

Interactive comment on “A refined TALDICE-1a age scale from 55 to 112 ka before present for the Talos Dome ice core based on high-resolution methane measurements” by S. Schüpbach et al.

Dr Schaefer (Referee)

h.schaefer@niwa.co.nz

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The study presents a revised age scale for a substantial period of the time covered by the TALDICE ice core and reports strongly reduced dating uncertainties. The results are a marked improvement with potential implications for the interpretation of palaeoclimatic ties between different regions. The work showcases the power of a newly developed continuous flow technique for high-resolution methane records that can be closely matched between different cores. A comparison of dust fluxes is used to evaluate the refined results in a useful and illustrative exercise. I consider the study a valuable contribution that should be published in *Climate of the Past*.

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Several details can and should be improved before publication as described in the following.

General comment: the choice of new tie points is well documented and discussed for the period 50–90 ka BP. In contrast, the period from 90–120 ka BP is only documented in Table 1 and Fig. 3, which leaves room for questions. How do the new age tie-points relate to those defined by Buiron et al. at the maximum of DO 24 and at the minimum between DOs 23 and 24? How big are the changes in assigned age at the inception of the last ice age? What is the estimated dating uncertainty in that interval? A short discussion of these and maybe related points would be a useful and arguably necessary addition to the manuscript.

Page 1177 Lines 10–11: a short note that these are present-day estimates but most probably didn't change substantially under the past conditions of the studied periods would be worthwhile.

Lines 18–22: the Köhler (2010) discussion paper has received criticism for likely overestimating the quantified offset and for being unclear in its arguments (CPD discussion forum). Personally, I agree at least with the latter, given that Köhler's Fig. 4(B) gives the impression that the mid-points of fast CH₄ transitions are mostly back-dated because he first adjusts the transition onset. Thus, he ignores the reason why mid-points were chosen as tie-points in the first place. I am possibly mistaken in this, but the authors may wish to discuss Köhler's results more comprehensively. I note that in the current version the authors take a conservative stance, as Köhler's mechanism worsens their dating uncertainty.

Lines 22–27: as above, the authors list a mechanism that has the potential to introduce uncertainty in their age matching. However, the matching is usually based on inflection points, which are independent of absolute methane values in the two hemispheres. Given that modern CH₄ variations are mirrored between the highest latitude northern and southern monitoring sites of the ESRL network on a year-by-year basis, it is not

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clear why the palaeo-situation should be different. I may be unaware of an argument here, if so it should be presented and referenced properly for the interested reader.

Page 1178 Lines 7-10: is this a reversal of the real argument? Buiron et al. matched the record to the Greenland ensemble and in so doing derived the age uncertainty.

Page 1179 Lines 10-14: I agree with the assessment that accuracy and precision of the method are not limiting for the study. In terms of the latter, quoting the magnitude of atmospheric CH₄ variability would be helpful for the reader here.

Line 28 and Page 1180, lines 1-2: the reason for applying a filter to the continuous flow data during the ACR, namely to subtract measurement noise from the real atmospheric signal, is no longer valid if the temporal resolution of the discrete measurements is lower than the temporal variability of the atmospheric signal. As the authors state, in the time period that is of interest for this study temporal resolution of the methane CFA is considerably lower than during the ACR (while the filtering process remains unchanged). Is it still higher than time scales of CH₄ variability? If not, the filter has the potential to mask true atmospheric variability. Although I don't see this issue having implications on the results of the study, the point should be discussed more quantitatively.

Page 1180 Line: a short note how the uncertainty was quantified would be helpful.

Lines 10-14: see previous comments on the Köhler (2010) study.

Page 1183 Lines 26-27: it would seem more appropriate to say that the Ca records show a high degree of correlation rather than synchrony, as the latter is a product of the assigned age scale and the former an inherent quality of the data.

Figures 1-3: Personally, I find the black and blue diamonds hard to distinguish. I would recommend a different choice of symbols or colours so that details in the records can be distinguished.

Figure 1: a main purpose of Fig. 1 is to illustrate the additional structure in the CH₄ C520

record that the CF method reveals in comparison to the discrete measurements of Buiron et al. I think that this could be better achieved by presenting the latter as a line plot (or data points connected by a line). This would show at first sight where the new data resolve, e.g., a precursor event and a DO where the discrete data give the impression of one single peak.

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