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Climate of the Past Discussions

Interactive comment on "Rapid changes in ice core gas records – Part 1: On the accuracy of methane synchronisation of ice cores" *by* P. Köhler

Anonymous Referee #1

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Review of Köhler, P., 'Rapid changes in ice core has records – Part 1: On the accuracy of methane synchronisation of ice cores'

The manuscript describes the effect of ice core gas age distributions on the technique of CH4 tuning to align records from difference ice cores.

This paper left me thinking that there must be a significant gap in my understanding of things. But I'm also left wondering exactly what is the step forward here? As Joos and Spahni (2008) state, the in-mixing of contemporaneous atmospheric gases with (by necessity) older snow / ice before closure leads to (1) an age difference between the trapped air and surrounding ice and (2) an age distribution of gas in any one bubble of

air. As they note, the first of these is accounted for during the construction of age scales for the ice core (and it is known as Δage). The second effect causes a smoothing of temporal atmospheric variations along the length of the record (I would have thought that this effect would also figure in the calculation of Δage but someone please correct me if I am wrong). I believe that the accompanying paper to this one (by Kohler et al.) implicates the second of these effects in the interpretation of the sharp rise in CO2 at the onset of the Bølling-Allerød. That is an interesting application. The present ms however seems to be lacking a coherent product. For example can this analysis be used to improve our understanding of bipolar climate linkages or is it just that we have to increase our estimate of uncertainty?

It would be helpful if the author could define exactly what is different (quantitatively) about his use of E (or 0.58E) and Δage, as used by other authors (i.e. is it just a larger correction?). This would help (me at least) to see how the approach described here may represent a step forward. I believe that this is the most significant weakness of the present ms. A simple comparison, using real data, of the difference between the traditional Dage approach and the approach described here would probably suffice to provide the additional content necessary for publication. Also there seems to be two points to the paper. One is an underestimation of the correction that needs to be taken into account (I think) and the other is the approach to correcting for it. But if I am wrong it is because the paper is perhaps not as clear as it could be.

I guess Figure 4 is central to the argument. Fig, 4A demonstrates what we know is true, that a sharp change in e.g. CH4 will be dampened and shifted back in time with respect to the ice matrix. But that is the key, it is with respect to the ice matrix, not time itself. All we need to know is the age offset between the (mid point) of the CH4 shift and the age of the ice which surrounds it. If we take the approach in Fig. 4B we would presumably be shifting the ice-based records forwards in time by differing amounts depending on the value of E. We should find that synchronous changes in e.g. δ18O should be aligned by this procedure. But we should get exactly the same result by aligning the in-situ CH4 records and then shifting the ice-based records forwards in time by differing amounts depending on Δage. The author should show us what difference this actually makes.

Fig. 4D suggests that if we synchronize the mid-points of the B-A CH4 shift within different ice cores, we would misrepresent the actual evolution of atmospheric CH4. But I do not think that is what is implied by the procedure at all. In fact the supposition is that CH4 changes are globally synchronous but that these changes are not recorded by ice cores in the same way, which means that a correction must be made to take this into account (i.e. via Δage).

Figure 3 seems to be about the actual approach to correction. Let's say our goal is to determine the relative timing of temperature changes in NGRIP and EDC. To be short – what is the benefit of using approach 3B over 3A? (the cartoon records of NGRIP δ18O and EDC δD look the same in each..?). The age offset between ice and gas is central to both approaches and whether it is taken into account during or after alignment should not make a difference (as suggested by the result). Perhaps I am missing something here.

In conclusion I am left a little confused by this paper – and it has taken far too long to get through. This is somewhat my fault but also I think the paper could be better set out and the explanations made clearer. Some of the sentences could use clarification.

Other comments:

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The opening line of the abstract sounds like it should be within the supplementary information of a separate study. CH4 synchronisation is a concept used to align ice core records. PERIOD.

Line 4: Atmospheric gases are recorded in ice cores – you mean their concentration? Their isotopic fractionation? You need to be more specific.

Line13: Odd referral to the bipolar seesaw – this should be introduced properly if it is to be included in the abstract. Again this makes it sound like it should be part of the supplementary material of a different paper.

Line 21: Changes in what?

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Line 8/9 – does this mean there is no hope to either approach?

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Line 27 and onto next page: this makes it sound like 0.58E and Δage are equivalent – is this the case?

Figure 4. Synthetic CH4 shift should be common to all panels (i.e. D should be 50yr or A-C should be 200yr)

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