Anonymous Referee #3

General comments

The paper deals with the important issue of dating uncertainties of polar ice cores with the focus on the role of discontinuous snow deposition due to snowdrift events. The authors present a comparison study between meteorological observations measured with the means of an automatic weather station and snow pit investigations. They performed two different dating procedures over a 3 year period, one based on the precipitation and temperature data of the weather station and one based on the chemical/isotopic records of the snow pit samples. The dating differences cumulate to 1 year which is quite large in respect to the 3 year period of consideration. Therefore they concluded that snowdrift processes have a significant influence on ice core dating. Due to the close vicinity of the area of investigation and Dome A they underline the importance of their findings especially for the dating of the deep ice core at Dome A.

In my opinion the paper in its actual configuration might be not suitable for publication in TCD for several reasons. The structure of the paper confused me. There is no separate chapter where the results are presented. Therefore it was very hard to distinguish between observations, implications and conclusions. The dating methods are not well explained. A critical discussion about the interpretation of the deposition records from the ultrasonic sounder is missing. The conclusion that the dating differences are solely due to snowdrift events is not convincing me. There is no discussion about the postdepositional change of the isotopic signal. On the other side there is unnecessary information presented that is not used for dating or other conclusions (density profile, wind direction, : : :). The generalization of their findings and the special importance for Dome A seems to be doubtful since Dome A has much lower accumulation and much less of wind. Overall, I would suggest major restructuring and rewriting before publication.

Thanks for the kind comments on the draft. According to the comments, we reorganized the structure of the paper and add more discussions in. Especially, we deleted some table and modified some figure. We also add a figure on the correlation between wind and snow loss/gain. Some induction about Dome A was not proper from our data, thus we modified it .

In particular, we also improve the paper according specific comments, please find it in the

following part.

Specific comments

#1 The historical context of ice coring projects is not necessary to describe.

A: It has been reduced.

#2 Acronyms are not explained including EPICA, NEEM, GNIP (GRIP?), SPWD, SMB).

A: It has been modified.

#3 Introduction: The enormous difference of 1275 yr in the dating of a shallow firn core by Jiang et al.(2012) and Li et al. (2012) might have other reasons than the suggested one of a disturbance of the stratigraphy by post-depositional events. In general, the dating is performed using well-documented volcanic events measured in the ice by chemical analysis or dielectric profiling.

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

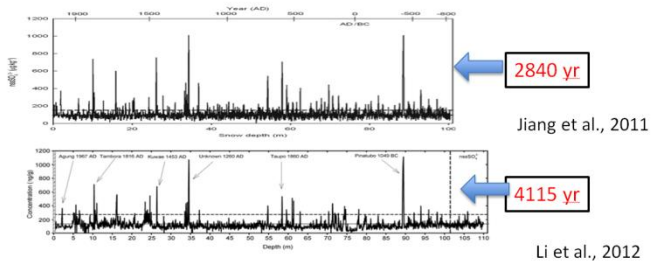

Figure S1.

#4 Accumulation must be related to water or ice equivalent (page 1419: line 18, page 1438: Fig. 6)

A: It has been modified into “cm-snow”.

#5 Figure 7 is confusing me. Is Fig 7a the result of the dating mismatch? And what is the typical regression between d18O and Temperature in this area? The labels on the axes of Figure 7 are to small.

A: The label has been enlarged. The typical regression between d18O and T is d18O=0.842T-9.118 [Ding et al., 2010, doi: 10.1007/s11434-010-3179-3], which is large different with the Eagle pit.

#6 It would be helpful if additional information about the snow pit’s stratigraphy (layering, crusts) and more chemical components like Ca2+ are available.

A: We added the new chemical records such as MSA, SO_4^{2-}, Ca^{2+}.