Anonymous Referee #5

General comment:

The paper "The snowdrift effect on snow deposition: insights from a comparison of a snow pit profile and meteorological observations" describes the analysis of 3 years of accumulated snow record from eco-sounding data and by chemical/isotopic dating, obtained from The "Eagle" AWS site (76° 25' S, 77° 01' E; 2852 m a.s.l.), placed ~ 500 km far from Dome A drilling core site. The comparison between the two datasets highlights a difference between the two dating results of up to 12 months. The authors suppose that the discrepancy is linked to the snowdrift processes which are not well detected and/or taken into account in snow pit/ice core dating. Finally the authors observe that the same type of error can influence the information provided by the Dome A ice core project, claiming the similarity of the two studied sites.

Even if the paper addresses a relevant scientific question about the interaction between Atmosphere and Cryosphere, the results are not sufficient to support the presented thesis, and not negligible lacks in the paper flow corrupt interpretations and conclusions . In conclusion the entire work should be heavily reviewed in both data analysis and paper structure before be reconsidered for publication on "The Cryosphere".

Thanks for your kind review on the manuscript. According to yours and the other 4 reviewers' suggestions, we have restructured the draft and modified the description and discussion of the context, as well as the figures. Please find it in the new draft and the specific answers is listed as below.

Major Comments:

Authors completely overlook the large errors present in snow accumulation dating using chemical/isotopic analysis. I wonder if authors are able to date snow accumulation at Eagle site (60 kg m^{-2} yr^{-1}, Ding et al., 2011) with an error equal or less than one year. The stake farm measurements demonstrate that accumulation hiatuses and/or erosion can occur at sites with accumulation rates below 120 kg m^{-2} yr^{-1} (Frezzotti et al., 2005, 2007; Ding et al., 2011). The differences between the cores and stakes can lead to the statistical misidentification of annual layers determined from seasonal signals at sites with SMB rates below 200 kg m^{-2} yr^{-1}, due to the inability to detect higher and lower values. With an average roughness height of 20–40 cm, achieving ±10% accuracy in the reconstruction of the SMB from single cores requires high accumulations (>700 kgm^{-2} yr^{-1}). Low accumulation sites are representative if their cumulative rates are computed over several years. Moreover seasonal cycles are difficult to observe in sites characterized by accumulation rate below 70 kgm^{-2} yr^{-1} (Frezzotti et al., 2013).

A: We have added more chemical records for dating, to make sure the results are reasonable.

The third section is not clear and it should be rewritten. Post depositional processes are not well described and a lot of references representing the state of the art are missing (e.g. Das et al. 2013; Frezzotti et al. 2013; Lenaert et al.2012a, 2012b, 2012c). The Authors should provide a quantification of sublimation effects (surface and snowdrift sublimation, e.g. Van den Broeke et al., 2004) and snowdrift transport (see Mann et al. 2000). The authors should better explain how they obtain Temperature from chemical/isotopic data (why in fig 6 there isn't a plot of temperature measured by AWS ?). All the Results should be discussed in relation to accumulation data, meteorological parameters and modeled snowfall (for example ECMWF or NCEP or other) in order to quantify the snow redistribution component.

A: We added more description in introduction and the other part. The discussion is also extended for better illustration. The figures, tables and the structure of draft are modified.

If the Authors believe that the Eagle site is similar to Dome A site in terms of accumulation condition they should demonstrate it for example giving more emphasis to and/or improve the discussion presented in Ding et al. (2011).

A: We deleted part of the text related to Dome A.

Minor Comments:

--Figure 6 is a key figure for the paper but it could be greatly improved. Why eco sounder and wind speed data are presented with different sampling? In this form it is not possible for the readers to make a correlation. It could be helpful to plot also other meteorological parameters (e.g. relative humidity, Temperature) and forecasted snowfall from model in order to help the readers to discriminate snowfall and snowdrift effects on eco sounder data.

A: Because of the extreme environment, some of the records are missing, thus we could not give the snow depth record and wind record in same sampling. We added detailed figure during specific period in the new draft, for better illustration.

--All the Acronyms should be defined the first time the authors use them.

A: The text has been checked and modified.

-- Define the longitude and latitude of each site described in the text.

A: The text has been modified.

--PAG 1420 line 17-18. Delete or rewrite "..."sublimation can still be recorded by an ultrasonic sounder (Scarchilli et al., 2008, 2010). Scarchilli et al. 2008 and 2010 do not say that. The ultrasonic sounder can only measure variation of snow surface height.

A: It has been modified.

--PAG 1421 line 2. Delete "…with a maximum value of 0.1–0.2mm d−1 around the Talos Dome (Scarchilli et al., 2008)". Scarchilli et al 2008 deal with Mid Point site (MdPt, 75° 32' S, 145° 51' E, 2510 m a.s.l.), which is a remote site on the Terra Nova Bay – Dome C route, and not Talos Dome (72° 46' S, 159° 2' E, 2384 m a.s.l.). The maximum value of calculated blowing snow sublimation for MdPt site is 0.4-0.6 mm w.e. d-1 and not 0.1-0.2 mm d-1 (see fig. 2 Scarchilli et al. 2008)

A: It has been modified.

--PAG 1423 line 10. In a logical flow Fig 7a should be defined and described before Fig. 7b .

A: It has been modified.

--PAG 1423 line 17. Is the correlation Coefficient R between temperature and $\delta 18O$ statistically significant? Otherwise the Authors cannot say that there is a linear correlation.

A: It has been modified.