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Climate of the Past
Discussions

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

Anonymous Referee #1

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The Law Dome DSS ice core is an important palaeoclimate archive and so its age model is important and the methodology and outcome of a new reference age model expected to last for many years deserves publication. Unfortunately this paper, while providing (as SI) the outcome of the age model, provides a very inadequate justification for the procedures behind the model. The purpose of such a paper has to be to fully justify the validity of the age model, and this requires providing information that allows the reader to judge that; this paper doesn't do that. I will go into more detail but among the issues are:

*The top part of the model relies on layer counting but NO examples of the annual cycles are shown to justify the low error assigned

* The next set of ties are volcanic ties to EDC, but again absolutely no records of

this are shown so the matching (which can be a very tricky business) has to be taken entirely on trust

* Similarly the dust match is not shown, denying the reader any chance to judge whether the match points are well-defined

* The isotope matches are also not shown. This case is even more severe because in work at other sites, authors have tried to avoid del18O matches because they assume climate synchoneity, the very issue that is often of value. While this may be unavoidable here, the fact that it is not even discussed is not acceptable

* Methane matches are again not shown, so again we cannot judge whether they are appropriate

* On the other hand a large amount of the paper is devoted to a rather strange calculation of delta-age, which thankfully is small but extremely poorly known

* The palaeoclimate implications of the new age model are not discussed, which must be the main scientific interest of the new age model

* I am willing to be corrected but to my knowledge many of the data underlying this work have not been published and certainly not been posted on databases. I can find at NOAA palaeoclimate the oxygen isotope data and very low-resolution (inadequate for this study) methane data, but not the chemistry for layer counting, sulfate data for volcanoes, dust or methane. Authors are required at least to state how the data can be accessed, and preferably should deposit data on a recognised database.

In summary this paper needs a huge amount of work. I was tempted to simply reject it, but do not want to send the message that an age model paper is not appropriate. However this is a very long way from being the right age model paper, and needs very very major revision.

Specific comments:

A picky comment but please regularise your use of age units, consulting https://www.climate-of-the-past.net/for_authors/manuscript_preparation.html (see abbreviations section). You should replace y with yr or a as appropriate.

Related to this it's a valid choice to use b2k, but slightly perverse when stating that ages are AICC2012 ages, since AICC2012 is bp. You need a sentence to explain to the unsuspecting reader so that they don't accidentally introduce 50 year errors. Finally in Fig 3 b2k not B2k for consistency.

Section 1, page 3, para 3. Somehow here and elsewhere the authors imply that using a model to simultaneously infer ages and accumulation is inferior. This may be the case at LD where the geography, and the fact that most of the years are in ice very near the base, means that the layer thinning is badly-behaved and cannot be treated properly (although I would like to be convinced that the uncertainty in later thinning will be so great that you cannot improve on the factor 3 uncertainty in acc rate you end up with from doing accumulation independently). However in other cases such as AICC2012, it is a clear advantage to use the two together because it means that the known (and in the case of most AICC sites until the lowest layers, well-behaved) physics of layer thinning and snow accumulation is used as a self-consistent constraint on the age. Given the incredibly unconstrained accumulation rate that is derived later it seems rather ridiculous to suggest that not deriving accumulation is somehow a virtue. This needs rewording to explain the particular circumstances of LD.

Overall methodology: the methodology used here is to take a small number (below the layer counting) of fixed ages and fitting a smoothed function through them. This is a reasonable approach, but given that everything is being keyed to AICC2012, I am wondering if the authors considered simply using the software used to make AICC2012 (or working with AICC2012 authors), and simply including LD in the framework. Perhaps the reason not to do that was that it would produce something slightly different – an absolute age model incorporating LD, whereas the current approach really produces a relative age model (LD keyed onto AICC2012). But anyway it would be worth

discussing the approaches and justifying the one taken right at the start of section 2.

Section 2 – you must show at least one example of the annual layer counting near the bottom of the layer counted section so that the reader can judge the validity of your uncertainty estimate (showing a section including one of the uncertain years would be the most informative). This has always been done in age model papers (eg Sigl et al 2016 for WD), and seems mandatory unless the layer counting is being published elsewhere simultaneously (there is no indication of that).

Section 3.1. Again I consider it mandatory to show the synchronisation of the volcanic layers in figure form so the reader can judge whether you have made unequivocal matches or not. Again there is good precedent for presenting such evidence eg in Fujita et al (DF vs EDC) and Parrenin et al 2012 (V vs EDC).

Section 3.1 again. You need to also show the dust record match LD vs EDC. The previous matching referred to is shown in van Ommen et al 2004 at very coarse resolution – one could not possibly tie two records together within 1000 years let alone the strangely precise 328 years from that. So we need to see the data lined up please. This turns out to be crucial in dating the start of Termination I discussed later for example.

3.1 again. For the isotopes there are two issues. Firstly we need to see the data. The Stenni reference given contains no LD data – it is van Ommen et al (2004) that should be cited here for the LD data. The resolution and sharpness of signals is clearly essential in assessing these ties and we need to see the data lined up with EDC, with the tie points drawn on.

In addition, you need to explain the compromise you are making by using del18O data matches. There is a good theoretical justification that single events (like volcanic eruptions) and signals with a common source (dust) must be synchronous, but there is no reason the slow climate signals have to be synchronous and indeed papers such as WD (2013) made important papers out of discussing a non-synchroneity. By using the climate signal to tie the records you are losing any chance to discuss phasing between signals around Antarctica. I understand the need for this compromise in order to develop a useable age model, but it requires discussion.

Page 4, line 30. "upper heat flux" – do you mean "limits based on the heat flux"? As written it doesn't quite make sense.

Sections 3.3 and 3.4. An enormous effort goes into this calculation but in the end it gives you accumulation rates with an uncertainty by factor 3! And I am not convinced by the basis for the upper or lower limits. For the upper limits you do the thermodynamic calculation of accumulation based on isotope values. But while this has theoretical validity for the Antarctic Plateau, and therefore perhaps for LD when acc rates were much lower in the LGM, it has no real validity for a high accumulation situation with synoptic precipitation. It no doubt gives some empirical estimate but I am not sure it really gives an upper limit.

The lower limit based on heat flux is based on the idea that there has never been basal melting at LD. I understand the present-day basal T is -7 so more than likely this is true, but I don't see what the assertion is based on. If there was basal melting at, say, 40 ka, then all we know is that it didn't melt the ice from the LIG (as an example, there is basal melting at Dome C today but ice from 800 ka still exists at the bed, and an assumption of no basal melt would be wrong). If you can justify this calculation then there are also a couple of strange details: page 7, line 15, you assume steady state, but why should the ice be in steady state if the accumulation rate is changing so hugely? And line 22, I don't understand the phrase "linear change between present ice thickness at 100 ky and 30 ky" – please explain.

In the end I don't see how these calculations, based on uncertain assumptions, constrain the delta depth or delta age at all. I was then rather astonished to find that you have access to some 15N data that at least constrain delta-depth, and give a physically based estimate of accumulation rate that you choose to ignore (page 17). I don't have a great proposal as to how to constrain delta age but I don't think it should form such

a large part of the paper when the basis for it is so weak. Page 11, line 10 "this [independent acc rates] aids in reducing uncertainties". How? This is far from self-evident.

Section 6.3. Please show how the newly dated core lines up with other cores in comparison to the old (Pedro) line-up . How does this affect the conclusions of previous papers? This is surely the whole point of making a new age model, to learn something about climate dynamics. I note in passing that the 1000 year shift at 19ka mentioned in the text must be entirely controlled by the dust match there, emphasising how crucial it is to show this in a figure. As an example of climate issues that should be discussed does the onset of TI at LD now look more like WAIS Divide or is it still firmly like the East Antarctic records?

Page 16, line 15. You have misinterpreted something in Parrenin 2007 here. The ratio of LGM to Holocene accumulation at EDC is at least a factor 2 (as easily seen in his Figure 3). The value of 1.4 you cite seems to be the ratio by which he considers previous estimates were wrong. This then requires a rewrite of this section.

Page 17, line 9. It's not obvious how this argument about accumulation related to dust concentrations should work, please spell it out. (I know the argument but I doubt most readers will).

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