The Cryosphere Discuss., 6, C1988–C1992, 2012
www.the-cryosphere-discuss.net/6/C1988/2012/

The Cryosphere Discussions

Interactive comment on "Event-driven deposition: a new paradigm for snow-cover modelling in Antarctica based on surface measurements" *by* C. D. Groot Zwaaftink et al.

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Received and published: 28 October 2012

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 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. 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.

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. 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". 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. Page 3585, line 8: Do you think this strategy, including Uevent, is applicable to other sites in Antarctica or is strongly case dependent? Page 3586, line 3 to 4: Please show us briefly how the vapor transport was evaluated. 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. 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. 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. Page 3587, line 12: What is the instantaneous wind speed? I suppose the wind speed used in this article is hourly average only. 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. Page 3587, line 22: “current knowledge” - Please explain specifically and cite references if available. 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 ïĄćïĂăand Tref,ïĂăwere determined arbitrary. If it is not the case, please describe the derived procedure more in detail. 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? 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. 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. Page 3591, line 1: Deposition of diamond dust is negligible here? 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. 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. 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. 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? 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. Page 3592, 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. Figure 5: Run for “event” is hard to recognize. 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.

Interactive comment on The Cryosphere Discuss., 6, 3575, 2012.