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Climate of the Past Discussions

Interactive comment on "Analytical constraints on layered gas trapping and smoothing of atmospheric variability in ice under low accumulation conditions" *by* Kévin Fourteau et al.

Anonymous Referee #1

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This paper analyses CH_4 across Dansgaard/Oeschger event 17 (DO-17) from the Vostok ice core with a CFA-based measurement system in order to improve understanding of layered gas trapping and smoothing of atmospheric variability in an ice core drilled in low accumulation areas. A thus dervied CH_4 record is then postprocessed and finally compared with CH_4 from the higher resolving WAIS Divide ice core (WDC) to conclude that gas age distribution (GAD) - or smoothing - in Vostok seems to be similar for modern and DO-17 conditions.

The paper is well written, especially the post-processing procedure described to

some detail. The detection of artifacts in CH_4 from such a high-resolution system as presented here is convincing.

However, the paper falls so far short in one aspect, that is the application of a previously assumed LGM gas age distribution used for EPICA Dome C (EDC) to transfer WDC CH_4 data into potentially signals recorded in Vostok, from which it was concluded, that gas age distribution are probably independent from climate background. Here, they use what has been used as gas age distribution in Köhler et al (2011), who used a log-normal function, and for LGM assumed a mean width of the GAD of 590 years. This GAD allowed large overshoots in the true atmospheric signal of CO_2, when compared with the EDC ice core record of CO_2, and was again used in Köhler et al. (2014). However, the new WDC CO_2 paper of Marcott et al (2014) showed, that the assumed GAD used by Köhler in 2011 was probably too wide since a much smaller GAD was able to transfer the WDC CO_2 (potentially very close to the true atmospheric variability of CO_2) to the CO_2 record obtained from EDC (Extended Data Fig 5 in Marcott et al., 2014). This revised narrow GAD was also then applied for the question of interest by Köhler, but I have to admit, so far only published in a conference proceeding, not widely known (Köhler et al., 2015, pages 135–140 in http://www.leopoldina.org/uploads/tx_leopublication/NAL_Bd121_Nr408_LR.pdf). Figure 2 of this 6-pages proceeding contains a transformation of a simulated atmospheric CO_2 into a signal recorded at EDC around the onset of the Bølling/Allerød (B/A) warm period around 14.6 kyr BP using the same log-normal function as introduced in Köhler et al (2011) of

$$y = \frac{1}{x \cdot \sigma \cdot \sqrt{2\pi}} \cdot e^{-0.5(\frac{ln(x)-\mu}{\sigma})^2} \quad (1)$$

with x (yr) as the time elapsed since the last exchange with the atmosphere, which leads to an *expected value (mean)* E of the GAD of

$$E = e^{\mu+\frac{\sigma}{2}}. \quad (2)$$

From the two free parameters μ and σ, in 2011 Köhler has chosen for simplicity $\sigma = 1$, but now in the revised application in 2015 uses $\sigma < 1$ to reproduce the shape of the GAD suggested in Marcott. In detail $\sigma = 0.5$ was used and μ defined in a way which guarantees the pre-defined mean values E of 150 yr. So, not only the mean width of the GAD has be reduced by a factor of 2.7, from formally 400 yrs to now 150 yrs (for this application to the B/A), but also the shape of the GAD.

I believe the authors are challenged now to also use a GAD that agrees with the WDC-EDC CO_2 comparsion, as brought up by Marcott, and in a first step probably at best also start with a revised log-normal function using different parameter values. Only then can they conclude (or not) if the GAD is indeed similar for modern and DO-17 climate or not. For any such exercise, please always state the used parameter values of the function, e.g. as given here, both the chosen form-shape factor σ, and at best the mean value E (directly derived from μ once σ is given). So far, no details on the applied log-normal function has been given. This will probably lead to a revision of the final conclusion and figure 5, but the rest of the paper is largely unaffected.

Further minor comments in chronological order:

1. Throughout the paper: Units are sometimes weight, with a dot (.) inbetween, e.g. "3.8 cm.min^{-1}", which should be "3.8 cm min^{-1}".
2. Figure 1: Labels in insert (top right corner) are much too small.
3. Page 9, line 4: " As explained in Rhodes et al. (2016), such a mechanism affects trace gases record only during periods of significant atmospheric variations." Variations of what? Probably "variations in concentrations of atmospheric gases".
4. Page 9, line 11: "monotonous variations", change to "monotonous in/decrease".

5. Page 11, line 21: What are the coldest sites in Breant et al (2016)? Please name here.
6. Page 12, line 4: "... the methane record from the WAIS Divide ice core (Rhodes et al., 2015), with gas ages with gas ages converted on the AICC2012 scale (Buizert et al., 2015)." Now, this needs some more explanation and probably correction. Buizert et al., 2015 does NOT plot WDC CH_4 on AICC2012, as suggested by this sentence. There is also the effort of explaining the gas age adaption of WDC CH_4 to AICC2012 in the SI Fig S11 (and corresponding SI section), which I also did not understand in detail. Please be precise here, and describe this step in the main text, not hidden in the SI.
7. Page 14, line 3: I believe, a spline normally comes along with a cutoff-frequency, which has not been given here.
8. Section 4.4 (removing artifacts) page 13-14, versus Fig 1. My understanding of the description of Section 4.4 was, that the spikes caused by layering artifacts are removed, and a continous CH_4 time series without artifacts is generated. However, the black line in Fig 1 (which according to the text should be such a time series) does not contain any data in the periods, in which artifacts has been removed. I would think the post-processing should give you some data points in exchange to the removed artifical peaks. Please refine text, or change Figure 1 accordingly.
9. Caption to Fig 5: You need to say explicitly WHICH signals you convoluted, e.g. green solid is probably the convolution of the WDC CH_4 with the Dome C GAD estimated for LGM of Köhler et al 2011.
10. Page 16, line 20: "modifying its two parameters", probably refers to the same 2 parameters given above in Eq 1. Please state, which values you choose in the end.

11. Fig 6: Needs a new GAD based on the Marcott WDC-EDC CO_2 comparison, and/or the new approach of Köhler 2015.

12. SI: Please either put all Figures to the end, or in the section, in which they are discussed.

13. Please check references to Figures in main text, on SI page 5, line 4 a reference is given to Figre 6, but the correct Figure refered to here is Figure 5.

References

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