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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 #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

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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.

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).

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.

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.

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

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

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