Response to reviewer 2

The paper by Stoker et al. addresses the timing of continental glaciation in northwestern Canada during the last deglacial period using surface exposure dating and modeling results.

Overall, I find this paper to be well written and provides a fairly comprehensive assessment of the glacial history, prior data, and prior work on the ice history in this region. The data itself are a nice contribution to the existing data and I think warranted for publication. To me, the authors have done a nice job in both presenting their new work and putting it into the context of existing datasets. Please see my line comments for mostly minor suggestions. Some of the questions I have regarding the Bayesian modeling are a little more major as are the reporting of information regarding the 10Be ages, calculations, and lab procedures.

Thank you for taking the time to review our manuscript and for the detailed comments. We copy your comments below in black and respond to each of them in red text. When we copy text from the manuscript into the response we 'underline' sections where we have added new text and 'strikethrough' to indicate deleted text.

Line Comments:

Line 53: Early 36Cl dates - how many dates were presented in the earlier work? Perhaps say.

Done.

Line 59: "On the all-time" – this is colloquial and hard to understand what is meant. Consider rephrasing.

Done.

Line 65: "Dipstick" – define this for the reader.

We have removed the reference to the dipstick approach in this section and we describe our sampling site selection in the methods section.

Line 79: "True" – what is meant by true? Can you define this better for the reader?

We have removed this phrase to prevent any confusion.

Line 98: HF – include the percentage since presumably not concentrated.

We have rewritten this to:

'… etched in a solution of 2:1 concentrated ACS-grade HF acid to deionized water'.

Line 103: The carrier concentration needs to be included for replication purposes. Very important.

Done. It now reads:

'of Be from $BeCl_2$ carrier "*Be Carrier B31 Sept 28, 2012*" which was produced at CRISDal from phenacite sourced from the Ural Mountains, with an ICP-OES-measured average Be concentration of 282 ± 5.64 µg/ml (replicated by N. Lifton at PRIME Lab with a measurement of 279 µg/ml) and density of 1.013 g/ml. The $^{10}Be/^{9}Be$ of the carrier was less than 1×10^{-16}.'

Line 112: The blank values need to be stated. I suggest included them in both the text and supplemental to help the reader understand the precision of the measurements.

Done, this now reads:

'The process blanks had $^{10}Be/^{9}Be$ ranging from 2.7 to 8.2 x 10^{-16}, so for all samples this correction was less than 1% of the adjusted ^{10}Be values.'

Lines 117-118: Again, what are all these uncertainties? Please state them (%) to help the reader understand how each is incorporated in the measurements.

Done. This now reads:

'Individual ages are reported to three significant figures with a 1σ external error (Balco et al., 2008) which considers systematic uncertainties in site production rate, and internal error which includes the following random error sources, added in quadrature: (i) 1σ AMS precision in $^{10}Be/^{9}Be$ (atoms/atoms), which averaged 2.1% and is the greater of the Poisson distributed statistic for the total number of counts on a target or the coefficient of variation about the mean $^{10}Be/^{9}Be$ after three, four, or five passes on each target; (ii) 1σ uncertainty in carrier concentration (µg/ml) which includes uncertainty in density and is based on the greater of the 1-standard deviation of three measurements for a given wavelength or the standard deviation of the two wavelengths (313.042 nm and 234.861 nm). Carrier concentration uncertainty for *Be Carrier B31 Sept 28, 2012* was less than 2% over 9 measurements, but is rounded to 2%; and (iii) 1σ error in sample Be concentration (µg/ml) as measured by ICP-OES, and error contributed by uncertainty in the process blank, which is calculated in the manner as (ii).'

Line 134: What did Cuzzone et al. find? State what is meant by "minimal".

Reviewer 1 had a similar comment, I am copying the reply here:

'In the cited studies the impact of atmospheric mass distribution changes on the calculated exposure ages is over an order of magnitude less than that of GIA impacts. In Cuzzone et al. (2016), the calculation for atmospheric changes was about 4% of the GIA correction, resulting in ages ~1% younger. In Ullman et al. (2016), the atmospheric correction ranged from 1 – 5 % of the GIA correction for their sample sites. In Dulfer et al. (2021), the atmospheric correction was <1% of the GIA correction. We now include these values in the main text.'

Line 135: Model resolution – what is meant by suitable resolution? The models that were used in Cuzzone et al. will have the same resolution for this location. I don't think resolution should factor into any of this or be a justifiable reason to not do it. There is also statistically downscaled data for North America that could be used, so it might in fact be better than the model used in Cuzzone et al.

The Alder and Hostetler (2015) model used by Cuzzone et al. (2016), Ullman et al. (2016), and Dulfer et al. (2021) has a spatial resolution of 3.75° x 3.68° and a temporal resolution of 3ka. The spatial resolution means that a single pixel will cover all of our six TCN exposure dating sample sites. While the rapid retreat over our study area will have been associated with rapid changes in the atmosphere which will not be represented in the 3 ka timesteps of the model.

The resolution is not the main reason that we do not undertake this step. The previous studies (listed in the response to the comment above) all found that a correction for changes in the atmospheric composition was at least an order of magnitude smaller than a correction for GIA-related changes in elevation. This tends to result in exposure ages that are ~1% older. Therefore, we believe it is not necessary to apply this correction as the method and models have previously been found not to substantially affect the calculated exposure age. This does not rule out the fact that improved models of atmospheric composition changes and an improved understanding of the processes operating following deglaciation may require that our ages undergo a correction for atmospheric composition changes in future.

Finally, we believe that the impacts of changes in the atmosphere during deglaciation are likely to have been minimal at our study sites. Staiger et al. (2007) identified that the production rate at an ice marginal site could be up 10% different to the modern day production rate. The strength of this influence is dependent on how long a site is exposed to these changed conditions. Staiger et al. (2007) suggested that sites which were adjacent to an ice margin for prolonged periods (e.g. nunataks or sites at the LGM margin will be most strongly influenced by these changed atmospheric conditions. Our study area experienced rapid deglaciation, and none of our sites fit the characteristics outlined above. This rapid retreat meant that our sampling sites were in an ice margin position for a very limited time and were therefore not exposed the changing atmosphere or katabatic winds for long enough to substantially affect the exposure age.

Lines 164-165: How does the spatial scale factor into the Bayesian model and the priors? It is clear when doing an elevational transect with the 10Be ages but how do the authors account for the 14C ages coming from different locations. Elevation and simple horizontal retreat of the margin will control the timing and it isn't clear to me how it all fits together. I realize this is challenging to explain but I do think it worth trying to explain this to the reader. The supplemental Figures 7 and 8 are just the line code from Oxcal which really isn't that helpful for parsing this all out. At the moment, I don't think a reader could reconstruct what the authors have done and/or the assumptions being made.

Spatial scale is not a factor in our Bayesian model and the priors. The ^{14}C ages we include in our Bayesian modelling setup are located in close proximity to our exposure age dating sites and from a stratigraphic setting which means these sites must postdate deglaciation (e.g. postglacial lake sediments). Therefore, these dates are included using the *Before* function as the stratigraphic setting they are located in means that they provide a strong minimum age on deglaciation which our exposure ages must be compatible with. This was not properly described in the text so we have now included further sentences to describe the model set up:

<u>'Our Bayesian model for the northern sites also included radiocarbon dates. These radiocarbon dates are from post-glacial delta and lake sediments which must postdate our exposure age sampling sites and therefore provide a minimum timing of deglaciation. These radiocarbon ages do not follow our age-elevation prior model, but can be placed in order as they are from sites which must have deglaciated after our northern sites.'</u>

Line 158: General comment on the Bayesian modeling – what are the uncertainties being used in the model for both the 10Be and 14C ages? Presumably, the authors are using the external uncertainties for the 10Be but this isn't made clear in this section. More information would be useful.

This is a good spot. We have now added a sentence to describe the uncertainties, it reads:

<u>'TCN exposure ages were input to the model using the external error while radiocarbon dates were input using the analytical uncertainty reported by the original authors.'</u>

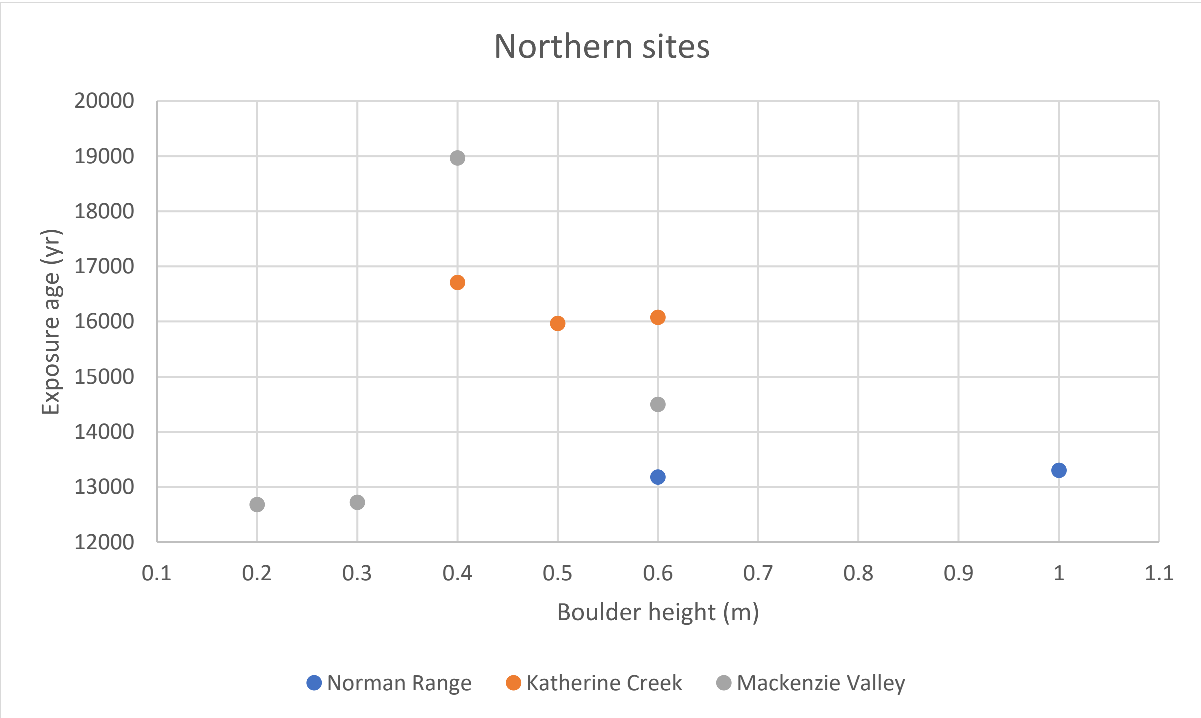
Line 299 and Figure S2: Many of the boulders selected for this study are fairly small and several look like they are being exhumed. The authors should discuss this either in the methods or results about the potential implications. Or, alternatively, give the reader some sense if this is problematic or not (e.g. boulder height plotted versus age by location or in general).

Reviewer 1 had a similar comment on boulder height. We have copied across the comment here:

'We have now expanded upon the methods section to highlight the potential issues of sampling smaller boulders. It now reads:

'In particular, we preferentially sampled the surface of erratic boulders which were: situated on stable ground away from steep slopes (Heyman et al., 2011), display a rounded shape which suggests a longer transport history by the ice sheet, exhibited limited evidence of surface weathering (Balco, 2011), large and well exposed above the ground surface (Heyman et al., 2016). Sampled boulders for TCN exposure dating should be exposed >1m above the ground where possible. Limited boulder availability meant that some smaller boulders were sampled. These smaller boulders are more likely to be impacted by shielding from snow cover or to have been exhumed following the denudation of surface till cover, resulting in an exposure age which underestimates the true deglaciation age (Heyman et al., 2016). In our sampling area, the annual precipitation rates are very low (average snow depth of less than 30cm; Government of Canada, 2019), meaning that snow cover is unlikely to be an issue. The majority of our samples were taken from high elevation sites with very limited till cover, meaning these samples are unlikely have been affected by a thick surface till cover.'

Below, we provide two charts showing sample exposure age against boulder height. In these charts, there is not a strong relationship of taller boulders having older exposure ages. We will include these charts in the supplementary material.'



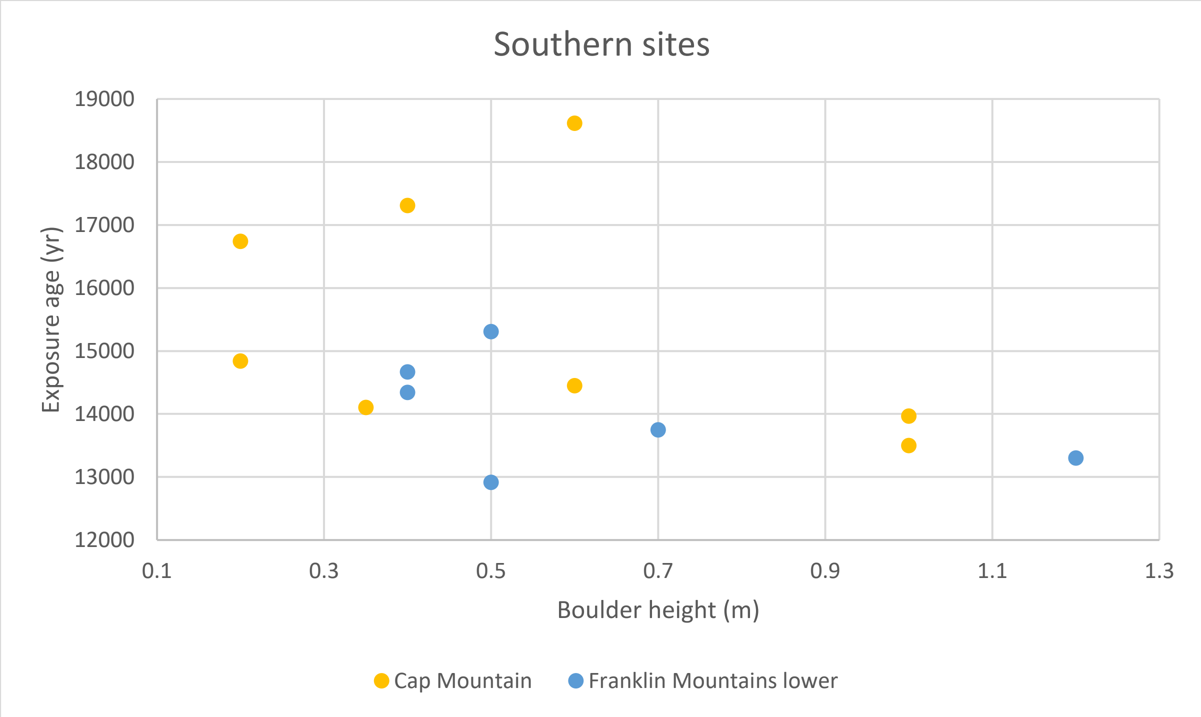


Figure S2: It might be better to break these photos into locations either as new figures or into rows.

The photos are organised in sequence so that each site is together. We have now added labels to each photo to highlight which site each sample relates to.

Line 275: I find this statement a little circular. It assumes the regional deglaciation a priori and then uses it to argue for relatively minor affects. 40km is quite large when considering it in the context of the elevational sampling being done – one may even argue that the elevation changes are quite small themselves, thus top-down elevation dates could in fact be synchronous or even out of order within the uncertainties. I think the authors need to better justify why this is in fact a minor issue – the surface slope of the ice may be a good place to argue and/or the modeling.

Currently, there are no strict best practice guidelines for the grouping of TCN exposure dating samples, which means the procedure can often be subjective with different approaches having positives and negatives. We believe that we have chosen the most appropriate approach considering the basic ice sheet mechanics, past ice sheet reconstructions, and our TCN exposure dates.

The style of deglaciation was dominated by ice sheet thinning in the region. This is demonstrated by our TCN exposure ages which indicate that when the Franklin Mountains and Norman Range at ~800m became ice-free there was still an ice lobe present in the Mackenzie Valley. Based on these observations, we believe the assumption that the higher elevation Cap Mountain site deglaciated before the lower elevation Franklin Mountains sites is reasonable.

The similar position of the sampling sites means that local topographic effects are unlikely to have influenced the pattern of deglaciation between the sampling sites. These sampling sites are all located on the eastern side of the Franklin Mountains, in the direction of the ice retreat compared to the Cap Mountain site. There are no topographic obstacles up-ice that would have impacted the retreat pattern, so the samples were likely exposed at the same time (Mas e Braga, 2021). So in this situation, we believe that ~600m of elevation difference is more important than the 20 km between each locality sampled (40km between the furthest two samples).

Our Bayesian modelling set up does not substantially skew the mean site deglaciation ages. A simple mean average of the raw TCN exposure ages at this southern site would suggest a site deglaciation age of 15.0 ka for the Cap Mountain site and 14.5 ka for the lower Franklin Mountains site. Whereas mean site deglaciation ages calculated from the Bayesian modelling suggest Cap Mountain deglaciation at 14.9 ka and the Lower Franklin Mountains at 14.3 ka. Within the uncertainties of TCN exposure dating, these differences are negligible. However, the Bayesian modelling setup allows us to better constrain the uncertainties of our TCN samples and to identify outliers in a clear and reproducible way.

For the reasons outlined above, we believe that the Bayesian modelling setup we used is appropriate and based on reasonable assumptions. Throughout the text we have included further details to justify our approach. We appreciate that other users may believe a different approach is more appropriate, so we have provided all the raw data necessary for others to recalculate and use our data as they see fit.

Reference: Mas e Braga, M., Selwyn Jones, R., Newall, J.C., Rogozhina, I., Andersen, J.L., Lifton, N.A. and Stroeven, A.P., 2021. Nunataks as barriers to ice flow: implications for palaeo ice sheet reconstructions. *The Cryosphere*, *15*(10), pp.4929-4947

Line 282: What does vetted mean? Try to be more exact and descriptive for your reader.

In this sentence, 'vetted model' refers to the Bayesian model where we have excluded the outlier TCN exposure date (NW-18-21). We have now cited the associated main text figure and supplementary figure to clarify this.

Lines 358-359: I don't recommend phrasing things this way unless the authors have made some considerable effort reaching out to L. Tarasov or the authors of the Reyes et al. paper. Did the authors in fact contact the authors? Otherwise, it is just calling them in published form. Please reconsider.

Thank you for highlighting this. This was not our intention in this sentence. We have now rewritten this sentence to:

'The GIA correction of Reyes et al. (2022) is based on simulations from Tarasov et al. (2012) ~~which are not available, therefore we cannot replicate this method of GIA correction~~. Instead, we apply the GIA correction described in our methods section and following the method of Norris et al. (2022).'

Lines 370-372: While I don't disagree with the authors argument about the Arctic data and it being appropriate (or not). I do think they need to justify things slightly better – the 47 samples may fit the elevation and ages ranges but the spatial changes in the production rate can be quite high because for a variety of reason (e.g. geomagnetic, snow, atmospheric, etc. affects). It would be useful if the authors provided some sense of where these sites are located relative to their own site. Clearly the Reyes et al. paper used the Arctic sites for this very reason so it would help the reader to understand the why the choice for this paper was made as clear as possible.

There is no production rate which perfectly represents the characteristics of our sample sites, but we believe the 'primary' global production rate is the most appropriate (Figure 8). In the global production rate dataset, the highest latitude sample sites are located at ~57°N. This places our sites approximately equal latitudinal distance between the sites of the global production rate and the

Arctic production rate. While our sampling sites may be more similar to the sites of the Arctic production rate in some characteristics compared to the global production rate, we believe that the elevation and age ranges of the Arctic production rate mean it is not appropriate for our study. Therefore, we opt to follow the established practice of using the global production rate where there is no specific local production rate which is appropriate. We accept that there will be some limitations with this choice. In the text, we now provide details on the latitude, elevation, and age of the sites in the Global production rate to allow readers to easily compare it with the Arctic production rate sites.

Regarding snow cover and atmospheric conditions. The sites of the Arctic production rate are located in the coastal areas surrounding Baffin Bay near to contemporary ice masses. Therefore, the atmospheric conditions at the Arctic production rate sites were influenced by the nearby ice sheet for an extended period of time and are likely still influenced by the nearby ice masses. This situation is not representative of our sample sites which experienced rapid deglaciation. In contrast, the global production rate dataset includes some sites from formerly glaciated locations in Scotland. These sites were not situated near an ice sheet margin for an extended and are perhaps more representative of the atmospheric conditions at our sampling sites. The low precipitation and snow depth (< 30 cm average snow depth) observed at our sampling sites are also similar to the global production rate sites which have low snow cover conditions.

Line 460: Corbett et al. 2019 reference is missing. Also, there are several other papers that could be cited here including Koester et al. and Barth et al. from the New England areas where dipsticks have been used to determine the timing of glacial retreat and presumably sea level contributions.

Thank you for the suggestion. We agree this was an oversight and have now included references to both Barth et al (2019) and Koester et al (2021) which refer to rapid ice sheet thinning associated with the Bølling period.

References in general: many references are missing. Please do a thorough review to make sure you have all of them and all extra ones are removed.

We have reviewed the reference list and in-text citations to remove any errors.

Line 488: Data should never be made available on request. Please provide all data in free, online repository following FAIR and journal data standards.

We have made every effort to make all materials and data used in this study freely available in both the main text and associated supplementary materials on Figshare (https://doi.org/10.6084/m9.figshare.20069222.v2). This sentence was intended to cover any oversights or data we may have missed. We agree that this appears misleading though and have now removed this sentence.

Figure 2: Make the text bigger in the figure – hard to read for us oldies. Try to make clear what is new data from this study and what is existing data from others. Some of the radiocarbon ages say "0.0 and null" for the uncertainty. Is this correct and if it is 0.0 make it another decimal and if null, you need to explain what this means.

We have now increased the font size for the radiocarbon ages to increase clarity.

The TCN exposure ages (yellow triangles) are the new data presented by this study.

Thank you for the comment. This was an error which we have now corrected in the revised manuscript.

Figure 3: Are these 14C or 10Be ages? If the latter, which I know they are, make sure to keep the symbols the same as you have in Figure 1. Text is hard to read – consider making larger. It's also hard to know what is transect or not – can you make an inset for the elevational transects that demonstrates the ages with elevation? This would help the reader understand the spatial and elevation data.

We have changed the symbols to match the TCN exposure ages from Figure 1. We have enlarged the text. We have now added elevation transects on panel A and D.

Figure 4: Need some reference to where the data were collected. Maybe plot the points where the ages are from on the map.

Done.

Figure 5: Label the ages on the surface elevation profiles for the ice sheet. This will help the reader out a bit.

Done.

Figure 6: Part C is missing from the figure. I am somewhat confused on final results of the Bayesian modeling. It seems to have picked the most improbable sections of the ages at each of the sites. I think this is largely related to the primary assumption that the dates need to be younger with lower elevation. Or, alternatively, the ages are too uncertain or there are inheritance/exhumation issues with the data to correctly apply this assumption for the Bayesian modeling effort. As reported in this figure (e.g. part A), it would mean that the age tails are the most probable age for four of the PDFs which seems very unlikely to me. This should be addressed. Perhaps this gets clarified once we can see figure C.

In previous versions of the manuscript we included Figure 7B as a single panel within Figure 6 (Figure 6C). In the final manuscript, we developed Figure 6C into a full separate figure (Figure 7). We left the caption for Figure 6C in the submission manuscript in error. We have now removed this portion of the text. Apologies for any confusion this may have caused.

At the northern sites, nearby radiocarbon ages are integrated into the Bayesian model as 'before' ages. The results of including these radiocarbon ages means that the modelled site ages are skewed older, towards the age tail of the possible exposure ages. The modelled site deglaciation ages stay within the uncertainty of the TCN exposure dating method. These radiocarbon dates are high quality and have been replicated, therefore, we believe it is important to include them in the Bayesian model. The Bayesian model set up we have used is the only way to quantitatively integrate all the available chronological constraints in this area and we believe is the most appropriate method.

At the southern sites, the lower Franklin Mountains modelled site ages are skewed slightly younger due to the assumption the lower Franklin Mountains site deglaciated after the Cap Mountain site. We believe this is a fair assumption as the lower Franklins Mountains site is located both (1) at a lower elevation and (2) on the east side of the Franklin Mountains ridgelines, which is in the direction of ice retreat. Both of these factors support the fact the lower Franklin Mountains sites should have deglaciated after the Cap Mountain site. In addition to this, the Bayesian model does not substantially alter the ages at this site (as outlined in a previous comment) and the modelled ages remain well within the uncertainty of the exposure ages.

We understand that there is a lot of debate about the most appropriate way to use TCN exposure ages for reconstructing the retreat of former ice masses. We have made every effort to be as transparent and open in our approach and we provide all of our raw TCN exposure age data so that it can be worked with by other studies and recalculated as others see appropriate.

Figure 9: I'm not sure if this figure is relevant to the main paper.

We believe that this figure provides some important empirical evidence to support the theory of ice sheet saddle collapse in the region and makes the modelling section more convincing.

Table: the authors should provide a data table for input into Cronus (Balco et al. 2008). This will allow the reader to easily replicate their work and if production rates or scaling change, easily adjust those data into the future. This should be standard policy for all work using this or any calculator. The exact input files should be provided. Apologies if I overlooked it.

This is a good point. We now provide the input table for CRONUS in the supplementary material.