

Interactive comment on “The snowdrift effect on snow deposition: insights from a comparison of a snow pit profile and meteorological observations” by M. Ding et al.

Anonymous Referee #4

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I understand it is quite important to reveal the snow deposition and the erosion processes in the Antarctica, since it gives the crucial effects on the ice core dating. I cannot agree with the authors more. However, unfortunately, this short manuscript (report) seems generally immature at this stage, and reorganization and improvements throughout the manuscript are needed before the publication. Although the chapter 3 is named ‘Discussion’, most of the contents are nothing but general information which we can find in the text book and the review of the previous studies, and it is far from satisfactory. Throughout the manuscript, more deep, careful and precise analysis and discussions are essential. First of all, throughout the paper more careful proof is necessary. Lots of careless typo and mistakes are found; such as ‘sonw’ on line 26, page

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1416, ‘the the’ on line 10, page 1417, and line 13 ‘because but not only drifting and blowing snow events’ is perhaps ‘because not only drifting but blowing snow events’. ‘SPWD’ seems the surface slope along the prevailing wind direction and ‘CHINARE’ that is the Chinese National Antarctic Research Expedition should be introduced on line 8, and line 16, page 1417 respectively. It is also strongly recommended that all units are described according to the MKS standard instead of ‘cm’ and ‘g’. Further, numerous questions arose are listed below.

Line 12 to 22, page 1417: Were these two cores obtained in the neighborhood? And, in conclusion, although nothing is described here, were there any specific differences of SPWD between two sites?

Line 25, page 1417: No blowing snow effects on the recording of the ultrasonic sounder were recognized during the observation? As you see, in the heavy snow storm, the particle concentration near the surface, say saltation layer, is quite high and it may affect the recording, I am afraid. I also would like to know whether the snow depth recorded every one hour is just an instant value or the average of several data (period)?

Line 3 to 6, page 1419: This paragraph should be put on ‘Results’ chapter, not ‘Location and method’.

Line 18, page 1419: ‘40.80’ must be ‘-40.80’. I am also curious that it makes sense to show values to two decimal places in spite that the accuracies of the measurements are lower.

Line 7 to 8, page 1420: Where does 0.91 come from in Fig.4?

Line 20, page 1420: How did you estimate the sublimation amount quantitatively in this study? Please let me know the procedures and the obtained results. Nothing could be found in this manuscript.

Line 26, page 1420 to line 10, page 1421: This paragraph is just a review of the previous work and is not suitable at the discussion part. It should be put on the introduction

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part, if it is really needed.

Line 20, page 1421: How do you know as 'not for precipitation'? I see the accumulation in summer is small in actual, but how could you get rid of the contribution of the precipitation from your data?

Fig. 6: Perhaps this figure is a key in this manuscript. However, it is hard to recognize what is happening. Although we see the continuous accumulation and the deposition happens repeatedly, it is difficult to imagine whether it is really due to the process of the blowing snow. In addition to Fig. 6, more detailed data, perhaps day by day for the specific period, with the wind speed should be introduced at least.

Line 1 to 2, page 1422: How do you know from Fig. 5 that the surface snow was accumulated in Oct. 2007?

Line 2 to 11, page 1422: All the processes described here are hard to recognize in Fig. 6. Prepare the figures which show how the snow height changed during each period with the wind speed data. Then please step forward to more careful and detailed analysis. Snow height variations of several cm are found during whole observation periods in Fig.6. These also can be explained with the accumulation and the following erosion caused by the blowing snow?

Line 14, page 1422: Why are you able to say that 'This assumption in the SNOWPACK model' is consistent, although the snow was eroded in this study on the contrary?

Line 15 to 16, page 1422: Durations in which you could not measure the wind speeds need to be shown in Fig. 6. In Fig. 6, we cannot think of the extreme environment.

Line 16, page 1422 to line 2, page 1423: As is noted here, the fact that the wind gives the substantial effect on the accumulation and the deposition of snow is quite familiar. No new findings are recognized. What would the authors like to say in particular here?

Line 10 to 13, page 1423: Why do the data at top 50 cm agree fairly well, whereas the lower part differs largely? I am afraid it cannot be explained simply by the snow drift

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effects.

Line 21 to 25, page 1423: This conclusion is quite general and all the issues have been well known. I do have an impression that nothing is new.

Line 6, page 1424: '27.8 cm', this specific value appeared for the first time in this manuscript. How did you deduce this, perhaps in Fig. 7?

Fig. 7: It looks there are continuous two trends at higher than -40C. Do you have any idea what do they mean?

Line 20, page 1424: '9cm' is an accumulation rate per year?

Interactive comment on The Cryosphere Discuss., 7, 1415, 2013.