

Interactive comment on “The WAIS-Divide deep ice core WD2014 chronology – Part 2: Methane synchronization (68–31 ka BP) and the gas age-ice age difference” by C. Buizert et al.

Anonymous Referee #1

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This manuscript presents the chronology (gas and ice) for the WAIS-Divide deep ice core back to 67 ka BP. This chronology is built using constraints from the GICC05 chronology, the Hulu chronology and Dage estimate from $\delta^{15}\text{N}$ measurements all along the WAIS ice core. This is an important study that should be published. However, before that, some comments should be addressed and complements brought to the paper.

1- The most important one is the link to the Hulu chronology. The new chronology for Hulu cave is not presented in this paper except for the short period between 58 and 60 ka BP. It does not seem to have been published elsewhere. As a consequence, it is not really possible to support the chronology of WAIS based on Hulu chronology if the

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later is not shown / published.

2- It is very difficult to understand how the link was done to the Hulu chronology. In the text, the authors explain that they use either warming or warming + cooling. When looking at Tables 1 and 2, it is clear that the link to Hulu has been made only through warming but cooling are linked to NorthGRIP chronology only. If the authors claim that there is a direct relationship between Hulu d18O and WAIS CH4 and/or NorthGRIP d18O for the warming, why should it not be valid for cooling ? Actually, when looking at figure 5, the shapes of events recorded in Hulu d18O does not always reflect shapes of CH4 and NorthGRIP d18O of the same events (e.g. shoulder at 59.5 ka BP in the Hulu record). This raises question on the correspondence between Hulu variations and CH4 and/or Greenland water d18O records. This correspondence should be much more discussed in this paper before giving this ice core chronology based on speleothem dating.

3- A wealth of firnification models have been developed over the last 30 years. Why then have the authors chosen to use the Herron and Langway model which is one of the oldest model with only empirical parameterization ? The author states that they have compared this model with other firnification models but no comparison is shown which could have been useful to quantify the uncertainty in Dage calculation due to the use of a particular model.

4- The calculation of $\xi(t)$ at the bottom of p. 3545 and the top of p. 3546 and in figure 2 is unclear. Please rewrite more clearly how the accumulation rate scenarios are determined. I think that it may be useful to display the two Ainit scenarios on Figure 2 in addition to the final A(t) scenarios / or show the $\xi(t)$ functions.

5- The discussion I. 7 – I. 23 on p. 3554 is difficult to follow without the Hulu data.

6- p. 3555 : there are some inconsistencies in the text when you discuss the phasing between CH4 and Greenland temperature (in phase or not ? I. 10 and I. 17). Baumgartner et al. Have clearly identified lags of methane over Greenland temperature over

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DO 5, 9, 10, 11, 13, 15, 19 and 20.

7- The discussion is very disappointed. Indeed the authors suggest many applications but do not show any. At least one figure showing the seesaw relationship of WAIS vs NorthGRIP should be added since the new chronology is partly linked to the GICC05 chronology.

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