Review of Neuhaus et al – Did Holocene climate changes drive West Antarctic grounding line retreat and re-advance?

Firstly, my apologies that this review is right up against the deadline that I was given – I had some unexpected personal circumstances.

Secondly, for contextualising some of my several comments on figures it is worth noting that I have a mild but common colour deficiency. It might be helpful for the authors to know that as a general rule when small colour symbols (or thin lines) are placed on top of other colours it can be extremely difficult to distinguish the colours and match them to a key. There is now wide debate on this in scicomms literature and some good reviews available to guide authors e.g. https://www.climate-lab-book.ac.uk/2014/end-of-the-rainbow/ – most of R, GMT, MATLAB etc now have colour-vision-friendly palettes available.

This paper aims to address the question of timing of a grounding line retreat and re-advance in the Ross Sea sector. This has been part of a debate for some time on retreat history of the region but this paper particularly follows on from the work of Kingslake et al (2018) which attempted to constrain retreat timing but which yielded dates for retreat that were inconsistent with significant amounts of other observations.

In this paper, the authors take three main approaches to constraining past timing of retreat and readvance: and in each case comparing model simulations to archived (and some new) measurements from subglacial and sub-ice shelf sample sites. First, they use temperature modelling of the basal ice to constrain the timing of grounding line readvance. Secondly they use iconic diffusion modelling of the topmost portion of subglacial sediment to constrain timing of readvance. And thirdly they use modelling of radiocarbon content of subglacial sediment to constrain the period of ocean exposure between grounding line retreat and readvance. The authors then go on to draw some conclusions on the likey forcing factors and conclude that the drivers for retreat and readvance were climatic rather than delayed ice dynamics and glacioisostatic adjustment, as suggested by Kingslake et al.

The paper is based on an interesting idea which has potential to provide insight into the grounding line history, and I very much like the use of archived samples and long-standing measurements. The results will be potentially of interest to a broad community, but they need further explanation and some additional clarity (especially on uncertainty) before the paper should be accepted for publication.

Broad issues

Model details. The derivation of model equations is mostly well dealt with but some other details such as numbers of runs, uncertainty (see below), and how to interpret some of the output (especially in some very rich figures), are not yet clear enough.

Model assumptions – in a number of places it is difficult to follow if assumptions/choices are made for model simplicity or are based on a comprehensive observational dataset. For example, a single porosity value of 0.4 (line 126) seems surprising: marine sediments vary significantly in porosity and I imagine that subglacial sediments do too. Similarly the supplementary gives a single value for geothermal heat flux but without discussing any likely range or sensitivity of the model to this parametrisation. Whilst the choice of parameters may be helpful it is difficult to assess the model results without knowing the uncertainty bounds created by uncertainty in parametrisation. Some sort of sensitivity or uncertainty analysis of the model is needed to try and understand the range in final exposure/readvance durations. For example, the plots in Fig 6, which I read as a form of

probability density plot translate into single thin lines on figure 7 but without uncertainty bounds included.

Unmodelled processes: What would be the effect of sediment accretion and/or deformation following grounding line readvance? Given this is the 'type area' for actively deforming till layers it probably needs some qualitative comment on the likely effects of such deformation on the radiocarbon and/or ionic diffusion observations. If it is only simple shear then there would be no vertical movement but till accretion is possible and this would likely lead to addition of deep interior radiocarbon-dead carbon. I think some qualitative discussion on possible post-depositional changes would be important here, as the implicit assumption is that the sediment being sampled was the same sediment exposed as the cavity was closed.

C:N rationale and treatment: The explanation of why C:N is being measured and analysed does not become clear until the Results in Section 3.3, when Figure 9 is called (out of sequence) and the different fields for marine and terrestrial become clear. I suggest putting Fig 9 much earlier and calling it from the methods where it will be possible to explain the concepts behind measuring C:N ratios and del-13C. This explanation can also then make clear the reasoning as to why terrestrial plants field matter (I presume this is because the radiocarbon-dead material is assumed to come from long-dead terrestrial plant material in bedrock beneath the WAIS but I couldn't see it made explicit anywhere). Note also comments below on use of weight:weight and atom:atom ratios on the same plot.

Presentation of (radio)carbon data on sediments: It would be helpful to include analytical error bars plus site means and standard deviations on this plot. There is a lot of discussion of 'average' values and ranges and so the descriptive statistics should be presented.

Paragraph 307-318 – I've misunderstood something here: I couldn't follow the description of the dataset in this paragraph against Fig 2, which is the only figure cited. It seems to suggest that Fm values at the different sites are variously similar or different but in ways I couldn't see. For example, Fm values at KIS and BIS are supposed to be similar but those at WIS and SLW different from the former. I disagree – there is only one value at BIS and that is similar to 2 of the three values at KIS but very different to the third value, and the range at KIS looks comparable to ranges at WIS and SLW, albeit with a very small n. If this is actually referring to model output, where is it illustrated? Apologies if I have missed something here.

I think the same section uses 'statistically independent' (which has a very specific meaning) when 'statistically distinguishable' might be what is actually meant.

Comparison to climatic forcings: Without seeing the uncertainty bounds of the results it is difficult to assess the robustness of the conclusions. To be clear I think the paper raises some really interesting questions about the driving factors for grounding line retreat but without a greater assessment of model output sensitivity and uncertainty on durations/dates it is difficult to comment firmly on the conclusions. Whilst I understand why the authors are drawing out differences to Kingslake et al I think it is important to note that Kinglake et al used an ice sheet model to explain forcing mechanisms and they found that they could not initiate readvance without including some sort of GIA processes or buttressing from ice rise formation. Their ice sheet model was forced by a similar temperature record to the one described in Fig 7 and so a simple comparison of timing of events probably needs to be tempered by a discussion of whether temperature changes would be sufficient to initiate readvance. I also note the use of Hall et al (2006) sea ice record as a proxy for ocean warming – there may be better records of Holocene ocean temperatures such as Cunningham et al (1999) (The Holocene) which are based on oceanic proxies alone (Hall et al note that their record may reflect both oceanic and atmospheric forcing).

Some line-by-line issues:

44 – this hasn't been a consensus for quite a while, starting probably with Bradley et al 2015 but also including Matsuoka et al 2015 (Earth Science Reviews) which discussed smaller-than-present configurations and the GIA and climatic mechanisms that would explain Crary ice rise etc.
58 – the comparison to Greenland is not as useful as it might be – the main part of the GrIS that retreated and readvanced in the Holocene is in the SW where it is mostly a terrestrial margin, and so unlike the WAIS ocean and GIA forcing would not be possible.
92 – "chose the freezing point of freshwater" (insertion of 'fresh' reduces confusion as to whether you were just sticking with seawater (line 83) after Phase 1). Also, is there an argument to look at a 3-phase model where the heat flux sets the basal temperature in a subsequent Phase 3 (a la Bindschadler) after a period of basal freeze-on
98 – 'retreated' rather than 'advanced' otherwise I don't think this makes sense
148 – not equation 7
240-244 – I'm not quite sure what this is describing – can you explain the inference that comes from the grounding line constraint of 8000 and why the preceding 4000 years is used.
242 – should this read 'earliest' rather than 'latest'?
293- add reference 14/12 value as a line to figure 2a.
297 – add ratio as a line to Fig 2c
298 – Fig 9 – called out of sequence (see comments above about reordering this figure)
308 – I don't see eight sample points – I see 8+3
325-327 – use of 'model' to refer to (I think) 2 or 3 different models. Would really help to specifically refer to radiocarbon, temperature or ionic models – I lost track of the logical thread here.
328 – do 'model matches' on axis of Fig 7 mean the same as 'positive models' here
323-324 – refer to Figs 7 and 8 together
404 – what is ultimate origin of terrestrial plants?
408 – the values of C:N at UC are lower than at WGZ
415 – Bradley et al 2015 give a series of observations that support retreat and readvance including observation of (unstable) readvance of grounding lines on reverse slopes, amongst others.

Figures and Tables

Figure 1
I can't see a cyan diamond. Would be helpful to define abbrevations in the caption to save constantly referring back to the text.

Figure 2
add reference lines (see above)
Caption suggests C(org):N(org) but I think it should be C(org):N(total).
Caption – '…matter from…..'
Caption suggests the C:N ratios of data in the paper are plotted as atom:atom but looking at the fields used from Lamb et al (2006) I believe they were plotted in the original paper as weight ratios. So the weight:weight column in Table S1 needs to be plotted, not atom:atom. I think this will lead to a shift of *1/1.17 for all your data points. All text, including caption, plus results and conclusions will need to be checked to see if the correction changes anything in text.

Figure 3
Labels for T(o) and T(i) should be centred over their durations on the diagram, not placed at their end points otherwise there is confusion that they are dates not durations.

Figure 4.
Thin colour lines not clearly discernible on top of background colour palette for temperature gradient

Not clear why there are 7 lines for 3 sites – I thought it was perhaps for different cores ? If so, please note this in caption (I couldn't find how many cores there were at UC).
It would be very helpful to give the observed values in a table somewhere – otherwise it relies on ability to read off a contour from these colour plots. This would also allow the uncertainties in the observed gradients to be provided.

Figure 5.
y-axis caption – not clear. Number of what ? (runs ?) Overlapping with what ? (presumably within 2-sigma 'match' to observations ?) Is this number or a percentage ? (looking at the next figures you use percentages)

Figure 6.
These are quite rich diagrams and I can see the potential but it would be very helpful to include a caption that walks the reader through interpretation. E.g. "The key shows the percentage of xx model runs that…….Fig 6c shows that at KIS, ocean exposure durations, T(o) of approx. 1200-2200 yrs are preferred by the model but the model provides less constraint on preferred grounding line readvance duration, T(i)"
756 - I can't follow what this means – produced *what* simulated values of Fm and TOC ? (presumably all the runs produced simulated values of these two variables?). And what do you mean by 'fit measured values' – within 2-sigma ? Some other measure? How many runs of the model do these % correspond to?

Figure 7
Worth reversing x-axis so that time goes same way as in rest of paper.
What are the thin curves plotting ? – I'm presuming it is the optimum ('highest percentage') values of To and Ti taken from Fig 6? How does the stacking work ? The results in fig 6 look like they are combined results for each site already.
These curves in Fig 7 also need some expression of uncertainty on them – for example Fig 6b shows that the curve for WIS should have broader uncertainty ranges than the curves for KIS in Fig 6c but at present this is not reflected in Fig 7.
Explain which constraint is plotted in the solid bars for T(i) – is it the temperature modelling?
As with figure 6 – the caption could be more helpful to a reader – it took me some time to work out that (I think) it is the peaks in the thin curves that I am looking at for most likely exposure-readvance duration combinations and which therefore are the most likely date for initial retreat past the site?

Figure 8
The blue and orange colour bars for frequency of successful runs have no scale.
Cyan diamond not visible to me
Add WIS and BIS/KIS flowlines to panel b.

Figure 9
See notes above. ND not needed in axis label for x-axis

Supplementary
77 – Celsius
Eqn S14 – should this be m(b) rather than m?
147 – if your model cannot distinguish between the two conceptual models then should say so
155 – the reference here to provenance relates to flowline (and geology) differences rather than any differences in timing behaviour and so I don't think it is relevant
Table S1
units missing from several columns

Make core nomenclature clearer e.g. in first row is this the data for RISP Core 7 at a depth of 46.6cm or for core 7-4 at 6.6 cm ?