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Climate of the Past
Discussions

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 2:
First, Thank you for reviewing our manuscript. We agree with many of the suggestions made by reviewer 2. A number of these were in regard to improving organization and conciseness of the manuscript. This requires rearranging sections and a significant number of edits at the sentence level. For brevity, we do not include every edit in our response to reviewer comments.

General Comments:

1. *This manuscript presents a suite of new gas records from an ice core drilled at Roosevelt Island, an ice rise in the Ross Sea. The primary objective is to*

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establish its chronology by annual layer counting for relatively shallow depths and matching of gas records with existing WAIS Divide and Greenland ice core chronologies. The continuous part of the ice core extends to 65 kyr BP, suggesting that the Roosevelt Island has existed since at least this age. CH4 records show centennial-scale variability throughout the Holocene, with implications on natural vs. anthropogenic CH4 emission in pre-industrial periods. These discussions have some important implications for past climate and ice sheet variations. The dating method developed here is a nice contribution to the ice core community.

However, the lack of water isotope records and interpreted temperature records in this manuscript makes it difficult to review the estimated annual layer thickness using a firn densification model and its effects on dating and paleoclimatic implications. I find this study is potentially an important contribution to paleoclimatic communities but do not recommend publication in its current form. The authors would need to decide if they remove some parts of the manuscript regarding annual layer thickness estimates from firn modeling (but it will make the manuscript much less attractive), or they add water isotope data and temperature estimate (I would recommend the latter for publication in CP).

The depth-age relationship, from which annual layer thickness is derived, is primarily dependent on the gas-based age constraints with only a small correction arising from the climate-dependent Δ-age. In this approach, temperature has only a secondary effect on annual layer thicknesses. One exception to this statement may be during the deglaciation when large changes in Δ-age are implied by rapid changes of δ^{15}N-N$_2$.

We estimate past temperature based on the measurements of δD. The full high-resolution record of δD will be made publicly available in a forthcoming RICE project community paper led by project PI Nancy Bertler.

2. *The discussion of anthropogenic and natural CH4 variability needs some quanti-*

tative analyses (for example comparing frequency and variability after detrending for different time periods). To my eyes, the CH4 records appear to have different centennial-scale variations in earlier and later parts of the Holocene.

We have decided to remove this section (5.1 New observations of centennial-scale variability in the Holocene methane cycle) from the manuscript in response comments from reviewer 1. Content of this section is not discussed elsewhere in the paper and we hope that its removal will focus the paper on the other chronology-based conclusions and on the chronology development.

Specific comments:

1. *P5, L5. Regardless of the careful trimming of the ice in the same shape, the cut-bubble effect should change (generally decreasing) with depth due to the change in bubble sizes. The cut-bubble effect thus needs to be corrected.*

   We agree that the effect described by the reviewer should exist, but as we mentioned in the original text we have not made this correction to our total air content measurements because we do not believe that an accurate correction can be calculated for the RICE samples. This is because many of the RICE samples were fractured. Air intersecting a fracture may escape under vacuum and it is difficult to calculate the surface area of fractures. The air lost through fractures may be significantly greater than that lost due to sample preparation. To avoid samples with obviously large cut-bubble effects we excluded samples based on visual observations: samples with large fractures, with many fractures, which were comprised of multiple pieces of ice, or were an odd shape.

   However, we chose to include samples with small cracks in our data set. Small fractures are inconsistent in allowing air to escape. For this reason, it may not be possible to separate variations in TAC from gas loss through small fractures. In practice, small fractures can be hard to see which makes it possible for air to be lost through a fracture which was not observed.

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To clarify this issue we propose the following change in section 3.2 (P5, L3-9).

Original text:

"Air trapped in bubbles, clathrates, or fractures intersecting the surface of the sample is lost, an effect called the cut-bubble effect (Martinerie et al., 1990). The cut-bubble effect is difficult to quantify, especially in ice which contains fractures through which air may be lost. No correction for the cut-bubble effect was applied to the TAC measurements presented here. Samples were cut to uniform shapes whenever possible to ensure that the cut-bubble effect was relatively constant in order to limit the influence it has on the variability of the TAC record. TAC analysis was rejected when the cut-bubble effect was believed to greatly impact the results, such as in samples which fractures could not be excluded or were excluded by cutting the sample into irregular shapes or into multiple pieces."

New Text:

"The cut-bubble effect is difficult to quantify, especially in ice which contains fractures through which air may be lost. Samples were cut to uniform shapes whenever possible to ensure that the cut-bubble effect was relatively constant in order to limit the influence it has on the variability of the TAC record. However, many samples contained fractures through which air may be lost and greatly impact TAC. TAC data were rejected when gas loss was believed to greatly impact the results, such as in samples with fractures or samples which consisted of multiple pieces. However, small fractures were difficult to see and their contribution to gas loss is unknown. For this reason, we choose to not correct TAC measurements for the cut-bubble effect."

2. *P16, L28. I do not understand why the temperature stability of the sample leads to the improvement in S/N of the gas chromatograph.*

Insulation was added to the system as an attempt to minimize the water vapor in the headspace of our sample flasks (by decreasing the head space temperature) and to minimize variations of water vapor throughout the day. We also have made efforts to regulate the amount of ethanol in the chilled bath for the same purpose.

We adjusted the sentence to clarify our intent and what we did.

Original text (P16, L26-28):

"Since Mitchell et al. (2013), insulation has been added around the ethanol bath and above where the flasks are mounted. The added insulation decreased the temperature variability of the ethanol bath and of the sample flasks throughout the day allowing for better measurement of pressure and improved signal-to-noise for the chromatograph."

Edited text:

"Since Mitchell et al. (2013), insulation has been added around the ethanol bath and above where the flasks are mounted. The added insulation reduced the temperature and water vapor content of gas in the headspace of the flasks and decreased variability throughout the day. Both can affect methane measurements by changing the pressure reading or the retention time of methane in the GC column. Additionally, we have made efforts to more carefully regulate the amount of ethanol in the chilled bath and the temperature of hot water bath during melting. These steps improved stability of measurements and extraction between days."

3. *P17, L30. Please explain why the solubility correction factors are so different for sample and bubble-free ice?*

The solubility corrections are empirically derived, so the difference in solubility between glacial sample ice and bubble-free ice is something we observe. We will add additional explanation to the supplement describing our theory about why our solubility results for bubble-free ice and glacial ice are different.

We believe the difference results from the differences between how blank ice and ice containing air behave during melting.

- Bubble-free ice melts slowly in comparison to glacial ice which sometimes melts rapidly and cracks violently. This, along with bubbles rising and breaching the meltwater surface, cause disturbances in the water-air interface and promotes exchange of CH_4 into the meltwater. This should lead to greater mixing and homogenization of air and water.
- Bubbles released into the meltwater will be at higher pressures than the overlaying air because of surface tension. The higher partial pressure of CH_4 in those bubbles, in comparison to the standard gas added over the bubble-free ice, will cause air to go in to solution faster.
- Because glacial ice tends to be melted sooner than blank ice, a longer time period for liquid-gas exchange is available.

4. *Fig. 2c and i. The scales of the axes should be the same for the left and right panels.*

   Done. Thank you for pointing this out.

5. *Fig. 5d. Why is the vertical line drawn at about 9000 yr BP and not near 9200 yr BP (highest occurrences)?*

   There is a difference between what we considered the "best" chronology and the most frequently occurring age of a specific depth. If we were to accept the most frequently occurring age of each sample depth in our Monte Carlo analysis as our final chronology, there is no guarantee that the age of ice increases with depth. Instead, we chose the age-scale with the best "goodness-of-fit." In this routine, goodness-of-fit is a single statistical value describing how similar both the CH_4 and $\delta^{18}O_{atm}$ records look like their corresponding records from WAIS Divide.

   Added text to be inserted before P8,L18:

   "The best age estimate of a sample depth (single point on the depth-age scale) is not necessarily the same as the most frequent age estimate for that depth.

Fig. 5d shows in example from sample depth 621.28 m where there is a large difference between these two age estimates. In the case of sample depth 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. The difference in estimates could be random because no significant improvement in the goodness-of-fit was found by adjusting the age of this depth or because shifting this age tended to worsen the fit of adjacent depths."

6. *Supplementary file "RICE17_Interpolated_Ages_20180530.txt" appears to contain two units for the ice age (probably C.E. and yr BP are switched at 343.5 m).*

   Corrected.