Anonymous Referee #2

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General comments

This paper addresses the important question of how well an ice core from a site where wind-blown snow redistribution occurs can provide a detailed climate record. The authors use acoustic surface height measurements from an Automatic Weather station (AWS) and detailed samples from a snow pit to investigate the accuracy of ice core dating using seasonal variations in stable isotopes and/or aerosols. They argue that at a site where snow may be transported subsequent to precipitation and accumulated elsewhere, the resultant stratigraphic sequence may not be chronologically correct. Their conclusion that redistribution can lead to serious errors in core dating has important implications, but is not well supported by the way that the data are presented and interpreted in the present version of the paper. Extension of the conclusions at Eagle to Dome A does not seem warranted given the much lower wind speed at Dome A.

The available data would seem adequate for this type of investigation, which is original and significant. But the paper is not suitable for publication in its present form, and needs considerable restructuring and rewriting.

We are appreciated with the kind review of the manuscript. You indeed pointed out our motivation of the draft and the weakness we missed to illustrate. We agree to improve it for better understanding.

The Dome A we mentioned is used to give an example we might face, but not be proved that the winddrift is responsible for the wrong dating of Dome A ice core by Jiang et al. [2012] and Li et al. [2012]. Nevertheless, wind speed and precipitation are the most important natural factors influencing the snow deposition, and the precipitation is only ~9 cm snow in Dome A. Thus we propose the winddrift acts in the dating of Dome A ice core, as well as the other post depositional processes do.

Specific comments

Two separate dating scales are derived for the snow pit profile. The acoustic height measurements are used to determine when the snow samples were accumulated at the site, and the isotope and aerosol seasonality are used to estimate a different stratigraphic dating sequence. The assumptions used for the latter are not described at all, but from Figure 5 it would appear that only the summer maximum in the isotope is used. There is very little correlation between the isotopes and sea salt, and this needs to be discussed (for example, the 2002 summer is a maximum in sea salt, but the 2003 summer is at a minimum).

A: Thanks for the reminding of this part. According to the AWS record, there are only ~95 cm snow accumulated after the deployment of AWS, which means, the samples below the 95

cm depth is contaminated by the field worker (they grubbed a pit for AWS and buried it). However, we did not address it clearly in the text before and now add this in section 3.3.

If we only pick the sequence above 95 cm depth, the maximum of isotope is well correlated with sea salt (the sea salts might be complex, but its minimum fit the max of isotope). This phenomenon is common in Antarctic inland, such as south pole [e.g. Oeschger and Langway 1988].

I found it difficult to logically follow the discussion in the way that the arguments are presented; and there is material introduced that has little connection to the present discussion. For instance density data (fig 3), average monthly wind speed data (Fig 6), and identification of individual accumulation events Table 2) are relevant to the theme, but are not well integrated into the argument. The material in Table 1 and Fig 4 is less relevant.

A: The manuscript might be not well organized and we will reconstruct it for better understood together with the other reviews.

We decide to remove Table 1, which we intend to demonstrate the credibility of the ultrasonic sounder; and keep Figure 4 in the text, because it's important to illustrate that the wind consistency is quite high, as our opinion.

There are also inconsistencies. On p5 it is noted that the snow accumulation is 30.33 cm (where does this come from and for what period?), yet in the following para it states that 95 cm of snow accumulated between January 2005 and January 2007 (47.5 cm/year). Yet Fig 6 shows 95 cm between Jan 2005 and Dec 2007 (32 cm/yr) and the seasonal stratigraphy on the side of Fig 6 gives about 40 cm/yr over the 2 years from Spring 05 to Spring 07.

A: Very sorry for the mistake in the text, the 95cm snow was accumulated during Jan 2005 and Jan 2008. It has been modified.

The accumulation in Ma et al. [2010] covered the period from Jan 2005 to Dec 2007, and it fit well with our calculation (91 cm +one month accumulation ≈ 95 cm).

Other minor problems (far from an exhaustive list) include: â˘A´c use of undefined acronyms (SPWD, EPICA, NEEM, etc.) â˘A ´c what is the difference between "densification" and "firnification"? â˘A ´c p5, l18 temperature should be -40.8C â˘A ´c p5, l18 what are accumulation units? cm of snow? â˘A ´c Fig 6 caption: "...wind speed between January 2005 and December 2007".

A: We modified the text about undefined acronyms and the others.

Densification it the increase of density by new snow precipitated on the top, sublimation or condensation of the snow layer, firnification… etc… but 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.)