

Review of 'An 83,000 year old ice core from Roosevelt Island, Ross Sea, Antarctica by Lee et al.

The paper by Lee et al. presents an age scale for the Roosevelt Island Climate Evolution project (RICE) ice core. Roosevelt Island is located in the Ross Sea, a primary outlet region for the West Antarctic Ice Sheet (WAIS).

Most of the paper is devoted to deriving the RICE age scale (RICE17), including novel use of centennial-scale variations in methane to synchronise the record with other ice cores. Following presentation of the age scale, the paper draws some conclusions on the glaciological history of the Roosevelt Island Ice Dome. Specifically, the authors argue that the WAIS did not override Roosevelt Island at any time during the past 65kyr (i.e. the island remained independent of an advanced WAIS during the glacial). The authors also comment on Holocene variability in atmospheric methane. Specifically, that centennial-scale variations in methane are present throughout the Holocene, casting doubt on previous work suggesting that similar centennial-scale variations in methane during that late Holocene have an anthropogenic origin.

I think the article should eventually be suitable for publication in *Climate of the Past*. In general the age scale work is good but the broader conclusions appear less mature. However some substantial work is needed to address the following points.

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.
- 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.
- 3) P6L1—4: An example figure is needed showing the stratigraphic annual layer selection.
- 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).

- 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.
- 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).

#### 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.
- 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.
- 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.
- 4) P2L35: No need to pers. comm. a co-author.
- 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.
- 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'.
- 7) P4L20: I don't see any points in the RICE methane curve (Fig 3a) sitting 30ppb above the WAIS curve. The legend does not inform which methane measurements are discrete and which are the problematic CFA.
- 8) P6L20: I don't think pers. comm. of a co-author is needed, remove here and throughout.
- 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.
- 10) P6L18: Also repeats earlier material in Intro.
  - 11) P6L30: '35% to 75% of the.. relevant variable": please clarify what is meant here.
  - 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.
  - 13) Section 4.1: it would help the reader if this section referred right at the start to Fig 5.
  - 14) P9L3: I think its now the 4<sup>th</sup> time I read this.
  - 15) P9L15—19: As someone who works with these records I find this very hard to follow. Please revise for clarity.
  - 16) Section 4.3: 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.
  - 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).
  - 20) Fig 4d. Adjust the y limits so we can more easily see the age uncertainty.
  - 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.
  - 22) P14L11: This interesting sentence suffers from being way too long.
  - 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.

- 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.
- 25) P15L4: Again refer to the scenario set up in the introduction, here and elsewhere in this section.
- 26) P15L18: Refer to the dD record in Fig3b. Not to a pers comm!
- 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.
- 28) Conclusions para 1: The fifth time we read this?
- 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.
- 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.