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Interactive comment on "An 83 000 year old ice core from Roosevelt Island, Ross Sea, Antarctica" *by* James E. Lee et al.

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Responses to Reviewer 1:
First, Thank you for your comments. We agree with many of the points brought up in the comments and will work to incorporate suggestions of both the technical and major comments. Many of the suggestions were in regard to improving organization and conciseness of the manuscript. This requires rearranging sections and a significant number of edits to sentence structure for clarity. We will undertake these changes, but for brevity, we do not include every planned sentence level edit in our response to reviewer comments at this time.

Major Comments:

1. *There is no doubt that the RICE ice core contains important information on climate and glaciological history in the Ross Sea region. The RICE age scale is essential to decoding this information and in my assessment the authors have done a good job with the age scale and this part clearly merits publication following some revision and restructuring. On the critical side, the sections on glaciological history of the Roosevelt Island and on methane variability are in my assessment much less mature than the age scale work and need to be substantially strengthened or removed all together. The methane findings are described as preliminary in the abstract and Climate of the Past should in my opinion not be publishing work that the authors believe is preliminary.*

   We have reorganized the manuscript to focus on the chronology development and what the chronology may tell us about the glaciology of Roosevelt Island. The introduction has been re-written, we have reduced how many times we describe development of the chronology, and we have removed section 5.1 "New observations of centennial-scale variability in the Holocene methane cycle". More specifics are given in response to other comments below.

2. *A further criticism is that the paper is excessively long, contains a lot of repetition and is not well structured – this makes it very tedious going for the reader. At present the paper reads more like a thesis than a journal article. Serious effort needs to be made by the author team to cut out information that is redundant to the results and conclusions presented.*

   This comment is addressed more specifically in our response to the technical comments, below.

3. *P6L1—4: An example figure is needed showing the straticounter annual layer selection.*

We would prefer not to take up additional space in the paper with this. The depth-age relationship based on annual layer counts is already compared to the gas-based age scale in figure 7. For more information about annual layer selection, we would rather refer to Winstrup et al. (2018) (https://www.clim-past-discuss.net/cp-2017-101/) which describes in detail the data sets used for annual layer selection, the straticounter routine used to interpret count annual layers, and description of the seasonal patterns observed in those data sets. Winstrup et al. (2018) was recently revised and resubmitted so hopefully it will be fully published shortly.

4. *Section 5.1: There is no doubt that the centennial-scale methane variability is an interesting and important observation. However, in my view it should be the subject of a stand-alone paper, in which one would like to see detailed comparison of the various records and labelling of the methane trends that have been attributed to anthropogenic activity. As it stands the two paragraphs do not give a thorough treatment but still take up substantial space. If it must be included then I would suggest to scale back the section, certainly not so much introductory material is needed (it's not until near the final lines of the section that the RICE results are even referred to).*

   The relevant material on centennial-scale methane variability has been removed.

5. *Section 5.2: The first paragraphs appear to describe a thickening of the firn column going in to the LGM (25.3 to 21.8 ka) and an increase in accumulation rate. I find it surprising that accumulation rate would increase through the LGM and this observation merits some discussion. I note that the accumulation rate declines during the ACR as one would expect under cooler conditions.*

   This comment refers to the sentence found on p 13, L27-28 and also in Fig. 8d:

   "After 25.3 ka, accumulation starts to increase and by the first δ^{15}N-N_2 maximum (21.8 ka), accumulation had increased to $\sim$17 cm ice equivalent per year"

   We agree that the early increase in accumulation is an interesting feature. It is the solution that the firn model interprets for periods of increasing δ^{15}N-N_2 values. The alternative explanation for rising δ^{15}N-N_2 is cooling during this time, which is not supported by δD data. The surprise expressed by the reviewer likely stems from the widely held assumption that accumulation is closely linked to site temperature – this is true in a broad sense Frieler et al. (2015), but does not apply to millennial-scale variations at (coastal) sites where synoptic systems deliver much of the snowfall Fudge et al. (2016).

   To address this comment, we have added the following:

   "This feature is not apparent in other ice cores from the Ross Sea region, but those records tend to be difficult to interpret because of chronological uncertainties, such as is the case for Taylor Dome, Baggenstos et al (2017), or because of unexplained jumps in δ^{15}N-N_2, such as is the case for Siple Dome, Severinghaus et al. (2009)."

6. *The reconstruction and discussion of RICE accumulation history depends strongly on the questionable assumption that dD is a faithful recorder of temperature across the deglaciation. The potential for non-thermal effects on the dD record is critical and should be made earlier on in the paper (currently it is not until P14L25—30).*

   How Delta-age is affected by uncertainties in past temperature history is included in our description of the Delta-age sensitivity experiment on page 11. This is the earliest section which describes in detail the firn models used to estimate Delta-age. Although non-thermal effects are not specifically mentioned, a wide range of temperature histories is included in the sensitivity analysis. To be more explicit, some text has been added to p11, l33:

   "The sensitivity tests include a wide range of temperature histories so as to account for the possibility that some variations in δD were caused by non-thermal effects such as variability in precipitation seasonality, moisture sources and pathways, and post-depositional vapor exchange."

Technical comments:

1. *Abstract line 4. Clim. Past should not be in the business of publishing 'preliminary observations'. See major comments on whether these should be presented at all.*

   This line has been removed. See response to Major comment 1, above.

2. *Intro first para: The main focus of the paper is timescale development and the introduction should direct the reader to that subject from the start. Marine ice sheet stability does not come up again in the paper so does not need to be described here. Remove the para and I'd suggest replace with some sentences on importance of timescale development.*

   Thank you for the specific suggestions on how to tighten the manuscript. We have decided to revise the content of the introduction to focus on discussion of difficulties of developing chronologies for ice cores. To accomplish this, we have removed text describing history of the Ross Sea and MISI from the introduction and shifted text from section 4 which summarizes our strategy for chronology development. This reduces the text in section 4 (preceding section 4.1) and eliminates some redundancy.

3. *Intro second para: Here two scenarios are put forward for glaciological history in the Ross Sea region. The later discussion should more clearly refer back to the scenario which is supported by the new results. Since this glaciological history is not the primary focus of the paper I would suggest to move the paragraph to the end of the introduction.*

   This paragraph will be edited and moved to section 2.

4. *P2L35: No need to pers. comm. a co-author.*

   Removed reference to "personal communication".

5. *P3L14: This is the sort of information that is most relevant to the main age scale development task at hand and which belongs in the intro.*

   The manuscript has been rearranged so that the introduction will focus on the chronology development. See response to technical comment 2.

6. *Section 2. Para 2 of the intro could be better fit into this section renamed something like "Roosevelt Island ice core and glaciological history".*

   The manuscript has be rearranged and the description of Roosevelt Island and discussion of the significance its glaciology has been moved to Section 2. See response to technical comment 2.

7. *P4L20: I don't see any points in the RICE methane curve (Fig 3a) sitting 30 ppb above the WAIS curve. The legend does not inform which methane measurements are discrete and which are the problematic CFA.*

   Problematic samples discussed on P4L20 are not highlighted in Fig 3, but can be seen in the figure. We will work to make these measurements clearer by adding the continuous CH_4 records (currently light gray in Fig. 3a) to the figure legend and by making the continuous CH_4 line darker.

8. *P6L20: I don't think pers. comm. of a co-author is needed, remove here and throughout.*

   References of "personal communication" to work performed by coauthors has been removed.

9. *P6L5-15: The method used for each section of the core is repeated in the abstract, in line P3L5-20 and later again in the results. That's far too much repetition*

and testing of the readers patience. Its essential to revise the structure to avoid this repetition.

We will work to eliminate needless repetitions, but because the chronology development is a main contribution of the manuscript we feel that it warrants some inclusion in the abstract, introduction, and conclusion. The reviewer notes that we have also provided an overview of the chronology here (P5L5-15).

We have reduced the text in this section (section 4 “Strategy for developing the chronology”). See edits described in response to technical comment 2.

10. *P6L18: Also repeats earlier material in Intro.*

    See response to technical comment 9.

11. *P6L30: “35% to 75% of the relevant variable”: please clarify what is meant here.*

    We have re-written this sentence to be clearer, as follows:

    Original text:

    “The method starts with a set of prior ACPs which all correspond to well defined variations in either methane or $\delta^{18}O_{atm}$ (Table 1). Age uncertainty of ACPs was estimated from the length of time between 25% and 75% of the observed change of relevant variable.”

    Revised text:

    “Prior ACPs all correspond to well defined increases or decreases of either methane or $\delta^{18}O_{atm}$. The age uncertainty of an ACP is assumed to be related to the duration of the corresponding increase or decrease. For this analysis we assume that the uncertainty (2 standard deviations) for an ACP corresponds to the time elapsed between 25% and 75% of the change in either methane or $\delta^{18}O_{atm}$ (Table 1).”

12. *Fig 5d): Please explain to the reader why there is a large difference between the “best realization”, judged in terms of the goodness of fit, and the number of occurrences of a particular fit.*

    Added text to be inserted before P8,L18:

    “The best age estimate (realization) is not necessarily the same as the most frequent age estimate. Fig. 5d shows an example from sample depth 621.28 m where there is a large difference between these two age estimates. Large differences can occur because the prior age estimate (i.e. the age estimate based only on prior ACPs) differs by a large amount from the “true” age of that sample and because the goodness-of-fit parameter considers the fit over the whole record. In the case of the sample at 621.28 m, most realizations resulted in an age estimate of this sample of 9200 yr BP, similar to its prior age estimate of 9,240 yr BP, but the best realization estimated the age to be 9012 yr BP. There are two possible reasons for this type of result: 1) that this realization is the best because it managed to push the age of this sample by $>$200 years towards younger ages while not significantly changing the ages of nearby sections which already fit well, or 2) that no significant improvement in the goodness-of-fit was found by adjusting the age of this depth, and the goodness-of-fit was dictated by other sections of the record.”

13. *Section 4.1: it would help the reader if this section referred right at the start to Fig 5.*

    Figure 5 is now referred to at the beginning of section 4.1 to show prior ACPs (white triangles in Fig. 5a) and a comparison between RICE and WAIS Divide CH_4 and $\delta^{18}O_{atm}$ on the final age scale (Fig. 5a-b).

14. *P9L3: I think its now the 4th time I read this.*

    We removed this line. See response to technical comment 9.

15. *P9l15—19: As someone who works with these records I find this very hard to follow. Please revise for clarity.*

    Original text (full paragraph quoted for context):

    "Buizert et al. (2015) found that the annual layer counted portion of the GICC05modelext chronology (0-60 ka) (Andersen et al., 2006; Svensson et al., 2008) is systematically younger than ages of corresponding features found in the U/Th absolute dated Hulu speleothem record. A fit to Hulu ages was optimized by scaling the GICC05modelext ages linearly by 1.0063. For the target records we adopt the same scaling as Buizert et al. (2015) for the annual layer counted section of NGRIP, ending at 60 ka in the GICC05modelext chronology and equating to 60.378 ka in WD2014 (and RICE17). Ages older than 60 ka in the GICC05modelext chronology are derived from the ss09sea Dansgaard-Johnsen model (Johnsen et al., 2001; NGRIP Community Members, 2004) which is not susceptible to under counting of annual layers. This portion of GICC05modelext was stitched to the annual layer counts by subtracting a constant 705 years (Wolff et al., 2010). For this section of the target NGRIP records, a constant 378 years is added to the age from GICC05modelext starting at the depth corresponding to 60 ka in the GICC05modelext (60.378 ka in WD2014)."

    Revised text (edits in red):

    "Buizert et al. (2015) found that the annual-layer-counted portion of the GICC05modelext chronology (0-60 ka) (Andersen et al., 2006; Svensson et al., 2008) is systematically younger than ages of corresponding features found in the U/Th absolute dated Hulu speleothem record. A fit to Hulu ages was optimized by scaling the GICC05modelext ages linearly by 1.0063. This suggests that on average 6.3 out of every 1000 annual layers were not counted. For our NGRIP-based target records, we adjust the NGRIP age scale by adopting the scaling of Buizert et al. (2015). Older ages in the GICC05modelext chronology are derived from the ss09sea Dansgaard-Johnsen model (Johnsen et al., 2001; NGRIP Commu-

nity Members, 2004). To make this section continuous with the adjusted annual layer counted section, we have added a constant 378 years (0.0063*60,000) for depths older than 60 ka in the target GICC05modelext ages."

16. *Section 4.3: Shorten it.*

    We will edit this section to shorten it.

17. *P11L7: The delta-age is established using a firn densification model, in which the modelling relies on a RICE temperature history derived from dD. The temperature history is thus integral to the development of the age scale of the ice, however the dD-based temperature reconstruction is cited as a pers. comm. I think the authors need to refer to a published temperature history or include the temperature history here... returning from coffee break... ok reading further down I see there are some more details on the assumptions in the temperature reconstruction and comparison to borehole data. Remove the pers. comm and see major comments.*

    Our estimates of Delta-age, which will me made available with the paper, are dependent on assumptions regarding the temperature history. This record will be made publicly available in a forthcoming community manuscript from the RICE project. As described in the paper we account for uncertainties in Delta-age which result from our assumptions with a Monte-Carlo approach. We include chronological uncertainties, uncertainties in the assumptions in deriving a temperature history, uncertainty in constraints due to measurement error, and uncertainties from non-temperature related effects within the δD record.

    Reference to "personal communication" will be removed.

18. *P12L24: Good. Agreed.*

19. *Section 5.1: There is no doubt that this discussion of methane variability is interesting. In my view it should be the subject of a stand-alone paper, in which one*

would like to see detailed comparison of the various records and labelling of the methane trends that have been attributed to anthropogenic activity. As it stands the two paragraphs do not give a thorough treatment but still take up substantial space. If it must be included then I would suggest to scale back the section, certainly not so much introductory material is needed (it's not until near the final lines of the section that the RICE results are even referred to).

This section will be removed. See response to major comment 4.

20. *Fig 4d. Adjust the y limits so we can more easily see the age uncertainty.*

    The axes in Figure 4d have been adjusted.

21. *P13L33: Include the uncertainty in the onset of the d15N change at 14.71 ka; I'm far from convinced that it significantly precedes onset of Bølling at 14.64 ± .19 ka.*

    The age of the depletion in δ^{15}N-N_2 unambiguously precedes the onset of the Bølling, which is defined by an abrupt increase in CH_4, because both events are recorded in gas-phase measurements and the change in δ^{15}N-N_2 is observed at a deeper depth than the change in CH_4. We have edited the text from:

    "Curiously, this abrupt decrease in δ^{15}N-N_2 precedes the increase in CH_4 marking the onset of the Bølling-Allerød."

    To:

    "Curiously, this abrupt decrease in δ^{15}N-N_2 is observed in samples deeper than the increase in CH_4 marking the onset of the Bølling-Allerød meaning that this climate event unambiguously precedes the Northern Hemisphere event."

    I have attached a figure which shows the δ^{15}N-N_2 and CH_4 records during the deglaciation plotted versus depth.

22. *P14L11: This interesting sentence suffers from being way too long.*

    See response to technical comment 23. We have shortened it.

23. *P14L19–40: It would be more logical and much easier for the reader to follow your arguments if you set out the preferred explanation first and then explain, briefly, why some potential alternative explanations are unlikely. I don't find the preferred explanation very convincing: I don't see any quantitative data to support it, only some arm waving analogy to recent periods.*

    Changes to P14 L5-30 were made in accordance with the reviewer's suggestion on how to organize the discussion of possible interpretations.

24. *Section 5.3: It would help the reader to refer early in the section to the "maximum" and "fast and thin" Denton (1989) scenarios that were set up in the introduction.*

    We will make this change.

25. *P15L4: Again refer to the scenario set up in the introduction, here and elsewhere in this section.*

    We will make this change.

26. *P15L18: Refer to the dD record in Fig3b. Not to a pers comm!*

    Reference to "personal communication" will be removed.

    We would like to note that δD is not shown in Fig 3b. What is shown it the ^{18}O/^{16}O ratio for O_2.

27. *P15L26: The comment about an MBL ice dome comes out of the blue and its far from obvious who it provides an alternative explanation for the continuity of the record. Clarify or drop.*

    We will change text to further incorporate the idea of a MBL ice dome. This was a hypothesis from several previous publications to explain geomorphological features in the eastern Ross Sea.

Original Text (full paragraph provided for context):

"Geomorphological features on the Ross Sea bed do provide evidence of an expansive ice sheet which extended past Roosevelt Island during the LGM (Shipp et al., 1999; Anderson et al., 1984, 1992, 2014; Halberstadt et al., 2016). The stability of the Roosevelt Island ice dome and of Siple Dome implies that at this time, WAIS flowed around these sites rather than over them. This observation implies that as WAIS grew spatially, its thickness in the Ross Sea was limited, conditions that indicate ice streams were active throughout the last glacial period. Alternatively, Price et al. (2007) proposed that the geomorphological features observed in the eastern Ross Sea may represent building of an ice dome in Marie Byrd Land. The RICE records can not distinguish between these scenarios."

Revised Text:

"Geomorphological features on the Ross Sea bed do provide evidence of grounded ice north of Roosevelt Island during the LGM (Shipp et al., 1999; Anderson et al., 1984, 1992, 2014; Halberstadt et al., 2016). If these features were formed by an extended WAIS, it would imply that ice flowed around Roosevelt Island and Siple Dome and therefore must have been limited in its thickness. These conditions would indicate ice streams were active throughout the last glacial period. Alternatively, these geomorphic features may be the result of ice from a different origin. Price et al. (2007) proposed that during the LGM, an ice dome may have existed on Mary Byrd Land. In this scenario, thick, grounded ice could exist north of Roosevelt Island without flowing over or around the Roosevelt Island sea rise. The RICE records can not distinguish between these scenarios."

28. *Conclusions para 1: The fifth time we read this?*

    We removed this. See response to Technical comment 9.

29. *Many references found in the introduction do not come up again in the discussion. I'd suggest a bit more focus and continuity between the most relevant literature*

flowing from the intro to the discussion.

We agree with this comment and will reduce references which are only used in introduction.

30. *As a final point, it is tedious as a reviewer to have to spend so much time commenting on structure, something the author team could have worked on internally prior to submission. The age scale is important and should be presented as accessibly as possible.*

References

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