

We first of all would like to thank Eric Brun and Samuel Morin for their constructive comments on our discussion paper. Below we retain their comments in italic for clarity.

*The authors address the difficult problem of surface snow compaction under windy and cold conditions. The observations collected at Dome C, routinely or within the framework of this study, are appropriate for the investigation of some of the compaction processes at work. The method which was developed for feeding SNOWPACK with snowfalls at higher densities than its basic version proves itself undoubtedly efficient for a long-term simulation of the surface snow layers density and temperature.*

*However, some aspects of this method would benefit from additional information:*

*- a comparison with a simpler method for representing snow deposition according to the observed precipitation time series and at the minimal density of equation 1 ( $250 \text{ kg/m}^3$ ) would help to assess the benefit of the wind dependency in equation 1.*

We indeed performed a simulation where snow was added at time of observation with a fixed density of  $320 \text{ kg m}^{-3}$ . The benefits of the event driven simulation compared to using a fixed density are a) a more structured snow cover, b) layer densities varying over the range of Eq. 1 from the time of ‘addition’. Moreover, using  $250 \text{ kg m}^{-3}$  as a fixed value for density, we would have had hard times reaching within reasonable time a mean density of about  $300 \text{ kg m}^{-3}$  in the top 10 cm of the snow cover. Because we thought this simulation to be of little use, we decided not to show it and concentrated on the event driven one. However, the above comment teaches us the contrary and we will reconsider including a simulation with fixed density in the final paper.

*– quantitative statistics comparing the different simulations over the complete 3y period would be very helpful as well, i.e. RMS on surface and subsurface temperature, RMSE and correlation on the albedo during the summer period, correlation and RMSE on the detrended snow height, ...*

We of course know the relevance of such quantitative comparisons. However, as described in the paper, subsurface temperature measurements were only valuable shortly after a repositioning of the sensors as the snow temperature measurements site, contrary to other observations, was affected by a local snowdrift. We will emphasize this in the revised version of our paper. Regarding the other parameters, we can most easily do it for surface temperature while missing verification data makes it trickier for the albedo. Nevertheless, we will consider including both in the final version. The irregular and at most weekly measurements of snow height definitely make them unsuitable for such an analysis. .

*- as mentioned by the authors, a better surface density is a prerequisite condition for long term simulations of the Antarctic firn, since it significantly affects the energy and mass exchanges between the surface and the atmosphere. This aspect could be illustrated in the paper by comparing in the different versions the contribution of different terms controlling the state of the snowpack, i.e. sublimation/hoar, sensible heat fluxes, radiative fluxes, heat flux from the bottom layer, ...*

This is an interesting aspect and the difference in modelled subsurface and surface temperatures due to the parameterization of new snow density and wind compaction indicate that these parameterizations are crucial for the energy and mass exchange. However, a discussion of the different terms is beyond the scope of this paper and may possibly be dealt with in a follow-up study.

*The authors refer to a previous work with the snowpack model Crocus where the representation of near-surface snow compaction was modified to account both on the wind velocity during the snow fall and on the snow drift events occurring after the snowfall, if any (Brun et al., 1997, Vionnet et al., 2012). Indeed, this former method was already an event-driven way to simulate the snow surface density over the Antarctic Plateau.*

We suspect that our description of event-driven is not completely clear yet. An essential point in our considerations is that snow has to be redistributed several times before it is added to the snow cover. Furthermore, this inclusion in the snow cover happens only during a long enough period of sustained winds (100 h), that is, an ‘event’ and the density of the added snow depends on the mean wind speed during the event (see Section 3.1). This is quite different from depositing snow at the time of snowfall and we expect an event driven simulation to also effect the metamorphism of near surface snow. Again a topic for further investigations.

*In this regard, the sentence on page 3579 spanning lines 14 and 15 “ Compaction, however, does not necessarily happen at the time of deposition but can occur days or weeks later“ is misleading since it gives the impression that Brun et al. (1997) only considered wind speed at the time of deposition. As mentioned in Brun et al. (1997) and described in details in Vionnet et al. (2012), this is not the case and the impact of wind after the time of deposition is explicitly taken into account and leads to quicker snow compaction close to the surface in the presence of wind, depending on the physical properties of snow.*

We are fully aware that the method implemented in Crocus causes a wind induced compaction of already deposited snow at times after deposition. That is what we wanted to express with this apparently confusing sentence and we will change it to: “That way, compaction due to wind may still occur days or weeks after deposition”. In fact, the mechanism described by Brun et al. (1997) and Vionnet et al. (2012) is similar to the one described in Section 3.2 of our paper.

*Compared to the method presented in the paper in discussion, the advantage of this former method was to allow the deposition of snow at the actual time of snowfall episodes, which is a necessary condition when the snowpack is simulated within a climate or a meteorological model.*

In our opinion the deposition of snow to the snow cover at the actual time of snowfall episodes is a disadvantage. Our data show that precipitation does not necessarily become a part of the snow cover during snowfall periods. SNOWPACK sums up the amount of precipitation until an ‘event’ occurs to deposit it then. Thus an event-driven SNOWPACK simulation is also compatible with climate or meteorological models.

*However, the method developed in Crocus has been evaluated in an Antarctic context only over South Pole. A comparison between both methods would be very valuable. Since SNOWPACK uses a description of snow grains very similar to Crocus, including very similar metamorphism laws, the implementation into SNOWPACK of the parameterization of compaction induced by blowing snow should be technically easy. An alternative could be to drive Crocus with the 3y forcing data sets and to compare the results with those in the paper in discussion. We are open to carry out such experiments using meteorological driving data provided by the authors if they are interested in doing so.*

We also welcome such a comparison and cooperation and think the best way to compare the methods would be to implement the Crocus-wind-compaction into SNOWPACK. However, this goes beyond the aim of the current paper.

*Finally, there are in the literature previous papers addressing the snow and meteorological conditions at Dome C or over the Plateau, as well as the performance of meteorological and climate models to represent them. Some of them are quite relevant for the paper in discussion and could be referenced, i.e: Gallée, H. and V. Gorodetskaya, 2008. Validation of a limited area model over Dome C, Antarctic Plateau, during winter, Climate Dyn.,34, 61–72. Gallée, H., G. Guyomarch and E. Brun, 2001. Impact of snow drift on the Antarctic ice sheet surface mass balance : possible sensitivity to snowsurface properties, Bound.-Layer Meteorol., 99, 1–19. Genthon, C., M.S. Town, D. Six, V. Favier, S. Argentini and A. Pellegrini, 2010. Meteorological atmospheric boundary layer measurements and ECMWF analyses during summer at Dome C, Antarctica, J. Geophys. Res., 115(D05104).*

Thank you for pointing out these papers to us; we will consider adding them to the discussion section.