Reviewer 1

Review in normal type, response in *italic*:

Mulvaney et al. present a timescale for the Skytrain Ice Rise ice core which covers all ice older than 2 ka. The timescale to ~106 ka is continuous and based on methane matches using the continuous methane record supplemented by discreet measurements. They then identify an area of ice flow disturbance. Using discrete measurements of d18O of O2 and methane from the same depths, they confirm a flow disturbance, date a section below the disturbance to ~106-126ka and suggest a continuous climate record during this interval. Below this, the ice is again disturbed and likely from the penultimate glacial period. Interestingly, both the onset of the Last Interglacial and the Penultimate Glacial Maximum are missing, which the authors suggest is due to flow disturbances caused by contrasts in ice fabric.

The paper is important, well written and should be published with minor revisions. This paper will be foundational for what I image are future high impact papers on the climate and ice sheet interpretation for which a timescale is necessary. The authors have made a wide range of measurements and performed a thorough analysis of the core. The timescale is well developed with a lot of care taken to explain the approach. I have some recommendations for making this even clearer, but I appreciate the effort the authors have taken. I have only two area to suggest significant changes. The first is the timescale from about about 75 ka to 109 ka. The second is the uncertainty of the timescale.

I also want to specific note that I appreciate that the authors have made the data publicly available.

We thank the reviewer for this generous summary.

First, one initial thought on the introduction. The authors lead the introduction with a statement about the "intense interest" in the stability of WAIS and the need for paleoclimate records to constrain potential ice sheet changes. Later they write "old ice might be available … Berkner Island and Fletcher Promonotory, but there is no published age scale for these cores so far". The lead author of this paper was the project leader for both of those ice cores. I hope that this work on Skytrain inspires those cores to be revisited and published.

Yes, indeed. The lead author's colleagues have made the same point to him!

Discussion of timescale from ~75ka to 109ka, particularly 95ka to 109 ka

The timescale for this period seems more uncertain than the authors imply and could use more description. I realize much of the emphasis of this paper is on the LIG, but I think it is important to discuss where the end of the continuous climate record is reached.

The methane matches are not particularly robust given how different the shapes are, like the wider width of the peak centered on ~82 ka. The subpeak at 80 ka is also distinctly different. The nssMg does seem to help in this period.

The nssMg match is less robust in the period 95 ka to 109ka. At 95 ka, the methane rise at Skytrain looks substantially larger than the EPICA composite. Since there are folds in this area, I think more explanation should be given about why the methane features at ~95 ka and ~106ka are not a repeat of younger events.

I think a new section which discusses this interval would be help. With more description, I might agree that the timescale is continuous to 109 ka.

The reviewer is correct to point out that the methane matches in the region 80-100 ka are not as robust as those in shallower ice. This arises mainly from the poor quality of the methane record because the ice was so cracked, meaning that occasional undetected air ingress was unavoidable, and that data are missing over some of the detailed methane features.

Taking the specific areas highlighted by the reviewer: the width of the peak at 82 ka arises from our decision to align the records on the lowest points at ~77 ka and 87 ka, with only one alignment point in between. We acknowledge that we placed too small an uncertainty on the depths of these alignments. We have tested a new age model experiment where we have raised the uncertainties of these 3 alignments to 1000 years, and (see response to next section) raised the uncertainty on the thinning function. This pulls in the width of the peak on the old side and makes the ST data consistent with the reference data within our new (greater) uncertainties.

The subpeak at 80 ka is hard to diagnose, because it consists of about 40 cm of ice, with 73 cm of missing data just above it. It's therefore difficult to decide whether it belongs where you see it and represents a too high concentration due likely to air entering cracks on the melter, or whether it belongs to the left where it would match the reference subpeak at 81 ka but have a rather larger dip than expected. Again by increasing the uncertainty we can accommodate either scenario.

We have implicitly assumed that the spikes at 90 ka that take the ST record higher than the reference are undetected fliers that should be ignored (they represent only a few cm of the record).

We recognise that the match in this section using methane alone is not as convincing as we would wish. However we would argue that the good match of nssMg with reference nssCa (in both values and age) between especially 75-90 ka support our alignment – remember that we imposed no ice tie points in this region of the core, so this is completely independent support.

For the section to 100 ka, we will add some text to section 6 to acknowledge the nature of our match to 100 ka. Our proposed additional text is: "The match between Skytrain and reference methane between 80 and 100 ka is less secure than it is in shallower ice, because ice with high concentration outliers and/or missing data is common as a result of extensive cracking. This makes it hard to match absolute values of methane, and forces us to rely on the pattern with depth. Nonetheless, the methane ties we have made result in a good match in this part of the core between nssMg and reference nssCa (Fig. 7 and section 6.3), supporting our choices."

Between 95 and 109 ka, we agree that we are relying almost entirely on methane, although the low concentrations of nssMg rule out most other sections of glacial ice. We do have the single del18O_air/discrete methane pair at 106 ka, which has values that don't occur again after 106 ka until 57 ka. Proposed additional text: "We have no reason to doubt that the ice is in good order until 605 m, but we acknowledge that the section we date as 95-107 ka (Figs. 5, 7) relies on the pattern of methane and on a single CH_4/ $\delta^{18}O_{atm}$ datapoint. This point, dated at 106 ka, firmly defines the lower end of this section, with values that do not occur again as a pair until 57 ka."

In summary, in a revised version we have increased the uncertainties in our tie points and as a result we have larger uncertainties in the final age model, which can now accommodate the comparison seen in Fig. 5. We will also add some sentences acknowledging the difficulties in this section in more detail.

Uncertainty

The uncertainty is not discussed substantially in the manuscript. The uncertainties are given as part of the paleochrono output in the supplement, but I think these are likely too low. They assume a continuous climate record with well behaved ice-dynamics and confident tie points. I think this is likely only justified to about 75 ka. Given the meter to decameter scale folding, there is almost certainly smaller scale folding as well. Thus, the assumption of continuously increasing age even in sections that are primarily intact is not a given (hopefully the stratigraphy is being looked at for a future paper because it could be fascinating). And the confidence in the tie points seems to drop. The 82 ka methane peak is a good example. The uncertainty is given as <300 years but it looks like there is a 1000 year offset between the midpoints of the rise. I suggest two things:

- the uncertainty be manually adjusted to be greater for the older portions of the core. This can be a qualitative uncertainty. It would serve as a warning to users in a way that text in the manuscript would not because it would travel with the timescale data file
- Add a section and a figure which specifically discusses the uncertainty and how it changes through the core.

The reviewer is correct that the uncertainties in the output age model are too low. As discussed we have raised the uncertainty on some of the tie points. However the key issue is what happens between tie points where the age-depth relationship may be highly non-linear. To allow for this we have, in our new age model, raised the uncertainty on the thinning function. This of course cannot allow for the possibility of age reversals. However we have enough clear tie points in the section 117-126 ka to rule out any large scale reversals in this section. The change that will be made in response to this comment is to increase the uncertainties in tie points and thinning model, leading to little change in the age model itself but to larger and more realistic uncertainties that will be shown in the age model file. We also plan to add, in Fig. 13, a panel showing the age uncertainty.

Suggestions for the presentation of the LIG timescale:

The figures and discussion of the timescale older than 106 ka can be revised to make it even easier to follow. Many of these figures should be integrated with subpanels because trying to find and compare the figures was challenging. At one point, I had a printed version open to three different pages and two electronic versions open on my screen. Here's a couple of suggestions:

- Provide a plot of the methane and d18O measurements below 600m by depth. The methane variations are not visible in Figure 3 because it spans the full core. When the methane is already matched and put on the timescale (e.g. Figure 5), the record can be deceiving.

- The methane-d18O cross plots should be combined into fewer figures. I suggest combining Figures 8 and 9 so that is much easier to compare the reference records with the cross plot. The cross-plot should also be extended to start at ~60 ka, such that the two intervals at ~80ka and ~100 ka, which have similar d18O-O2 and ch4 values similar to the LIG, are shown. Figures 10 and 11 should be combined with subpanels and have the same length of time shown – i.e. both run from 100 to 130 ka so that the colors remain the same in the two cross plots.

We apologise if we made the relationship of the figures to the text too complicated. We have tested a number of ways of combining panels to see what might work best. We did also test a number of options for the time period covered in each cross-plot. If the age range is too large it becomes impossible to discern the age across the interglacial, so we prefer to keep it narrow but provide the context of the longer timescale as shown in what was Fig.9. Eventually what we have decided to do is as follows:
a) We have combined Figs 8 and 9 into one figure as they provide different visualisations of the same data (this was proposed by Rev. 2).
b) We have extended what was Fig. 9 from 0-200 ka to give the full context, and we have also plotted onto it the points we assign with definite LIG ages, so that the reader can see how unusual the values are in the long context.
c) We have also added a version of this figure with a different colour palette in the supplementary data. This figure is particularly hard to interpret because there is such a lot of data on it, so we believe that having the full range of colours used in the main text is justified but we acknowledge that the alternative may be easier to interpret for some readers (including those with colour vision issues).
d) We have also combined what was Figs. 10 and 11, using the same time range (100-130 ka) for both. We kept what was Fig. 12 separate because this necessarily must show a longer time range and so there is no advantage to combining it.
e)As requested we have added a new Supplementary Figure 1 which shows methane and 18O_atm data on a depth scale below 600 m.

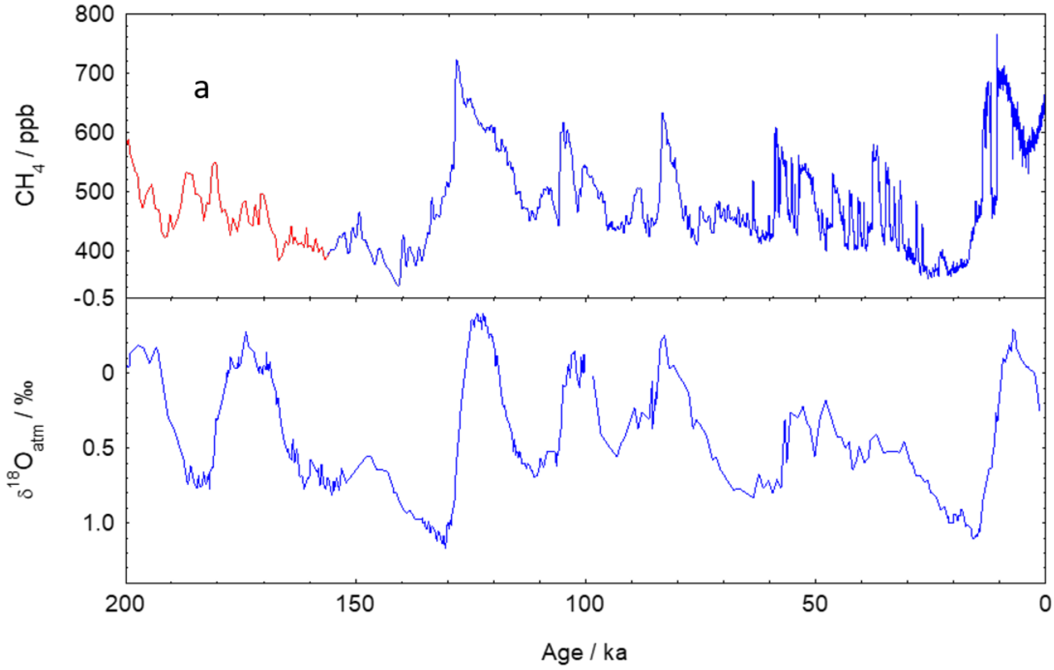
We hope these changes do meet the request of the reviewer in a satisfying way. We have pasted the two new combined figures below.

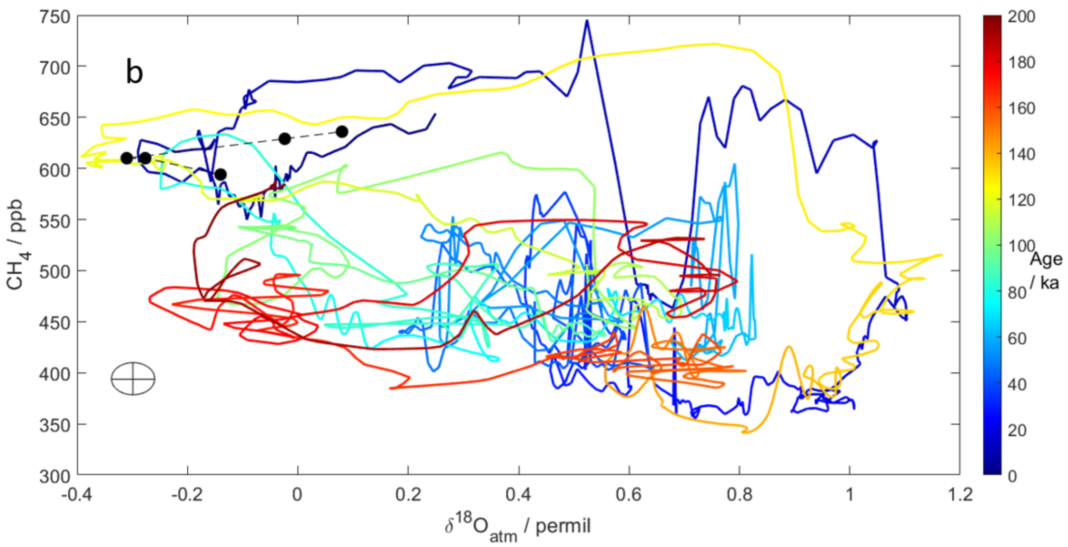
Other comments:

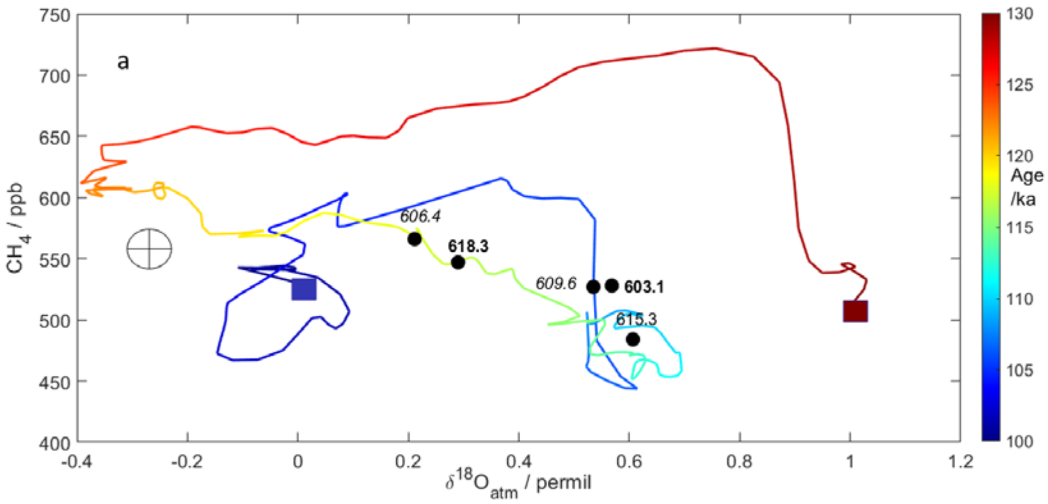
L503 – Can you rule out ice from the Ellsworth mountains flowing to Skytrain ice rise at a time when Skytrain was lower in elevation (and not an ice rise)? Are the elevations Ellsworths and its glaciers not sufficient to flow onto the bedrock beneath Skytrain? Or are the water isotope values in the Ellsworth mountains too cold? You have demonstrated that there is a stratigraphic disturbance, but I think stating that the "only plausible" explanation is due to contrasting rheology is too strong – particularly since there are no ice fabric measurements presented in the manuscript.

Our main point was that it seems impossible that ice from inland WAIS could have come over the Ellsworths (with peaks of around 2000 m asl behind Skytrain) to reach Skytrain. As the reviewer hints, if it did it would have had to originate at very high altitude, with highly negative water isotope ratios which we do not observe. As we stated the bedrock of Skytrain is a high point a few hundred metres above sea level surrounded by a very deep bathymetry (1000 m in the two inlets and several hundred metres in the inland section in between). This makes it impossible for valley glaciers originating in the Ellsworth Mountains to reach *across to Skytrain. However we have not carried out ice sheet modelling to prove these assertions, and agree that “only plausible” is too strong a wording and we propose changing it to “most plausible”. We also propose to add a sentence “However, detailed ice sheet modelling, as well as rheological studies on the Skytrain ice core, are required to firmly rule out other causes.”*

Revised figures are shown below (captions and figure numbering will of course be changed to match):







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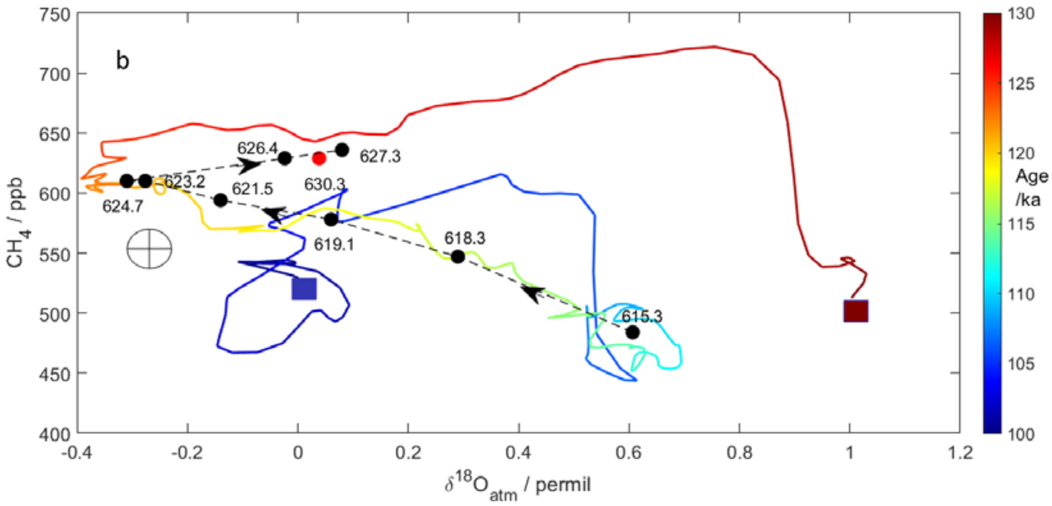

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