The Cryosphere Discussions

Interactive comment on “Mid-Holocene thinning of David Glacier, Antarctica: Chronology and Controls” *by* Jamey Stutz et al.

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Please find below my review comments, as well as attached as a pdf (just in case this is more helpful for the authors, particularly with formatting that may not show up below)

General comments

Using 10Be and 3He exposure ages at multiple sites, Stutz et al. constrain the minimum LGM thickness and post-LGM thinning history of the David Glacier, one of the largest glaciers draining ice from the East Antarctic Ice Sheet into the Ross Sea. The paper adds to our knowledge of the past behaviour of the EAIS, filling in a large spatial gap. Through flowline modelling, the authors then explore the potential dominant mechanisms/forcings that could help explain the retreat and thinning history of the glacier, informed by both their own constraints as well as marine evidence. The paper will be of great interest to both glacial geologists and numerical ice sheet modellers alike. I thoroughly enjoyed reading the paper and found it very informative and interesting. The paper is well written, logically structured, and the figures are of a high quality, making it easy to follow for the vast majority of it. I would not class any of my comments as “major”. Most of my comments are requesting a little bit more information in a few parts of the paper, or minor technical corrections/suggestions. I recommend publication after addressing some points, listed below:

Specific comments (intermediate)

We thank Dr. Nichols for their clear and thoughtful review, and offer our responses below each comment in italics.

Sect. 2.1 - I think a short paragraph (either at the end of this section of the beginning of the next section, 2.2) describing how exactly the exposure ages inform the modelling approach would be helpful. I think at present it is a little unclear as to how the two are linked.

We tried to highlight this in the first sentence in section 4 ‘Results: Glacier Modelling’ but we agree this can be expanded and emphasised in Section 2. We will highlight the conditions required of a geometric fit (i.e. the initial ice surface covers the site of interest prior to thinning). However, this is quite difficult to do without first presenting the mid-Holocene exposure ages. For this final reason, we highlight how the exposure ages focus our modelling *during the thinning period identified in the exposure ages and data model comparison figures (Section 3 Results-chronology).*

Additionally, I think a little bit more info on which parameters were varied in the sensitivity experiments, and how they were chosen, would be beneficial.

We expect this comment refers to modern sensitivity experiments. We use the published in situ and satellite data (e.g. ice sheet upper surface, bed, accumulation and velocity) to support our modern sensitivity experiments, those were held constant, helping to hone in on a suitable basal traction condition that best matched the modern configuration (upper ice surface and grounding line position). We did not vary sub-ice shelf melt rate or lateral buttressing parameters for these modern experiments. If this comment applies to deglacial sensitivity experiments we primarily focused on sub ice shelf melt rate and lateral buttressing, as regional proxies for internal ice temperature and accumulation show relatively minor variation, therefore we did not focus on these parameters. We will add these details near L200 around at "Using an optimised set of accumulation and temperature forcings…

As with the previous comment, we will highlight this further in section 4 as we first should present the chronology which then focuses us in the mid-Holocene. We will also include a comment here regarding limitations of this model and the parameters it includes. Additionally, we intend to supply a table in the supplement showing exactly how we vary parameters in sensitivity experiments.

Could the authors produce a figure for the D'Urville Wall and Mt. Neumayer area similar to Figures 2 and 4? At present, section 3.3 comes as somewhat of a surprise, and is difficult to place spatially (though it is helpful that the location is shown in Figure A2. I must say that I very much like the supplementary figures).

Yes, we are happy to do this.

The authors refer the reader to the online antarctica.ice-d.org database for nuclide concentrations and other information required to calculate the exposure ages reported. I think it would be beneficial to add a table to the supplement of this paper including both information that is already included in the ICE-D database (sample IDs, nuclide concentrations, samples thickness, shielding factor, etc.) as well as some information that is not. The latter would include (for Be) quartz mass, Be carrier mass, and the 10Be/9Be ratio (+ for process blank(s)).This information would be necessary if a reader were to want to redo the data reduction before recalculating exposure ages.

Yes, we will add supplemental tables (in .xls format) for sample information as well as sample analytical data.

Additionally, because the sample data is not included in a table in the paper, the only place to see which samples were analysed is in Figs 2 and 4. Because there is no figure showing the samples analysed for the D'Urville Wall and Mt. Neumayer, the reader cannot double check the exposure ages or recalculate them independently.
In the ICE-D database, there are no exposure ages or nuclide concentrations included for any of the samples from the D'Urville Wall site. Additionally, the D'Urville Wall site is named "Mt. Neumayer", whilst there is another separate section for the Mt. Neumayer samples.

This is mislabelled will be fixed for clarity.

Specific comments (minor/technical)

L 24 "Antarctic ice sheet" – this is the first mention of this phrase here, the authors could add "(AIS)" here rather than on line 28. *Thank you, noted.*

L 47 A space is needed after "Oscillation" *Agree, noted*

L 54 Are the references for the statements in this sentence the same as the next one (papers by Anderson and McKay)? If not, I think references may be needed here in line 54, otherwise please disregard.

The references are different. The sentences on L53-54 needs updated references and we will add these (Licht et al., 1996, Domack et al., 1999 and McKay et al., 2008)

L 59 "TAM" hasn't been defined yet. After defining it here in Line 59, you can remove "Transantarctic Mountains" in line 64 and replace with TAM. *Agree, noted.*

L 76 “sampled” could be changed to “collected”. *Agree, noted.*

L 79 Should it be “using the structure from motion technique…”? *Agree, noted.*

L 84 I think starting this sentence with the phrase “The aim of the sampling method is to track the upper ice surface…” would be more accurate. *Agree, noted.*

L 90 I think some extra context would be useful at the end of this section. Why would bedrock be more useful for longer term exposure vs erratics?

Noted. While bedrock is not the focus of this study, we will add “Exposure ages from bedrock is useful for understanding longer term exposure histories and duration due to recognition of non-erosive burial by cold-based ice (e.g. Atkins et al., 2013; Joy et al., 2014).

L 95 How many etchings were done with the samples? A range would be useful.

We will expand this to include “Two etchings in total: One day etching at 2.5% HF and a multi-day etching at 1% HF”

L 101 (and reference list) The reference to Balter et al. (2020) can be updated from the Cryosphere Discussion paper to the final paper (Possibly Balter-Kennedy et al., 2020 now instead?). *Agree, noted.*

L 101-102 Which nuclides were measured in these additional samples?

Be and He as indicated. We will clarify this for the additional samples.

L 106-108 I think links to the online calculators, both the ice-tea one and that which has evolved from the Balco et al. (2008) paper, would be useful additions here. *Agree, noted.*

Sect. 2.2.2 When the authors use the phrase "consistent with all existing geological constraints" (L 187) and "consistent with geologic constraints" (L 190), does this refer to the exposure ages produced by this study, prior geologic constraints, or both?

It refers to both. W12 fits well with all geological constraints (prior to 2012 publication) and the modelled initial ice surface lies above our highest elevation Holocene aged erratics. We will clarify this in the text by including after L182: "W12 is chosen as, at the time of its publication, fits well with all existing geological constraints" Further on L190, we will include "the modelled initial ice surface lies above our highest elevation Holocene aged erratics"

L 108 (and reference list) Balco (2020) is referenced for the ICE-D database. In the reference list, the entry for Balco 2020 is for a study in the Annual Reviews journal, however, I think the paper the authors intend to reference is that in Geochronology (https://gchron.copernicus.org/articles/2/169/2020/). *Agree, noted.*

L 206 Table number is missing here (also line 256). *Agree, noted.*

L 246-247 “High elevation bedrock samples are much younger than exposure ages from nearby bedrock at similar height above the local ice surface” - Should the second part of the sentence read “from nearby erratics”? Otherwise, this sentence is a little confusing.

We will clarify this and include a plot of bedrock samples measured in this study and their position on the landscape relative to other bedrock samples from previous studies (e.g. Ricker Hills, Strasky et al., 2007,2009 and NVL, Di Nicola et al., 2012) as a way to contextualise the bedrock data without further nuclide measurements. This is not the focus of the study but a comparison of local bedrock data will provide some context from higher elevation sites along the David Glacier.

Sect. 4 L 252 I think one or two sentences briefly summarising the exposure age findings (timing and magnitude of thinning at the different sites) would make for a handy intro to this section. At present it feels like a jump to go from Sect. 3 to Sect.4, I think an additional sentence would help link them.
Agree, noted.

L 264 To help the reader follow, I would reiterate here that, as stated in L 144 – 146, “a reduction in lateral buttressing is expected as the expanded David Glacier and grounded ice in the Ross Sea decouple” *Agree, noted.*

L 295 “The reconstructed palaeo-thinning along the David Glacier during the mid-Holocene is synchronous with rapid thinning reconstructed at a number of sites in Antarctica”

In addition to citing the study by Small et al. (2019), I think it would be helpful to the reader to list and cite the sites around Antarctica which the authors have in mind here. In the abstract, the authors mention that the timing and rate of thinning at David Glacier is similar to reconstructions in the Amundsen and Weddell embayments, so I think it would be helpful to know the exact sites and records in those two regions.

We agree and it will also give better credit to those studies that preceded ours.

L 313 – 318 I think a sentence or two on the rationale/motivation for the data model comparison may be helpful to the reader. Something on what the data model provides in the grand scheme of things (like helping to inform future modelling studies) could be useful. This may also help to link this part of the paper to the rest of the study.

Agree. In fact, the exposure ages and the DMC all setup the rationale for our flowline modelling. For this, we propose to move the DMC section from Discussion to Results: specifically, the Chronology section starting on L250.

On the same point, the paragraph at lines 331 to 357 covers what I think would be better suited to the start of this sub-section. I think this paragraph would be better placed prior to the data model comparison (so prior to line 313). *Agree. We will move L331-357 to follow L318.*

Additionally, I think the paper would flow better if the Palaeo-thinning rates and data-model comparison were separated into two sub sections. So 5.1 with the thinning rates, then 5.2 with the data model comparison.

Thank you, we agree. As mentioned before, we will move this section into results after L250.

L 324 “15-13 345 ka” should this be 15-13 ka? *Agree, noted.*

L 331 "…widespread interior in its interior…" should possibly be widespread "thinning"? Agree, noted.

L 387 Should it be adjacent "to" Mt. Kring, rather than adjacent "at"? *Agree, noted. We will remove adjacent and keep 'at'*

L 424 Two question marks here within the brackets – I imagine this might be two references missing due to a reference manager error? *Yes this is an error. Thanks.*

Coulman Island is mentioned a few times but is not included in any of the location figures (though the Coulman Island GZW is mentioned in Figure A2). If possible, labelling it in one of the earlier figures would be helpful – though I do not think this is a problem worth making an entirely new figure for. If it cannot be easily labelled in an existing figure, at the first mention in the text, the location could be described in a little more detail (e.g., XX km in XX direction from the DIT) to save from making a new figure just to add a label for one location.

We will label it on the model map (second fig A1-which will be relabelled as A5).

Figures

Figure 1 caption – there is an "A)" at the start of the caption, but it appears to be the only part of the figure (i.e. no Figure 1B, C etc.). *Noted, will remove 'A)'*

I may have missed it in the text, but what is the source of the bathymetric features? The iceberg scour, grounding zone wedge etc. locations? If not mentioned in the text, I think this could be added to the caption (my apologies if I missed this in the text, though). *The author mapped these features using GeoMapApp based on analogs and experience from MSc research. We will indicate the mapping method, type of data, spatial resolution and include link to GeoMapApp GMRT dataset as well as analogs (after Dowdeswell et al., 2016 10.1144/M46.171) in L138.*

Figure 2 and 4: Changing the colour of the 3He exposure ages from grey to something else may help them stand out – at present they blend in with the colour of the ice.

We'd prefer to keep standard colors for nuclides (Grey for ^{3}He as suggested here: https://cosmognosis.wordpress.com/2018/10/08/what-color-is-beryllium-10/). Also to emphasise Holocene aged samples, the focus of the study. We will ensure the grey boxes stand out better.

Figure 2 – It is not clear which samples in ICE-D match those with the sample IDs MtKring01px4-5, MtKring02px, 03px, and 03ol in Figure 2. MK04 in Figure 2, but there are no ages or nuclide concentrations for this sample in ICE-D. Additionally, MK14 is a 10Be age but is grey, should it be red?

We will fix the labels in fig 2 to match ICE-D. MK04 is mislabelled on the map and should be MK03. The samples are from the same location and only MK03 has been measured for 10Be. MK14 is plotted beneath MK13 (again, same location). We will ensure the Be derived age is visible.

Figure 3 B "20" on the y axis, and "7.5" on the x axis are overlapping. *Agree, noted.*

Even though some of them may be obvious, I think some of the terms in equations 1-6 are not defined. *Noted. We will ensure all terms are appropriately defined.*

Figure 9 My apologies if I have missed it, but SIS is defined in the figure caption, but I don't see SIS labelled in the figure.
We will remove SIS as an earlier version of this figure included SIS.

I was a little confused by the appendix – is it meant to be split into two parts (the latter with the model setup and results)? At present there seems to be two Figs A1, 2, 3, and 4.

Appendix should probably be a supplement. There are two sets of appendix figures labelled A1-3. This is a latex derived plotting error and will be fixed.

Figure A3 (first one) Orange circles – do the authors mean red circles? Also, the grey squares are not mentioned in the caption. *Agree, noted*

Figure A4 (first one) - The red squares are not mentioned in the caption. *Agree, noted*
Figure A1 (second one) This is not of huge importance, but I think Figure A1 would be more useful within the main text given the importance of the flowline model to the overall study. Also, location name abbreviations in the figure caption need to be defined (my apologies if they have been defined elsewhere and I missed them).
We agree and will include it in main text as well as define the abbreviations in the caption.

Please also note the supplement to this comment: https://tc.copernicus.org/preprints/tc-2020-284/tc-2020-284-RC1-supplement.pdf.

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2020-284, 2020.

Interactive comment on "Mid-Holocene thinning of David Glacier, Antarctica: Chronology and Controls" *by* Jamey Stutz et al.

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In this paper Stutz et al. present a combination of geochronological and numerical model evidence for the glacial history of David Glacier and the potential drivers of its retreat.

I really like data/model comparison investigations like this study, and the paper includes some interesting results regarding the dynamics of the largest outlet glacier in Victoria Land. I have included detailed points for consideration by the authors below. In addition to these, as a general point, I think the findings of the paper would come through better if there was a clear separation between background/results/discussion in section 5. This may require some restructuring/rewording of the paper, but would really allow a

more concise discussion of the key results of the paper and their implications while communicating its overall findings more clearly.

We sincerely thank referee James Lea for their thoughtful and detailed review of this manuscript. We offer our responses below each comment in italics. We agree with this general comment and will provide a clearer separation between the background, results and discussion in the revised paper. We acknowledge that the discussion does contain suitable material for the background section but we feel is better suited in its current place in line with the major discussion points. We propose to move Discussion section 5.1 to the results section 3 (after L250).

L25 – (Weber et al., 2014). *Agree, noted*

L53-64 – there's a few names of locations mentioned that I'm unfamiliar with – if names of locations are mentioned they should be labelled on location figures

This is highlighted by the other referee and we agree. We will include all appropriate place names on maps and map insets.

All figures – I would encourage the authors to ensure that all figures and their labels are at the very least red/green colour blind friendly to improve accessibility and interpretability

We agree in principle but we prefer to keep the existing colours for samples/data because they follow an effort to standardise colours in the surface exposure dating community. In our maps, surface ice velocity is typically in a rainbow colourmap, but this will be changed in the figures. For modelling results, the rainbow pattern does help to highlight the various phases during retreat but we will ensure red/green colour blind friendly where applicable by ensuring that colours are not superimposed.

L79 – should state whether this from ground based photos or drones.

Agree. Will indicate this is photography from a helicopter.

L81 – There are two sets of figures A3 and A4 (p 24/25 and 30/31). *Agree, noted.*

L86-90 – should include a supplementary table indicating location, type and (if available) geomorphological setting of samples that were collected, those that were analysed and information about results of analysis. *Agree, noted. Will include data tables in .xls format as supplementary data.*

L119 – should make clear that by ice sheet flow, you're referring to the ice sheet interior rather than the entire domain. *Agree, noted.*

L149 – figure A1 (p28) – it would be worth having a panel showing a zoomed in view of the region around the grounding line so the transition from stream to shelf flow can be resolved in detail. A map of subglacial topography would be valuable in this area too to show how representative the ice stream width is of the trough where flow is most rapid.

We agree this is an important area to show detail. Fig. A1 (p24) is meant to convey both the transition from stream to shelf flow as well as provide along and cross flow cross sections of topography/bathymetry. We will highlight this and reference this figure.

Section 2.2.1 – the authors should expand on how width is defined in the model, especially in the regions where the grounding line is observed to be dynamic. Upstream definition of width is also important as defining the accumulation area and hence balance flux velocities. These are always tricky to define, but a bit of information about how they have been arrived at would be useful.
Agree, we will expand on the methodology for determining basin dimensions in this section.

Also, a table of key model parameters (e.g. grid size, ice T, ice density, proglacial water density etc) would be informative.
Agree. We will include this information (in table form) alongside equations 1-6

L162-3 – this is where a zoomed in view around the modern grounding line would be useful for the reader.

We agree that a zoomed in view around the modern grounding line is useful. We think that Fig. A1 (p28) provides a reasonable scale view and context for the modern grounding line and surrounding regions, and would prefer not to generate another figure unless strictly necessary.

L174/Section 2.2.2 – some more info about the model spin up to LGM would be useful, i.e. is it tuned to the W12 configuration or is there a relaxation period from this?

Also given that you're using W12 which was derived using the shallow ice approximation based GLIMMER model, are any mismatches between spun up configurations/velocities and the W12 configuration observed/expected. Given W12 was simulated on a 20 km grid this may be tricky to identify, depending on the along flow grid size that is being used in the flowline model. Are there reasons why W12 was chosen over other model simulations? If the model is struggling to replicate the steep descent from the interior, my gut feeling is that it may be due to a combination of too wide ice width and the SSA nature of the model that include longitudinal stresses. Without a map of the subglacial topography in this area however, it's tricky to say. It may also be a product of how bed/surface topography values have been input into the model and how the real world data have been summarised (i.e. whether they are a simple transect, or if they are width averaged). These points should be addressed if it is thought that they impact/have impacted the tuning of the model, and/or if it will impact the delivery of ice to the grounding line or significantly impact downstream ice thickness (i.e. have implications for the comparison of modelled results to observations).

Most Antarctic deglacial simulations do not attempt to fit to all available geological constraints, and other alternatives that did fit to constraints are coarse resolution (e.g. Briggs et al., 2014, 40 km). We mainly were interested in a model that fit to all geological constraints and thus provided a reasonable starting point in which to model the upper ice surface. W12 is on an old bed topography, has a lower spatial resolution and is solved using the shallow ice approximation, so we should not expect it to match our surface profiles – it's purely a starting point for the model from which our model equilibrates as it adjusts to the boundary conditions, parameters and physics of our flowline model. We will expand and clarify this in section 2.2.2.

L207 – Table number needs filling in. *Agree, noted.*

Section 3.3 – as earlier, place names referred to need to be labelled. *Agree, noted.*

L252 – this sentence dives straight into the detail, and would benefit from clarification as to whether the ice thinning is the observed or modelled thinning. *Agree, noted. We feel that by moving discussion section 5.1 to ~L250 will help us explain our motivation for undertaking the modelling work.*

L256 – Table number *Agree, noted.*

L257 – why were melt rates of -1.5, 2 and 11 m/yr chosen? If they were part of a larger ensemble of simulations (as indicated by the end of L259?) this is worth reporting. At present the values chosen to be reported in the paper appear a bit arbitrary

We agree that we should include an explanation that we progressively increased melt rate until partial to full retreat is initiated. Further, we will add a table of parameter values and experiments in the supplement.

L261 – how much above the Hughes Bluff site is the modelled ice surface?

300 m above modelled ice surface, Fig A2 (pg29).

Printer-friendly version

L261/262 – are there criteria for what represents good agreement? If not, the difference between the reconstructed and simulated elevation should be included.

Good agreement means the modelled ice surface at the end of the simulation lies slightly below lowest collected erratic. We will include the difference in the text but do not see much value in this as we do have a discussion of final modelled upper ice surface (particularly for Mt. Kring) in Section 5.4.

L264 – again, a bit of justification for the range of simulations presented would be good to have, in addition to the forcing value choices for the combined forcing simulations

We agree. A table of parameters and listing the different experiments will be included in the supplement

L266-269 – check this sentence for grammar. *Noted*

Fig 6, A3, A4 (model simulations) – on the right hand panels, is the time axis appropriate in that I don't think the model is being forced by any date specific reconstructions?

The model is not forced by a date-specific reconstruction, but it is plotted in model years to allow general comparison with cosmogenic ages.

Discussion paper

L282-4 – need to be clear what exactly you mean by "match periods of onshore thinning" (linked to above comment). Although retreat occurs approx. -6.5kyr in model simulation time, it should be explained why it is anticipated/expected that this matches to "real world" years.

We agree that this can be clearer. We will highlight the geometric fit and improve on what appears to be a chronological fit. The modelled period is 15,000 years with spin up during the first 7,500 years. This approach approximates the timescale for change following the Antarctic Cold Reversal and main phase of deglaciation in Antarctica. It is not meant to reflect 'real world' years but simply serves as a common timescale in which to compare against our thinning chronology. "match periods of onshore thinning" refers to the simultaneous upper ice surface elevation and grounding line location being consistent with onshore thinning (e.g. upper ice surface is below the lowest/youngest erratic at each site). This is a geometric fit and we will highlight this point. The fundamental take-home point is that the upper ice surface lies above the Hughes Bluff site when the grounding line is pinned to the sill at the outlet and the resulting modelled retreat over this sill is responsible for the thinning history deduced from our chronologies.

L287-8 – this should probably be referred to up front in the methods. *Noted*

L291-99 – I think these would go better in the results section, with any methods employed described there. *We agree and will change this.*

Figure 7 – the plots don't really give much of an impression as to the variability within the line cloud – is it possible to replot the lines but set a transparency on each so can get an impression of the distribution of the modelled uncertainty?

The uncertainty bounds represent a quantitative assessment, and we do not agree that simply changing the transparency would provide any relevant insight

L313-324 – again, a clearer separation of results from the discussion would help

Agree, noted.

L313 – I would be very cautious of attempting to read too much into straight data/model comparisons without accounting for model grid size, flow approximations/model physics used, forcing and boundary conditions in the interpretation.

This is a reasonable point which we agree with, but the multiple ice sheet models that we compare against have a range of different resolutions, boundary conditions, parameter choices and flow physics considered. This is the point. The purpose of the comparison is to highlight the differences between models as well as between the model suite and geological data – to illustrate that few models perform well and that there is still important work to do in this space.

L325 – magnitude instead of amplitude?
Agree, noted.

Section 5.1 – this would benefit from a sentence or so on what the motivation for undertaking the data/model comparison is. As it's not mentioned in the paper before it appears a bit out of the blue currently.

This is a fair point that was also noted by referee 1. We will move this text to the results section to help contextualise the modelling results.

Section 5.2 – data presented in the paper are only written about in the last paragraph of this section, and otherwise is background info about the site.
We agree and will include more discussion of our data. We argue that the 'site information' is critical here to highlight the offshore ice constraints as well as gaps in understanding.

L383 – if the ice tongue is grounded then definitely, however if it isn't then it could be that the upstream ice thickness is maintained in a scenario where the Drygalski Ice Tongue is lost (as its removal would not change the amount of buttressing). To demonstrate this for certain though would require a separate set of model experiments. Unless there is other evidence for the Drygalski Ice Tongue being a permanent feature since 6ka BP I would still be cautious about linking it to the Terra Nova Bay polynya.

We will clarify this point by better explaining existing geological constraints from around TNB e.g. The raised beach chronology suggests open marine conditions are established in TNB…our chronology from Hughes Bluff is the 'other evidence' for the persistence of the DIT. We include references to modern observations and paleo-oceanographic studies that suggest the intimate link with DIT and TNB polyna.

L389-403 – most of this is site description rather than discussion.

Agree. Happy to remove lines 391-394 but in our view L 396-403 remain a powerful comparison with modern understanding from satellite data as well as highlighting complexities in projecting Mt. Kring data over 10's of km to the flowline location in the middle of the ice stream / glacier.

L408 – write out full abbreviation of MISI. *Agree, noted.*

L416-7 – if this is the case it should be acknowledged/alluded to when the definition of the model domain is described. *Agree, noted*

L422-30 – more site description than discussion of results.

Agree, we will move some of this to background section but we argue some of this is relevant to the discussion topic: controls on thinning and retreat, particularly the potential for lingering ice on bathymetric highs and it's impact on lateral (drag) buttressing.

L463-465 – this is quite a bold statement, and it is a bit of a leap to say that the results of this study show this conclusively.

We respectfully disagree. Our modelling and chronology highlight a well-known process – dynamic thinning which has been observed in modern satellite data as well as in models. This process has been poorly documented over geologic timescales and we argue that our unique chronology documents this process and provides a first glimpse at how long dynamic thinning can persist. Given the unique nature of our chronology, we do not agree that this does not apply elsewhere in Antarctica, particularly those areas that have been shown to be undergoing dynamic thinning currently. Further, we state, 'if the data and modelling presented in this study is representative of outlet glacier behaviour more generally' and 'may' suggests the potential inconclusivity of our results. Essentially, this is the first paleo-documented case, otherwise we've only observed dynamic ice sheet thinning during the satellite era (last 40 years).

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2020-284, 2020.