The Cryosphere Discussions

Interactive comment

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

James Lea (Referee)

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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).

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.

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).

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