**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

**EW Wolff (Referee)**

ewwo@bas.ac.uk

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The use of methane synchronisation between cores is central to many of the most important chronology issues in ice core science, so we should certainly do it to the best of our ability, and be aware of the uncertainties and implicit errors involved. It is of course well-known that the methane record seen in ice is a filtered/smoothed version of the atmospheric record, because air of a given age is enclosed over a range of depths. Because the filter has a different width at different sites, it has implications for the way a particular atmospheric methane signal is recorded in ice at different sites. The consequences of it on ice core amplitudes were covered by Spahni et al (2003), but it is

probably true that it has not been discussed explicitly in relation to the synchronisation of ice cores.

This paper treats the issue in an approximate mathematical way and makes an estimate of the way that it might affect the accuracy of synchronisation. I don't doubt that it is an issue that should be considered as age scales improve. The question in this paper is whether it (a) uncovers a problem as significant as suggested and (b) makes a sufficiently important point to warrant a paper in its own right (or could be folded into part 2).

The author separates out the issue of the synchronisation of methane between two sites and the issue of estimating the delta-age value at each site. In order to follow his logic I will do the same for now. However, later I will consider how they relate to each other, as this also affects this discussion. As the author acknowledges, any correction is very small where the width of the age distribution is small (sites with high snowfall rates), or similar at the two sites being synchronised. (It can also be avoided completely by estimating the start of the methane change rather than the mid-point of the transition - but see later as this may complicate the delta-age step). The mid-point seems to have been used in the most recent synchronisations (by Blunier et al 2007, EPICA 2006, and Lemieux-Dudon et al 2010 (LD)), and that this is a case where one of the sites had quite a wide age distribution. This is the situation the present paper wants to improve.

The authors listed above did not specify exactly what their thinking was, but they do indeed appear to have aligned the mid-points of the CH4 transition and then simply applied an uncertainty large enough to cover the "synchronisation error". One can guess that they did this because they recognised that this was not the largest component of their uncertainty – see for example Blunier 2007 Fig 3, where it is clear that the largest part of the error in synchronising between north and south is the delta-age at DML. I can therefore accept the argument that the systematic error implied in this paper could have been noted.

However, unfortunately the present paper exaggerates the problem by concentrating on the size of the problem in the case where EDC methane is used for synchronisation with NGRIP. In fact, this was not done in any of the papers discussed. In Blunier 2007 and EPICA 2006, the synchronisation was done between NGRIP and EDML CH4. In LD, multiple constraints are used, and one has to think what has actually controlled the way that the comparative timing of climate events in north and south is set. Effectively CH4 is used to synchronise NGRIP-EDML, while the EDML-EDC linkage has effectively to find a compromise between methane synchronisation of its gas scale, volcanic synchronisation of its ice age scale, and a reasonable densification model. In practice, the relative timing of events is set through EDML, because the uncertainties in the EDML-EDC ice synchronisation are small. While this is quite technical it argues that using the NGRIP-EDC match as the example is unreasonable, and the statement on page 1459, line 11 is not strictly relevant; the worst case is actually EDML-NGRIP in the LGM, where the author suggests an error of 88 years, less than the quoted error in the papers.

Until now, I have accepted that we can assess the methane synchronisation error by itself. However, the final purpose is not generally to align the methane records (although that would be the case if we were trying to estimate interhemispheric differences in methane concentration). The point is generally to align the ice records, involving calculation of delta-age at each site. So, we need to consider what delta-age is. It is generally described as the difference between the age of the ice and the mean age of the air at the same depth. So, let's take a situation such as a Dome C-NGRIP synchronisation for the LGM, use data in Table 1, and assume an instantaneous jump (I'll refer to it as the impulse) in methane concentration at t=0. If we assume a lognormal distribution as the author does, then we will identify the mid point of the rise as being when 50% of the air is from after t=0, which is (I think) the median, ie at 0.6*E (see comment below). Thus we would "synchronise" the gas records at t=54 in NGRIP and t=360 at EDC (this is essentially the estimate at line 5 of page 1459). But now we would apply the delta age at each site, which would be calculating the age difference between the

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age of the ice and the mean age of the enclosed air. The mean age of the enclosed air at the synchronisation point is E older than the age at which the impulse occurred, ie 90 years for NGRIP and 600 years for EDC. Thus when I calculate the delta-ages, and then synchronise the ice records, the actual error I introduce is the difference between (E-0.6E) at the two sites, ie 204 years. In the real worst case scenario (EDML-NGRIP, LGM), the introduced error is now only 60 years.

The paper has therefore actually made the wrong point: the error actually arises NOT because we have used the midpoint of the rise, but because we have used a different statistic for the two steps in aligning the ice records. For the CH4 match we have used the median age of the air, while for the delta-age calculation we have (according to the literature) used the mean age of the air, which is a larger number because of the long tail. Thus a correct recommendation from this paper would be that, if the mid point of the rise is used, then the median of the age distribution should be used as the statistic for delta age. I have to admit that I don't actually know what people do use in calculating delta age, ie whether they really calculate the mean age of the air or the median age of the air. We need input from one of the delta-age modellers for this one.

We are therefore left with this: the author is correctly pointing out a systematic error in the methane synchronisation itself; this error was certainly understood by earlier authors, but they chose not to address or discuss it. However, for the alignment between the ice records, the true issue is a different one, relating to the possible inconsistent use of different statistics (or perhaps an assumption that the median and mean age are very similar). The possible error remains below the quoted uncertainties and makes a very small change to the overall alignment of climate records north and south. The material in this paper, with an appropriate recommendation (which I suggest would be to carefully describe and use the same statistic) is of interest and might make a short technical note (shorter than the present), or a section embedded in part 2 of the paper, if that is going to go forward. However I don't feel it warrants a full paper of the current length.

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There are some issues with the paper which I describe below, and which should be considered by readers who cite the CPD version of the paper.

Methods, page 1457, line 5. Should read E=mu+0.5 (not minus). This is clearly incorrect, the plus sign is in the literature, and is needed to reproduce the results shown in Fig 2.

Line 13, important to include a comma after "not be confused with", otherwise the sentence has the wrong meaning.

Line 25 and 5 lines on, and Fig 4. The author must be thinking about this in a very different way to me, because although I follow perfectly what his issue is, the phrase "shifts the onset. . .by E years towards older ages" makes no sense to me; nor does Fig 4 a which appears to show the increased concentration being recorded before it has happened. I suggest that, if this goes forward in any form, the paragraph should be rewritten, and Fig 4 should miss out panel A.

Page 1458 line 10 and surrounds and Fig 5. This is a strange section: in the case of an instant change in concentration, there is an analytical answer, because in the lognormal case the median (which is the point at which 50% of the rise is seen) is at exp(mu) and the mean (E) is at exp(mu+0.5). Thus the median is at exp (-0.5) of E, ie 0.60E. Of course the ratio will change slightly in the messier case where the methane rise is not instant but it hardly warrants a figure and discussion.

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