Anonymous Referee #1

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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.

Thanks the anonymous reviewer's comments on the draft and the papers referring the similar topic. Some of the conclusion we claimed in the text might be a little overemphasize/not suitable such as the inference from Eagle to Dome A, cause the distance and the altimeter from one to another. Thus we decide to illustrate our opinion more cautious, by modified some part of the draft (for example, in the conclusion section). Furthermore, we added more description on our dating by chemicals and isotopes and improved the discussion.

We think, although this paper is just a case study in the inland Antarctica, the phenomenon we want to demonstrated to the scientist on ice core studies is often ignored or contemptuous in the ice core dating. As for Dome A ice core dating by Jiang et al. [2012] and Li et al. [2012], the error should be blamed into dating method they used, but why they (at least one of them) misjudged the event by the volcanic stratigraphic markers? You can see from the figure below that the chemical signals are in a perfect consistence. I believe that the abnormal accumulation or missing snow layers might mislead their judgment.

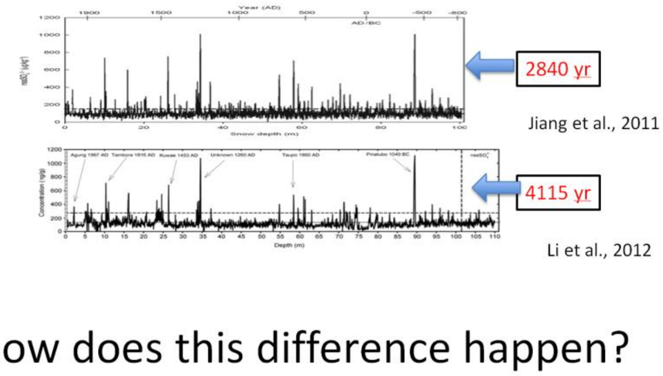

Figure S1.

Anyway, this kind of mistake happened few in the past, and the data we have can only give a case study. We have improved the paper for better, especially the specific list you give. Please find it in the following part.

Specific comments

#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 this situation was caused by the snowdrift.

A: The snowdrift process itself (the physical principle etc.) does not be discussed in the paper, but we focused on the effect it caused on the snow deposition. To improve the discussion, we added some figures and analysis in the draft.

You can see from the accumulation figure (figure 6 in first draft = figure 4 in new draft) that the surface height changes, and the other processes are excluded in the section 3.2. Thus we deduced (not guessed) the situation was caused by 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.

A: thanks for the comment. Although the snowdrift process is often mentioned in many paper, there only few studies have focused on the in situ measurement (such as Mcmorrow 2002 Annals of Glaciology). The open access journal is attractive for its high transmissibility.

#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.

A: We changed the title into "The snowdrift effect on snow deposition: insights from a comparison of a snow pit profile and meteorological observations in east Antarctica"

#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.

A: We modified the text into “Among the proxies used for paleoclimate reconstruction, ice cores have become increasingly more attractive due to their high temporal resolution……”

#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.

A: We modified the citation: change Ren et al. into the newest paper Neem community Members 2013; replace Ekayakin et al by Petit et al. 1999, etc.

#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.

A: we agree with it, and that is why we use “primarily comes from…”

#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.

A: we have explained more information of Zwaaftink 2013 in the following paragraph and section 3.2, 4.2.

#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.

A: We have added more introduction of these references and modified the text according the paper you recommended.

“SPWD” suddenly appeared here without explanation.

A: we have modified it.

#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 reexamine 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.

A：Our motivation is not to attribute the mistake to a single reason- post depositional process, this problem should be first blame to the misjudgment of the volcanic eruptions, just as your opinion. However, we did not express it clear. Thus we modified the text.

As we have shown in the figure S1, both of the paper by Jiang et al. [2012] and Li et al. [2012] have the same sulfate record. Thus they could get the similar time nodes by these peaks. But when they calculated the accumulated history by densification model or the others, the missing layer by snowdrift when precipitation, abnormal ice stretch and the other post depositional processes might be misleading the estimation

#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.

A: We modified it.

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

A: It has been modified in section 2.

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

A: It has been modified.

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

A: It has been modified.

#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.

A：the slope of Eagle towards to northwest, and the wind direction is northeast. The katabatic wind should be vertical with elevation contour, not along the elevation contour. The meteorological condition here has been detailed discussed in Ma et al. [2010, Antarctic science].

#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?

A: the firnification only refers to the changes of physical characteristics of the snow. (I am not sure it is clear by this explanation, so a metaphor given: there are 100 balls in a box, 1. if we put another 10 balls in it, the previous 100 balls might be more tight and the density will change to high, that is densification; whether or not the new balls are putted in, some of balls might merger into one ball or a layer, that is firnification, this process might influence the density and it also might not.)

We added the process related to water isotope redistribution.

We discussed ice stretching effect because it is one kind of post depositional processes, although it only acts in deep ice layer. For reader not major on this subject, it is useful.

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

A: the absorption of radiation/light.

#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.

A: The sublimation is important, especially for surface mass balance study. But for this case study, its influence is not obvious. It also can be deduced from figure 6 [figure 4 in new draft], the record are more stable in austral summer than winter (more sublimation in summer). This kind of small change could be ignored in this study. For better understanding, we add paragraph and figure 7 [in new draft] to analyze the sublimation.

For the opinion that “the net mass loss by sublimation on the Antarctic surface is no more than 5% of the precipitation”, it is concluded by Qin et al., [2001] from many research. The newest research such as Lenaerts et al. [2013; TC] estimated that ~16% of snow could be removed by snowdrift sublimation, the net sublimation by solar radiation is much less than snowdrift sublimation. Thus the estimation in Qin et al. [2001] should be right.

#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.

A: We have added this discussion, although our measurement of snow density shows little variation.

#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.

A：We add a new paragraph to discuss to correlation among wind and precipitation/mass loss. We did not install the long wave radiation on the Eagle AWS so it's impossible to identify the daily weather conditions. And the authors are not major on the simulation with reanalysis data such as ERA, which is the other tool to identify the weather condition.

We added atmospheric general circulation over Antarctica in winter/summer for detailed analysis.

Although we have not direct evidence for the effects of the snow drift, I think it is general idea that IT IS the snow drift process affect the surface height changes, as many papers have proved. Thus we need not to have a visual illustration.

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

A：It has been modified.

#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.

A: We changed the demonstration.

#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.

A: It has been added in the discussion and conclusion.

#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.

A: This paragraph has been rewritten.