Reviewer 3

In this manuscript, Miles et al reconstructed the migration of ice front and the evolution of the velocity field of Denman Glacier, Aurora subglacial basin, East Antarctica based on satellite images from 1962 to 2018. The ice sheet model Ua is then implemented to study the potential drivers of the widespread acceleration of Denman glacier between 1972 and 2009.

The manuscript is well written and easy to follow. Reconstruction of historical evolution of ice flow as well as the calving events is valuable for modellers to verify and improve the physical processes and parameterizatons in the ice sheet models. While the observation work is fascinating, I find the numerical modeling experiments not enough to support the conclusions. The authors conclude that grounding line retreat, ice tongue thinning and unpinning from the pinning point are necessary to rebuild the acceleration over the glacier, and therefore emphasize the impact of ocean warming and calving events to the dynamics of Denman glacier. The three experiments are thinning the ice shelf, adjusting the bedrock elevation near the grounding line, and altering the bedrock elevation of the pinning point. Changing the bedrock elevation to mimic the same unpinning seen by the ice fractures is very tricky, since it affects the entire ice geometry for the wrong reasons. I think the ice sheet model Ua is capable to simulate what you want to investigate, but I suggest adaptations of the simulations (see specific comments). Therefore, I suggest a major change of the manuscript for the simulation sections.

We thank the reviewer for taking the time to comment on our manuscript and for the positive comments detailed above. We address the reviewers concerns on our simulations below:

Specific comments

Line 15: It's mentioned several times in the manuscript the potential instability due to the retrograde slope. I think it could help the readers to understand the configuation better if the authors show in one of the figures (e.g. Fig. 1) a transection along the flow line to show the geometry.

We now include a transect along the flow line in the subpanel in Figure 1 to show the retrograde slope.

Line 30: Is 'Wilkes Land' here and also in Line 49 the same region as 'Aurusa Subglacial Basin'? If so, use one of them to avoid confusion.

Wilkes Land is the geographical region, whereas the Aurora Subglacial Basin is the subglacial basin within Wilkes Land. We have amended the text to avoid confusion:

'This has raised concerns about the future stability of some major outlet glaciers along the Wilkes Land coastline that drain the Aurora Subglacial Basin (ASB)'

Line 68: mention also the conclusions section.

We have added reference to the conclusion in the text.

Line 127-129: The authors predicted that calving event is unlikely due to the absence of any significant rifting or structural damage. The context of calving is missing in the introduction section, such as what could be the earlier indicators of calving events and how do we predict calving.

We appreciate the reviewers point here. However, we note that the concept of the prediction of Denman's next calving event is only a very small part of the manuscript. We feel that adding *background information on the early indicators of calving events in the introduction would distract from the manuscripts main aim – to explore the drivers of Denman's acceleration since 1972.*

Line 139-140: Could you make the scales of subplots of Figure 2 consistent to clearly show the information?

This is a good suggestion. We have revised Figure 2 to make the scales of the subplots in figure 2 consistent.

Line 147-148: Are the rifts a indicator of calving events? Can you discuss more about the formation and development of the rifts? From Figure 2 it's hard for me to see.

We hypothesize that the rifting observed on the Denman ice tongue in the 1970s was important in Denman's calving in 1984. The is because an analysis of the rift patterns on the ice tongue in 1974 and on the subsequent grounded iceberg in 1990 show that the iceberg calved from Rift 7 (See Fig. 2e,f). However, further discussion on the formation and development of the rifts is tricky. This is because we only have satellite imagery available in 1972 and 1974, the next full available image over the Denman ice tongue is not until 1989. Therefore, commenting further on the formation and development of these rifts is very difficult without a greater density of observations.

We have clarified this in the text:

'The rifts periodically form ~10 km inland of Chugunov Island (Fig. 2e), on the western section of the ice tongue, before being advected down-flow. But a more detailed analysis of how the rifts form is not possible because of the limited available satellite imagery in the 1970s and 80s.'

Line 151: 'Fid. 2d' → 'Fig. 2d'

Amended.

Line 159-161: In year 1984 there is a calving event. Do you think that could be one of the reasons that the speed-up is much higher between 1972-74 and 1989 while between 1989 and 2016-17 is slower?

This is an interesting point. If the ice calved in 1984 provided buttressing, a speed up after the calving event would be expected. However, we note that changes in ice shelf extent have not been a dominant driver in the longer-term speed up of Denman. This is because the ice front was further advanced in 2018 than it was in 1972, but ice at Denman's grounding line is flowing 17% faster in 2018 than it did in 1972. We think the main importance of the calving event in the longer-term evolution of Denman, is that it enabled ice to re-advance at a different angle and make less contact the pinning point.

In order to test the direct importance of the calving event on ice velocity we would ideally need satellite image pairs either side of the event, but unfortunately such imagery is not available. In the absence of satellite imagery, we could simulate the calving event by altering the ice-front position in Úa. We agree that this would be an interesting experiment, but we note that we already show seven numerical modelling experiment and that adding a further perturbation may add further complication to an already busy manuscript. Indeed, we note our justification for choosing to compare ice geometries in 1972 and 2009 is that observations show that ice front position was similar in each time period.

We have added further clarification in the text at line 225:

'We chose 2009 for this baseline setup, because the calving front is in approximately the same position as in 1972 when our glacier observations start, thus ruling out any acceleration is response to a change in ice-front extent'

Line 180-182: Could you have another layer of ice flow magnitude and directions (arrows like Figure 5) on top to show the divergence of the ice flow?

We have added the MEAsUREs velocity magnitude and direction to the overview figure (4a). We do not add the change in direction in flow due to the patchy nature of the data in the 1970s.

Line 202-204: 'Ice rheology is assumed to...'→ 'The relationship between creep and stress is assumed to...'

Amended.

Section 4.1: Modelling work is done to understand the acceleration/slowing down of the observed ice flow. Therefore, I think it's essential to at least show the momentum equations implemented by the ice sheet model, where the readers C4 TCD Interactive comment Printer-friendly version Discussion paper could clearly see how ice geometry, basal sliding and ice rheology influence the velocity field.

We have added the following to line 192:

Úa is used to solve the equations of the shallow-ice stream or `shelfy-stream' 193 approximation, (SSA , Cuffey & Paterson, 2010). This can be expressed for one horizontal dimension as :

$$2\partial_x \left(A^{-1/n}\, h\, (\partial_x u)^{1/n}\right) - \quad G \qquad C^{-1/m} u^{1/m} = \rho g h \partial_x s + \frac{1}{2} g h^2 \partial_x \rho$$

ρ

Where A is the rate factor with its corresponding stress factor n, h is the vertical ice thickness, G is a grounding/flotation mask (1 for grounded ice, 0 for floating ice), C is the basal slipperiness with its corresponding stress exponent, m, ρ is the density of ice and g is the acceleration due to gravity.

Section 4.2: The experiments are diagnostic based on the ice geometry from 2009 and the reconstructed ice geometry from 1972, is that right? This should be clarified.

For clarification, Line 223 has been changed to:

"To ascertain the most likely causes of the observed acceleration for Denman ice shelf we start from a baseline set-up representing the ice shelf in 2009 where both ice geometry and velocity are well known and compare to diagnostic simulations of reconstructed 1972 ice geometry"

Experiment (i): The authors modified the ice-shelf thickness with an annual rate. How about the grounded ice upstream from the ice shelf? Are they kept the same between the two simulation years? Will it cause a dramatic thickness change near the grounding line in 1972? Or is there an interpolation done? Please describe your method. Could you show the geometry difference between the two simulations somewhere in the figures?

The thinning is applied only to fully floating nodes with grounded ice kept constant between simulations in a similar methodology used in Gudmundsson et al. 2019. As only fully floating are modified in this way, the thickness at the grounding line itself remains unmodified between the two simulations. Supplementary Figure 4 shows the thickness change applied, with a ~10 m thickening in the vicinity of the grounding line. The paragraph beginning line 235 has been changed to the following to include these additional clarifications.

“To represent ice shelf thinning since 1972, we take the mean annual rate of ice-thickness change from an 1994–2012 ice-shelf thickness change dataset (Paolo et. al., 2015) and scale it up to represent the total thickness change over the 37 years between 1972 and 2009, assuming that the 1994–2012 mean annual rate remains constant during this period. This thickness change is then applied to the 2009 ice geometry, modifying it to better represent the estimated 1972 ice thickness distribution of the Shackleton Ice Shelf, Denman ice tongue and Scott Glacier. Similar to the methodology of Gudmundsson et al. 2019, we only apply this thickness change to fully floating nodes, with no change of ice thickness for grounded ice and ice directly over the grounding line. The total thickness change applied is shown in Supplementary Figure 4. We refer to this perturbation as ‘ice shelf thinning’ because the majority of the floating portions of Denman’s ice tongue and Shackleton Ice Shelf have thinned since 1994, although some sections of Scott Glacier have actually thickened near its calving front (Fig. S4).”

Experiment (ii): I think it’s not appropriate to call this experiment ’grounding line retreat’, because grounding line retreat is impossible without ice geometry change. This experiment adjust the bedrock to have grounding line at a different location. How much uplifting is needed? Normally the bedrock won’t have significant change in short term. The difference of velocity comes from additional basal friction in the uplifted region. This experiment actually shows the sensitivity of velocity field to the basal sliding near the grounding line.

Our methodology is not designed to represent any real earth processes such as isostatic rebound but is instead intended to show the instantaneous effect of a grounding line perturbation on ice velocity with the minimum possible bias to the existing ice velocity field. Directly modifying the ice geometry at the grounding line will have a noticeable effect on the regional ice velocity field due to conservation of flux in addition to any changes arising from the shift in grounding line position, and so we instead raise the bedrock to force the models grounding line to be at a given location. The paragraph starting line 247 has been modified to clarify this:

“In the Úa ice model, the grounding line position is not explicitly defined by the user but is instead a direct result of ice thickness, bedrock depth and the relative densities of ice and sea water. As such, the two ways to perturb a given grounding line are to either modify the ice thickness or the bedrock depth. Modifying the bedrock depth is the less disruptive approach because the resulting effect upon velocity is not biased by an imposed change in ice thickness at the grounding line effecting the regional ice velocity field due to flux conservation, in addition to that caused by shifting the grounding line. Note that raising the bedrock to meet the underside of the ice shelf in this way is not a representation of any real earth processes, it is merely forcing the model to have the grounding line in a particular location, that than enables a diagnostic simulation. To represent grounding line retreat since 1972 we advanced Denman’s grounding line from its position in the 2009 baseline set-up by 10 km to a possible 1972 position. This is achieved via raising the bedrock approximately ~20-30 m in the area shown in Fig. S4. We justify a 10 km retreat since 1972 based on the rate of grounding-line retreat observed between 1996 and 2017 (~5km; Brancato et al., 2020). For the newly grounded area, values of the bed slipperiness, C, are not generated in our model inversion, we therefore prescribe nearest-neighbour values to those at the grounding line in the model inversion.”

Experiment (iii): It’s mentioned in the abstract and the discussion section that the unpinning of ice from Chugunov Island is due to the calving event. From Fig. 4d, e, and also mentioned in the results section, the ice around Chugunov Island might be heavily damaged, leading to unpinning/debuttressing. The calving effect and the damage effect could be simulated by changing the ice front or modify the rate factor 'A'. Why did the authors decide to evaluate the unpinning effect by changing bedrock? Furthermore, how much do you need to change to have the proper unpinning effect?

We agree with the reviewers point that the unpinning from Chugunov Island could be investigated by modifying the rate factor 'A', in addition to the regrounding experiment shown. Due to the limited information about past conditions of ice geometry and properties available to us, any attempt to simulate past conditions will have to some extent rely upon assumptions. We would argue that the assumptions made for the regrounding of the ice at Chugunov Island are more justifiable than those that would be required for an investigation into the rate factor, 'A'. Performing new model inversions using 1972 velocities and ice geometry would be the ideal way to investigate this. However, as the 1972 velocities are relatively patchy and the 1972 ice geometry itself an unknown under investigation it would be impossible to separate out the effect of the damaged ice on both velocity and rate factor from that arising from ice geometry change. The assumptions needed to investigate the effect of regrounding the ice at Chugunov island are easier to justify. Raising the bedrock by ~30m is enough to ground ice along the edge of the model domain and we have assumed basal slipperiness, C, is the same as that located near the grounding line. The paragraph beginning line 263 has been modified to further articulate our reasoning:

"To represent the pinning of Denman's ice tongue against Chugunov Island in the 1972 observations (e.g. Fig. 4d, e), we artificially raise a small area of bedrock on the western edge of Chugunov Island (Fig. S3). Bed slipperiness was set to a value comparable to that immediately upstream of the grounding line. Note that, although past observations suggest that the ice in front of Chugunov Island has been damaged, possibly having an effect on its rate factor, A, we have decided to limit our investigation to the effect of pinning the ice on Chugunov Island without changing rate factor. To properly investigate the possible change in past rate factor we would need less spatially patchy 1972 velocities as well as an accurate understanding of past ice geometry (itself an unknown under investigation) to perform a model inversion for 1972 conditions."

Line 267: E1 is Fig. 5b, not Fig. 5a.

Amended

Line 281: 'experiment 5' → 'experiment E5' or 'E5' same for the other experiments there after.

Amended

Line 286-288: Can you explain why the acceleration on the ice shelf is much higher than the grounding line (Fig. 5a) but all simulations show the opposite pattern?

For most of these simulations a direct comparison can be potentially misleading as we are applying the perturbations in isolation of one another to investigate the general pattern of change in velocity association with each individual perturbation. For example, the isolated un-pinning experiment (Fig. 5e) clearly shows that unpinning from Chugunov Island has a negligible effect on grounding line velocity. The difference from observations for most of these simulations is probably due to the simulations not only omitting a perturbation in ice geometry but also the interactions between different types of perturbation. For the simulation which includes all three perturbations (Fig. 5h) the difference from observations is noticeably less than in the isolated perturbations, and remaining differences can most likely be attributed to the uncertainties in the perturbations applied (eg., *assuming the thickness change over the 37 years is the same as the annual scaled mean change between 1994—2012).*

Line 361: 'E3; Fig. 5e' → 'E3; Fig. 5d'?

Amended

Line 362: 'E4; Fig.5d' → 'Fig. 5a'?

Amended

Line 367: 'E4; Fig. 5d' → 'E4; Fig. 5e'?

Amended

Line 379-381: Could the authors add the simulation of calving event by simply change the ice front position and evaluate its influence on ice velocity?

Our observations show that changes in ice-front extent was unlikely to have driven the long-term acceleration of Denman. This is because that Denman's ice-front is currently further advanced than it was in 1972, but we still observe a 17% acceleration in flow over the grounding line. Therefore, we do not simulate the impact of the calving event by changing the ice-front position because our observations demonstrate that this is not an important contributor to the long-term acceleration of Denman

In our description of the perturbation experiments we clarify our rationale for not simulating the change in ice-front position (Line 225):

'We chose 2009 for this baseline setup, because the calving front is in approximately the same position as in 1972 when our glacier observations start, thus ruling out any acceleration is response to a change in ice-front extent'

Line 433: 'hydrofracturing' → 'hydrofracturing.'

Amended.

Line 447: Morlighem et al., 2019 is not in the reference list.

Amended. The citation in the main text should read 'Morlighem et al., 2020' – we have corrected throughout.

Figure 1: Could the authors add the transection of the geometry along a flow line to clearly show the retrograde bed? Maybe show the perturbation of bedrock in experiment (ii) in the same plot. Point out the position of Chugunov Island.

This is a good suggestion and we have added a transect of the bedrock elevation along the flow line in a subplot to show the retrograde slope.

Figure 2: The subfigures are oriented in different ways, and with different scales, making it hard to compare the size of ice bergs, the development of rifts and so on. Could you have a zoom out subfigure like Figure 1b and put the boxes on top to show the zoom in area of the subfigures?

We have revised Figure 2 to include a zoom out subfigure with boxes indicating the location of the other subpanels. We have also added reference coordinates to all panels and made the scales consistent to enable an easier comparison of the icebergs and rifts.

Figure 4: Could you add a layer of velocity (magnitude and direction) on top of the satellite images to show the change of ice flow? That will make the figure more self-explanatory.

We have added velocity magnitude and direction to the overview figure (4a). We do not add the change in direction in flow due to the patchy nature of the data in the 1970s.

Figure 5: I think there should be a grounding line contour at the pinning point Chugunov Island.

We have added a grounding line contour around Chugunov Island on the appropriate panels.

Figure S2: Could you have a subfigure of simulated velocity? And also please show the location of the grounding line.

Figure S2 has had the recommended changes made.