

Interactive comment on “A revised Law Dome age model (LD2017) and implications for last glacial climate” by Jason Roberts et al.

Anonymous Referee #3

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Roberts and colleagues describe a new ice age scale for the Law Dome ice core in Antarctica, that is based on a combination of layer counting and stratigraphic matching based on volcanic deposits, CH₄ d18O-atm and d18O-ice. The combination of these different approaches is somewhat ad-hoc, but I understand age scale construction is a pragmatic endeavor and one must make use of what is available.

Clim. Past has a history of publishing ice core time scales, and therefore the manuscript fits well within the scope of the journal. However, the work has some clear shortcomings that need to be addressed.

The authors do not show any of the data used in constructing the age scale. Figures need to be included, so readers can judge for themselves whether the CH₄, volcanic and d18O matches are valid. LD records should be plotted with the target records.

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While the authors describe the Delta-age modeling in great detail, they do not reconstruct it for the entire core, and consequently there is no gas age scale. This should be included, for LD2017 to be a complete chronology. Since the modeling has already been done, this will be trivial to add. Having a gas age scale is further crucial for validating the CH₄ and d18O-atm matching described in the text.

I would like the authors to consider converting the time scale from B2k to BP, with present taken to be 1950. The latter is the convention in nearly all fields of paleoclimate (14C dating, U/Th dating, paleoceanography, AICC2012, WD2014, etc). B2k is an unfortunate choice used solely in the GICC05 chronology, that has caused nothing but confusion without being an improvement in any way.

The authors match the ice-ties to AICC2012, and the gas-ties to GICC05. This is potentially very problematic, because these two chronologies are of course not perfectly synchronized (due to the large Antarctic Delta-age). The uncertainty in the AICC2012-GICC05 synchronization is easily several hundred years, i.e. larger than the stated Delta-age.

I have several comments regarding Delta-age:

*The delta-age calculation uses very unorthodox approach to estimating past accumulation, by using the notion that past basal temperatures must remain below the melting point. However, they use a steady-state approach, which is far from perfect given that the LGM minimum only lasted for a short duration. Also, the ice thickness is far from certain, as there are virtually no data-based constraints.

*It is unclear what the Delta-age in Table 2 is based on – it appears to be the higher value based on vapor pressure. We know this is a poor estimate in coastal locations due to large contributions from storm systems. *Why is the Delta-age so much smaller than previous estimates by Pedro et al?

*How does the modeled firn column thickness agree with the d15N data shown by

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Landais et al. 2006? That would be the better constraint on the problem than the unconventional ones used here.

*The authors can actually test the validity of the Delta age around 15ka, where they have a gas-based and volcanic-based tie in close proximity. Are both consistent?

The stated age uncertainty is much too small for a core that has so few age constraints. At the very least, the AICC2012 uncertainty should be added (in quadrature) to the LD2017 uncertainty, given that those uncertainties propagate into LD2017.

Why are the upper 760 m not included in the LD2017 chronology given in the supplement?

The timing of climatic variations seems to worsen using the LD2017 chronology. In the pedro et al. chronology deglacial warming started around 17.8ka, like the rest of Antarctica, whereas in LD2017 it is 18.8ka, which is much too early. However, it is hard for the reader to evaluate the agreement with other records, since no $\delta^{18}O$ data are shown.

Throughout the MS the authors refer to "age-ties", which is an incorrect concept, in my view. What they do is to derive stratigraphic ties, either in the gas or ice phase, after which they use another age model to assign an age to these stratigraphic markers. The tie is not to an age, but to another record. When the EDC chronology changes, the LD2017 chronology should change along with it.

The method of piecewise parabolic fitting is not at all well described. It was not clear to me how they ensure continuity over the age points. Also, how are the fitting parameters a, b and c found? The difference with the smooth layer method of Fudge et al. is not well justified.

It is not clear what the implications are for the last glacial climate that are promised in the title

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