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

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We thank Kouichi Nishimura for his thorough review. His valuable suggestions and comments will help us improve our manuscript. We also note that he addresses some key questions that cannot be answered in this study yet. Indeed, we hope this paper will stimulate further improvements both in field measurements and simulation work in the near future.

General comments

This manuscript describes the SNOWPACK application on the Antarctic snow based on the observations over three years at Dome C. I appreciate very much for the authors' efforts not only for the systematic observations at extremely severe meteorological conditions, but also to make the snow cover model SNOWPACK, that is very popular for both scientific research and operational use all over the world, applicable for the low temperature and high wind situations. Thus, I do believe this article involves worth publishing contents a lot. However, throughout the manuscript, a number of questions and comments arose as shown below. These should be satisfactorily addressed before the paper can be accepted for the publication.

It is true that the snow accumulation on the Antarctic ice sheet is strongly influenced by the wind. In general, the snow on the Antarctic ice sheet is eroded by the strong katabatic wind where terrain inclination is rather high and deposits near the coast where the wind speed is weakened. At high altitude area like Dome C, the wind seems relatively low as well. As is indicated on the title, 'event-driven deposition' is a key on this manuscript. However, as you may probably know, Fujita and Abe (2006 in GRL) have already noticed that snow deposition on the surface increases during (or just after) the blowing snow events at Dome Fuji. So this idea is not always new. Anyway, please let us know why the snow deposits after the blowing snow event.

Authors: We are fully aware that several studies have described snow deposition during blowing snow events. We neither claim nor want to give the impression that this would be new. On the contrary, in our manuscript, we refer to Birnbaum et al. (2010) who also describe this phenomenon. However, we are not aware of publications that describe snow-cover models driven by such 'events' instead of by measured or modelled precipitation. We therefore only maintain that the present paper is a new approach to snow-cover modelling.

Snow becomes immobile during blowing snow events because its properties change during the event, going to smaller and more rounded grains, leading to enhanced compaction.

Authors say that according to the observation, the change of snow height from the stakes somehow depends on the long term average of the wind speed (such figure which shows the relation between the snow height and the mean wind speed is required at least), but mechanism is not so clear. Is the Dome C situated on the deposition zone in general? One more point we should know is that where the blowing snow particle come from? Please make clarify these points first of all. Otherwise, we don't see whether the strategy introduced in this manuscript is applicable all over the Antarctica or is strongly site dependent.

Authors: We will add a figure where we show the 100h moving average of the wind speed compared to measurements of snow deposition and erosion on one of the surface boards. In our approach we assume that erosion and deposition occurs locally. We will deepen the discussion on the applicability of our model in the final version.

Secondly, I do have impressions some of the coefficients in the SNOWPACK introduced to adjust the Antarctic conditions were determined more or less arbitrary, and am a bit anxious whether they are quantitatively correct enough.

Authors: Please see our answers to your specific comments below.

Specific comments are listed below.
Page 3584, line 11: "The snow becomes immobile during or after the blowing snow events." Needless to say, the strong wind gives the effect on the snow cover; it easily blows away the newly deposited snow. As described above, please explain the mechanism which makes the snow deposit here.

Authors: We assume that the final immobilization is mainly caused by the compaction of the snow during wind 'events', as described in lines 24 (p 3584) – line 6 (p3585).

Page 3584, line 14: "The amount of precipitation can be retrieved from the measurements taken on the table at 1m above the surface" The drifting snow flux will be less at the position of table than the surface, but wind is stronger. So, the snow on the table will be easily blown away and it obviously does not correspond to the "precipitation".

Authors: We are aware of the influence of the wind and discuss this in the manuscript. This measurement method was chosen because we wanted to measure precipitation, not the deposition on the snow surface. Therefore we performed the measurements 1 m above the surface on a table that had borders 5 cm in height on three sides to reduce blowing snow effects. This will be added to the description of the tables.

Page 3584, line 16: "strongly wind influenced stratigraphy" may happen. Is it actually observed at Dome C and confirmed? It is not clearly shown in Figure 7.

Authors: It has been observed by e.g. Palais et al. 1982, as referred to in line 4 (p3584). It has also been observed in our snow profiles. Unfortunately, there was no wind crust in the particular profile shown in Fig. 7; it will be more apparent by including several observed profiles for comparison.

Page 3585, line 8: Do you think this strategy, including U_{event}, is applicable to other sites in Antarctica or is strongly case dependent?

Authors: We think that the strategy of adding the snow to the snow cover during drifting snow events or storms rather than during calm precipitation is applicable to other Antarctic sites with low accumulation. The lower limit of U_{event} at 4 m s^{-1} may be site dependent. We will make this clear in the final version.

Page 3586, line 3 to 4: Please show us briefly how the vapour transport was evaluated.

Authors: We calculated the latent heat flux at the snow surface and the vapour flux within the snow cover with SNOWPACK. These fluxes, however, are very small due to low air and snow temperatures; maximum values of the surface mass flux are in the range of 10^{-4} kg m^{-2} s^{-1} and hardly occur. We mixed the description of these two processes in the current manuscript and will make clear what we mean in the revised manuscript.

Page 3586, line 18 to 19: In this manuscript, not only the new surface snow density but also densification process is adjusted for the polar snow in 3.2 and 3.3. If the latter is determined rigorously without arbitrary parameters, the former procedure may be fine. However, it is not always the case here. Authors say that there are no data available to test these model implementations in page 3587.

Authors: We fully agree that there is still room for improvement provided additional new data is available. Nevertheless, these equations are not completely arbitrary. First of all, qualitative observations of deposition during and after drifting snow events confirm the high density caused probably by the closer packing of fine sized crystals. There are however, as far as we know, no measurements of the surface snow density during these events that could help us better establish such an equation. The parameterization is such that a) we cover the range of density observed at Dome C and elsewhere on the East-Antarctic Plateau; b) the mean density of deposited snow in simulations over multiple years correspond to the mean density observed in the top 20 to 30 cm of snow profiles. Requisite (a) could not have been reached with the assumption of a fixed density as done in earlier studies. Requisite (b) can be even better achieved using the mechanism proposed in 3.2. This will become apparent in the final version where we will present results for different combinations of surface compaction. Regarding 3.3, however, runs with the former temperature dependence for settlement (see Eq. 5) showed that this part hardly affects surface compaction but strongly influences the settlement of the underlying old snow cover. Surface compaction and settlement of lower lying snow can thus be dealt with separately. We therefore suggest that our approach presents useful parameterizations to simulate the snow cover under these special conditions.

Page 3586, line 19 to 23: Dendricity and sphericity were set according the visual observations of deposited snow? If the blowing snow particles reach here after the long travel, it is reasonable to assume as rounded ones, but, as is also indicated in this manuscript, needle type precipitations are occasionally observed in the Antarctica. I am not certain it can be expressed properly with these two parameters.

Authors: It is indeed difficult to set dendricity and sphericity from visual observation. However, as we argue that precipitation is not added to the snow cover until it has been transported by the wind over longer periods, the 'new snow' in event-driven simulations of SNOWPACK thus mainly consists of fine, rounded blowing snow particles.

Page 3587: Density of new snow deposited on the surface can be higher at windy conditions. However, how the strong wind affects on the old "deposited snow" and makes the density higher? Physical explanations are needed.

Authors: We argue that it is a continuation of the process of immobilization, that is, if the wind is strong enough, surface snow can still be moved locally but is further compacted at the same time.

Page 3587, line 12: What is the instantaneous wind speed? I suppose the wind speed used in this article is hourly average only.

Authors: You are right. We will adapt the text accordingly.

Page 3587, line 15-18: How do you determine "n" and derive equation (3) without data? Are these determined arbitrary? I wonder "SfcDens" shown later was the calculated with taking into account both "snow compaction by wind" in 3.2 and "snow settlement" in 3.3. Please compare the contribution of each process and show which process gives larger effect.

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Page 3587, line 22: "current knowledge" - Please explain specifically and cite references if available.

Authors: 'Current knowledge' refers to studies that have shown that the mass flux of drifting snow can be described as a cubic function of the friction velocity, for example, Nishimura and Hunt (2000). This lead us to take n=3 in Eq. 2, for example. As for Eq. 3, we want the process to stop with increasing depth. This is also the approach taken by Vionnet et al., (2012, section 3.5) and we could have used their mechanism to mimic the surface compaction of snow after immobilization. Finally, note again that the processes described in 3.2 and 3.3 can be looked at as quasi independent.

Page 3588, line 26: f-function in equation (4) is the same as the one in equation (2), although both are related to the snow compaction? More explanation is needed to avoid misleading. It is a good idea to introduce an Arrhenius relation to express the temperature dependency. However, it looks like some of the
parameters, such as ï ¸ A´c and Tref were determined arbitrary. If it is not the case,
please describe the derived procedure more in detail.

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Page 3588, line 19: How do you obtain the activation energy for Alpine Snow? In other words, please explain the calibration procedure. Then, why the activation energy for the Alpine snow becomes
larger than the value obtained by Schweizer et al. (2004)? The energy for the alpine snow can be directly applicable for the polar snow as well?

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Page 3589, line 1 to 5: In fact, equation (4) sounds to express the settlements well at low temperature. But I
am a bit anxious whether it also fits at higher temperature quantitatively, say near the melting point, where numerous measurements and discussion have been conducted so far.

Authors: The f-functions are indeed different and this will be taken care of in the final version. T_{ref} is based on Schweizer et al. and the other parameters follow from calibrations with data from our Alpine study site as explained in the text. However, it is out of the scope of this paper to describe the calibration more explicitly here. Our goal was to use one and the same function to describe the temperature dependence of snow settlement. We don't think that there are physical reasons for very cold polar snow to behave differently from warmer Alpine snow with this respect. In fact, Eq. 4 works very well with Alpine snow too, as far as we can tell from our operational use of SNOWPACK and further checks. Therefore the parameters proposed represent the best fit to accommodate the whole temperature range.

Page 3589, line 19: The new formula dropped the age term and changed the coefficient values from the equation (6). Does it fit better with the measurements? I wonder if you can show a figure as well.

Authors: We compared both parameterizations to measurements of the albedo at Dome C during summer around midday. We summarize this comparison by means of the mean absolute error (MAE) and the modified coefficient of efficiency (E, following

Legates and McCabe, 1999). For an event-driven simulation including surface compaction and the Antarctic albedo parameterization: MAE=0.02, E=-0.51. For a simulation with the standard albedo parameterization used with SNOWPACK but otherwise the same setup: MAE=0.04 and E =-1.86. Both measures thus indicate that the Antarctic albedo parameterization fits better with measurements. In our revised manuscript we will not go into detail on the performance of the standard parameterization, but we will further discuss the performance of the Antarctic version.

Page 3591, line 1: Deposition of diamond dust is negligible here?

Authors: No, diamond dust is included in both the precipitation observations and the NWP precipitation.

Page 3591, line 4: As is mentioned above, the accumulated snow on the table never corresponds to the precipitation there. Thus, comparison with the value by NWP has no meaning. Anyway, just one comparison over the long period of 9 months is not enough. More detailed analysis, for the duration of short period or, at least, every blowing snow event is necessary. Then, if both agreed well, physical explanations need to given. Suppose the snow on the table explains the precipitation amount here, why does this amount is given as an input of SNOWPACK simulation? The deposition here is regulated with the event, that is "blowing snow", and is not the precipitation. "Where does the blowing snow particle comes from?", which is a key issue. If the precipitation just around Dome C is blowing, the story can be fine. However, if the blowing snow particles arrive here after long distance trip, it does not make sense at all. Please make clear authors' point of view.

Authors: We expect that our measurements at 1 m are an underestimation of the actual precipitation at this location. However, we need to compare this to NWP and surface measurements to confirm this and to estimate how large the error is since it is an important input to our model simulations (see lines 18-22, same page).

This paper discusses the difference between the precipitation and actual deposition on the surface. The difference is due to the wind, acting on the surface snow. This phenomenon is neither represented by the NWP nor by precipitation measurements, as we argue by a comparison to surface boards. We suggest that precipitation is causing the long term accumulation and wind is producing short time local deposition and erosion. To know if we add the correct amount of snow to our model in the total simulation we compare precipitation on long time scales to mean accumulation and estimates of the NWP. We will improve sections 4.1 and 4.2 to clearly distinguish short and long time scale processes.

Page 3591, line 22: One is untouched during the observation, while snow was cleared every day for the other. I wonder if snow surface level of two boards were the same? If the either is higher, the snow on the board will be easily eroded.

Authors: The boards were at the same level, 50 cm apart, and positioned on a line perpendicular to the prevailing wind direction. As we can see from Fig. 4, snow accumulation on the boards was at most a few centimetres per day, except for a one time increase of 12 cm. Each snow height is the average of 4 to 5 measurements over one board. Despite the small scale heterogeneity, we think that none of the boards did notably influence erosion and deposition on the other.

Page 3591, line 25: I agree that the special heterogeneity needs to be taken into account. This is the reason why lots of stakes are set to evaluate the budget in the study area. However, if you stand on the position that the fluctuation is not negligible, all discussions based on the measurements on only one board and snow surface become questionable.

Authors: The two boards are mainly used as indicators of what happened on the surface, that is, showing when 'events' may have occurred. In that sense we can use them for comparisons.

Page 3592, line 22: Again I do not know the reason why there is a good correspondence, since the snow deposit on the table does not express the precipitation. Further how do you remove the hoar from the sample; manually with the eye? No diamond dust exists here?

Authors: Diamond dust does exist and is included in these amounts as it is also included in NWP prediction amounts. The type of solid deposit was determined at the time of observation. We expect underestimation of precipitation during windy conditions, the edges on the table will give some protection then, but not always enough. On calm days however, our measurement method should work fine.

Page 3594, line 4 to 19: Since the discussion in 4.2 involves number of unreliable assumptions, I am not certain whether you can refer into the snow settlement issue as well.
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Page 3594, line 24: During this period the wind looks low and I can expect the effect of snow drifting is small. Thus, probably it is the best opportunity to confirm the new snow settlement process introduced at 3.3 without any disturbance.

Authors: We refer to our answers above where we state that the compaction of surface snow (section 3.2) and the settlement at greater depth (section 3.3) can be looked at independently, which allows us to do such a comparison. Regarding the relatively calm, short period used for the analysis of snow temperatures, the latter allow for an indirect check of our settlement routine. However, more reliable data would be required to fully confirm the appropriateness of our approach.

Figure 5: Run for "event" is hard to recognize.

Authors: We will change the colour and/or line in a revised manuscript

Figure 7: Authors say snow pit observations were conducted several times. Perhaps it is helpful to show other results and compare with the simulated one in addition to Figure 7.

Authors: We will add one or two additional observed profiles in Fig. 7. We will not, however, do this for different times as these profiles are not detailed enough.

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