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

James Lea (Referee)

j.lea@liverpool.ac.uk

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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.

L25 – (Weber et al., 2014)

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

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

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

L81 – There are two sets of figures A3 and A4 (p 24/25 and 30/31)

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.

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

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.

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. Also, a table of key model parameters (e.g. grid size, ice T, ice density, proglacial water density etc) would be informative

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

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).

L207 – Table number needs filling in

Section 3.3 – as earlier, place names referred to need to be labelled

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.

L256 – Table number

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

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

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

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

L266-269 – check this sentence for grammar

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?

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.

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

L291-99 – I think these would go better in the results section, with any methods employed described there.

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?

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

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

L325 – magnitude instead of amplitude?

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.

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.

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.

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

L408 – write out full abbreviation of MISI

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

L422-30 – more site description than discussion of results

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.

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