“The snowdrift effect on snow deposition: insights from a comparison of a snow pit profile and meteorological observations”

General comments

In the manuscript entitled “The snowdrift effect on snow deposition: insights from a comparison of a snow pit profile and meteorological observations”, Ding and others present the findings of their AWS observations and snow chemistry analysis based on pit work. The authors compared the snow surface height record and the chemistry layers for a ~95 cm deep snow pit. By assuming that oxygen isotope ratio should have temperature dependence at the timing of snow deposition, the authors claim that during the three years, there are nearly one year dating error if we count the oxygen isotope fluctuations at their "Eagle" site , a site ~500 km away from Dome A in distance and ~1200 m lower in elevation in East Antarctica. The author claim that this counting error at Eagle could be a risk for ice core research at Dome A.

Although some of the data are of interest, their appeal to the readership of this journal The Cryosphere is too limited to warrant publication. In addition, the combination of the surface snow height data and AWS data has very large room to be developed. My major concern is that the major claim of this paper is that they found their d18O dating have one year error in the three years deposition. This claim is too limited, still weak to my view and rather subjective. Also, considering the logical flow, descriptions about their motivation, citations, considered physical processes, and impact of this work, there are many items to be improved, corrected or reconsidered.

For future improvement of this subject, the authors could consider items listed below.

#1, Title. Snow drift process was not really studied, at least, not directly in this paper. The authors just found that fluctuation of the d18O did not necessarily represent the seasonal variations. The authors guessed that the this situation was caused by the snowdrift.

#2, Abstract. What is new in this paper is in lines 10-12. As a peer reviewed paper such as TC, finding and claim is too limited and still weak to my view.

#3, In both title and abstract, readers understand that this is paper about Antarctic ice sheet only by the last word of the abstract. “Antarctica” needs to appear either in the title or early timing of the abstract.

#4, Introduction L15-18. “Ice cores, loess, and stalagmites are the most important proxies”. This statement is very subjective. How about ocean sediments, corals or tree rings? Loess, and stalagmites provide useful information of land environment only but they do not provide information of ocean, atmospheric components or air temperature. Ice core is the best proxy for all ocean, atmospheric components, air temperature and land environment.

#5, P. 1416, L. 18-24. Choice of citation is biased. Ren et al. (2008) is not representative paper of the NEEM. Also, a term GNIP is not found in Ren et al. (2008). Please explain. Petit et al. (1999) would be one of representative papers of the Vostok ice core results, rather than Ekayakin et al. (2010). Ekayakin et al. (2010) paper is just for hydrology of the Lake Vostok. I wonder why Tibetan ice core is especially cited in this paper. It seems out of focus.

#6, P.1416, L. 25-26. Ice core include information from ocean, atmosphere and land environment. In terms of volume, water is, precipitation. But air, aerosols and dusts have various origin including land.

#7, P.1417, L. 2. I wonder why very recent specific paper (Groot Zwaaftink et al., 2013) is cited here just to explain general knowledge of snow deposition. More general authority literature should be given.

#8, P.1417, L. 6-14. Please provide more structured review of the snow drift effect in terms of Antarctic plateau sites. Just listing various locations and various conditions will not help readers' understanding. For example, I think that recent papers by Lenaerts and van den Broeke in JGR and J. Glaciol. will help.

“SPWD” suddenly appeared here without explanation.

#9, P.1417, L. 15-27. It is surprising that within the Chinese Dome A science community, two groups shared a ~109-m-long shallow firn core, giving completely duplicated data sets, different loggings and different dating. The authors must make efforts seriously to solve this situation of duplicated efforts. To my view, the cause of the dating discrepancy seems simple, just lack of communications and misjudgment of the identity of volcanic eruptions due to insufficient constructive examinations/discussions. But it seems to me that a further problem is that the authors attributed the reason of the discrepancy to disturbance of the proxy by post-depositional processes. It seems to me that both logical flow and opinions for the causal chain have a serious problem. The authors must just re-examine volcanic tie points of the ~109-m-long shallow firn core to the other firn cores in Antarctica, rather than giving reasons to snow post-depositional process.

#10, When the authors introduced a problem of dating for the ~109-m-long shallow firn core at Dome A, they say that they study Eagle site, ~500 km away from the dome. Such a slope site far away from dome cannot be used to assess depositional environment of Dome summit.

#11, P.1419, L.10. The Eagle site is on the slope of Antarctica, away from dome by ~500 km.

#12, P.1419, L.11. When DEM is used from some database, the identity of the DEM must be cited.

#13, P.1419, L.14. When wind speed is cited, please inform the sensor height together.

#14, P.1419, L.16. The authors claim that katabatic wind has almost no influence. But to my view, prevailing wind shown in Fig. 4 (right) is really katabatic wind along the elevation coutour. Driving force of this prevailing wind must be gravity.

#15, P.1420, L.2 and after. Please clarify components of the post-depositional process.

Densification is OK. But what firnification means? How about metamorphism related to movement of vapor based on vertical gradient of temperature? This should have very big impact on water isotope redistribution. To my knowledge, annual layering of the water isotope near the surface of the ice sheet is strongly modified by this. In the discussion of the present paper, this view is missing. What is the ice stretching effect? Please explain to readers. Is it necessary for the top 1-m firn discussed in this paper?

#16, P.1420, L.10. Please clarify meaning of the transmittance. It is vague.

#17, P.1420, L.2 and after. Finally, readers will not understand if sublimation is significantly important or not. So many different papers are cited without good structure or flow. Did Qin et al. (2001) really “proved” that sublimation is unimportant? Continuous evaporation and condensation occur within firn near the surface of Antarctica due to diurnal cycles of temperature and seasonal cycles. I believe that it cannot be included within the snow drift process.

#18, P.1421, L.18-23. Did you consider seasonal snow density changes during the investigation period? In summer, sublimation (evaporation) can dominate at the surface but destinations of the vapor would be within firn due to temperature gradient, causing densification of firn.

#19, Section 3.4. in general. Readers would like to see how each synoptic event such as blocking or cyclonic activities transport moisture to the site. The authors have sufficient data to analyze correlations among wind direction, speed and temporal precipitation. This kind of analysis would really help readers' better understanding about phenomena discussed in this paper. A point of this paper should be to demonstrate how accumulation and ablation occurred depending on weather conditions. The authors could show which weather events brought/took which signals from the snow deposition. This view point is missing. If the authors know very detailed snow surface height by the ultrasonic sounder, they should be able to predict chemistry strata signals in snow layers. But I do not find such analysis in this paper. The authors just compared the tentative dating and claimed date difference. They just attributed that the difference were caused by snow drift. It seems to me that the author never demonstrated any direct evidence for the effects of the snow drift.

#20, Figure 7. It is very hard to see because of the very tiny letters.

#21, First paragraph in conclusions. At the moment, I am far from convinced. I just feel that some tricky demonstration of numbers were done to readers.

#22, Second paragraph in conclusions. It is well known fact that snow in inland plateau is deposited by limited numbers of synoptic stormy events. The authors failed to cite papers below.

Reijmer, C., and Van den Broeke, M. R.: Temporal and spatial variability of the surface mass balance in Dronning Maud Land, Antarctica, as derived from automatic weather stations, J. Glaciol., 49, 512-520, 10.3189/172756503781830494, 2003.

Fujita, K., and Abe, O.: Stable isotopes in daily precipitation at Dome Fuji, east Antarctica, Geophys. Res. Lett., 33, 10.1029/2006GL026936, 2006.

Kameda, T., Motoyama, H., Fujita, S., and Takahashi, S.: Temporal and spatial variability of surface mass balance at Dome Fuji, east Antarctica, by the stake method from 1995 to 2006, J. Glaciol., 54, 107-116, 2008.

Hirasawa, N., Nakamura, H., and Yamanouchi, T.: Abrupt changes in meteorological conditions observed at an inland Antarctic station in association with wintertime blocking, Geophys. Res. Lett., 27, 1911–1914, 10.1029/1999GL011039, 2000.

#23, Last paragraph in conclusions. The Eagle station environment is so different from Dome A summit. It is in terms of slope, prevailing wind direction and strength, distance (~500 km), elevation different by about 1200 m. It seems impossible to me that the authors claim that environment at Dome A is very similar to that of the Eagle. If it is really so, please demonstrate AWS data, ultrasonic sounder data and chemistry strata data directly from Dome A. Discussions in the authors' TCD paper can apply only for this site. Conditions at Dome A must be discussed directly from Dome A data.