Review of tc-2021-288

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General comments

Benn et al. present a comprehensive analysis of the processes that have contributed to the weakening and fragmentation of the Thwaites Eastern Ice Shelf (TEIS). They begin with a detailed description of the recent changes in ice velocity, strain rates and fracture patterns inferred from Sentinel-1 imagery, with a particular focus on the progressive weakening and development of the shear zone upstream of the TEIS pinning point. The discrete element model HiDEM is used to simulate fracture development under two conditions: low basal friction versus a 'no slip' boundary condition over the pinning point. Similarities between the modelled and observed fracture patterns lead the authors to conclude that relatively high backstress from the TEIS pinning point is responsible for the extensively fractured ice-shelf state. Additional prognostic experiments performed with the ice-sheet model BISICLES show that ungrounding of the TEIS pinning point or additional ice-shelf damage will not significantly increase mass loss from the Thwaites Glacier basin. Altogether, the authors demonstrate that the TEIS pinning point currently acts as a destablising feature because the pinning point backstress is sufficient to trigger the failure of unconfined, damaged ice undergoing thinning.

This manuscript is timely and of scientific interest given the projected rapid retreat and mass loss from the Amundsen Sea glaciers. Overall, the manuscript is well-written, enjoyable to read, and it provides a valuable record (and explanation) of the processes leading to the destabilisation of TEIS. The use of two different modelling approaches involving an elastic fracture model and a continuum ice dynamics model, combined with the detailed, high temporal resolution analysis of recent Sentinel-1 imagery, is where the manuscript builds on previous analyses of the weakening of TEIS. My main concerns are with the assumptions made about pinning point basal friction, and the need for additional detail about the model representation of the pinning point.

Specific comments

- The conclusion that high pinning point backstress is responsible for the pattern of failure across TEIS (Pg. 9, L9) required a 'no-slip' boundary condition over the model pinning point, resulting in a fairly large zone of zero-displacement upstream. Could this be an overestimation of the basal friction provided by the pinning point? In reality, the pinning point does not reduce ice velocity to zero (Fig. 4 suggests flow speeds of 0.1 to 0.5 m per day over the pinning point). After finding that the inferred pinning point friction coefficient from the Elmer/Ice inversion was too low to modify the pattern of ice displacement, why not increase the friction coefficient over the pinning point area (since you are already rescaling the friction coefficient anyway for HiDEM). This could be an alternative to using a more extreme (and somewhat unrealistic) no-slip boundary condition over the pinning point that appears to overestimate the backstress provided as shown by the large area of stationary ice in Fig. 9. Similarly, in the discussion, you justify the requirement for a high damage density of 0.6 in order to produce a shear zone, but you haven't justified the requirement for very high basal drag provided by the HiDEM pinning point.

- The statement that there is close similarity between the observed Feb 2021 pattern of fracture and the 'no-slip' simulation (Pg. 8, L51) would be more convincing if Fig. 10 and Fig. 3d (2021) were presented beside each other. Fig. 10 has a different coordinate system and orientation to Figs. 3 and 1 (the pinning point has rotated by 45 degrees in Fig. 10), and as result, it is not easy to pick similarities between the two (even with the labels). Including the pinning point outline in Fig. 3 may also help. This comparison is important given that one of the main conclusions from the HiDEM 'no-slip' experiment is that recent fracturing and TEIS fragmentation is due to the backstress provided by the pinning point, rather than gradual ungrounding and a reduction in backstress.

- Pg. 2, L73: The citations provided are not examples of ice shelf disintegration occurring in response to loss of contact with pinning points.

- Pg. 2, L86: I don't think it is correct to say that TEIS crossed a threshold from stable to unstable within the last 5 years when there is much evidence to suggest that TEIS was undergoing gradual change prior to 2016. This also depends on whether you define an unstable ice shelf state as undergoing irreversible change, or by some other definition.

- Pg. 4, L57: You mention that the DEM doesn't include recent data from Wild et al. (2021) on the TEIS pinning point, but it would be useful to provide more information on how the pinning point is actually represented in the model geometry. Does the model pinning point consist of two separate pinning points or one broader grounded region? (You have to zoom quite far in to Fig. 11 to see this). What is the model pinning point height above flotation and is it comparable to the different height above flotation calculations for the same pinning point by Wild et al. (2021)? What is the difference between the modelled and observed ice velocity over the pinning point? Since BISICLES simulations are conducted to show that removal of the pinning point will have no influence on ice loss, you should demonstrate that care has been taken ensure the accurate representation of both model pinning point morphology, and flow resistance provided by the pinning point.

- Pg. 4, L60: What was the time period required to relax the model, and did the model relaxation change the geometry near the TEIS pinning point? Is the model pinning point area and height above flotation still representative of the realworld pinning point after relaxation?
- Pg. 5, L9&15: At this stage of the paper, it isn't clear whether you are referring to the shear zone immediately upstream of the pinning point, or the shear margin between TEIS and TWIT. The TEIS shear zone is introduced in the following section.
- Pg. 5, L15: How large is the area where the ice thickness is set to zero to simulate unpinning? This could also be indicated in a figure.
- Pg. 5, L97-98: Did you vary the pinning point friction, or remove the pinning point entirely?
- Pg. 5, L98: Why did you choose not to relax the model before each simulation?
- Pg. 5, L99: How did the friction coefficient pattern evolve during each forward experiment in comparison to the 2016 basal friction? Did the friction coefficient over the pinning point also evolve in Experiments 00, E0 and ER?
- Pg. 8, L87: Fig. 13 shows that the discharge of ice above flotation, V, decreases by approximately 30% by 2100 in each BISICLES simulation. This is not intuitive and the reasons for this decrease deserve some further discussion.
- Pg. 8, L96: It's not clear where this region of reduced traction is in Fig. 11.
- Fig 12: Is there a reason why you chose to use year 2032 to compare to year 2016? 16 years doesn't seem a sufficient amount of time to allow a model to adjust to a perturbation such as unpinning or an increase in damage. Do the speed changes shown Fig. 12 persist after the year 2032 or is there a further change in speeds as the model readjusts to a new steady state?
- Fig 13: Why does the line for experiment UR end at 2050, experiment E0 end at 2070, and experiment ER end at year 2120 if you ran each simulation until 2100 as stated in the method? As shown, the figure doesn't support the claim that all of the experiments show the same long-term trend if the change in V until 2100 isn't shown for each of the four simulations.
- Pg. 9, L10: Why not modify the model seafloor topography by $+200$ m in order to achieve a more accurate height above flotation at the pinning point location? The BISICLES simulations demonstrate that unpinning will have very little impact on ice discharge from Thwaites Glacier, but if the bathymetry is too deep, is it possible that you are underestimating the flow-resisting effect of the pinning point?

- Pg. 11, L41: Neither of these studies implicate unpinning as a mechanism of ice shelf collapse.

Technical corrections

- Pg. 3, L6: Provide the resolution of the other three velocity products, similar to the Sentinel-1 description.
- Pg. 2, L16: BedMap2 = Bedmap2
- Pg. 3, L26: Begin the paragraph with: "HiDEM is a brittle-elastic fracture model that can be used to simulate..." And then continue with the explanation of how ice is represented as arrays of particles.
- Add north arrows to Figs. 2 and 3. In the text you refer to the regions southwest and northeast of the pinning point.
- The manuscript has two subsections entitled 'Modelling'. The paper would be easier to follow if you changed the first to 'Model experiments' or the second to 'Model results'
- Figs. 2, 4, 7. The resolution is too poor and the text size too small to read the text by the colourbar. Alternatively, use one larger colourbar corresponding to all of the subplots.
- Fig. 4. The legend says shear strain rate, but the unit suggests strain.
- Fig. 6. Do the different dot sizes represent the velocity error or something else?
- Fig. 9. Why does the pinning point outline extend beyond the no-slip region? Is the pinning point grounding line in this figure the modelled grounding line from Elmer/Ice after relaxation?
- Fig. 12. Is the grounding line in the figure the model grounding line at year 2032?