snow on the Ground, IHP-VII Technical Documents in Hydrology N° 83, IACS Contribution N° 1, UNESCO-IHP, Paris, viii+80 pp., 2009

Frezzotti, M., Pourchet, M., Flora, O., Gandolfi, S., Gay, M., Urbini, S., Vincent, C., Becagli, S., Gragnani, R., Proposito, M., Severi, M., Traversi, R., Udisti, R., and Fily, M.: Spatial and temporal variability of snow accumulation in East Antarctica from traverse data, Journal of Glaciology, 51, 113-124, 10.3189/172756505781829502, 2005.