Review of tc-2021-130

General comments

Wild et al. study recent changes in ice dynamics of Thwaites Glacier in West Antarctica and investigates the impact of a subglacial pinning point under the eastern part of the Thwaites Ice Shelf. The authors use a suite of satellite imagery and ice-penetrating radar data to characterize changes over the last decade. A numerical ice sheet model is used to perform diagnostic (fixed-geometry) experiments to understand the ice dynamics associated with recent changes, and to assess the consequences of potential near-future ungrounding of the pinning point.

Thwaites Glacier is a major contributor to sea-level rise in West Antarctica, a region where there is potential for non-linear future mass loss. The underlying processes are not well constrained and the study is thus a timely contribution to a growing body of research on the glaciers in the Amundsen Sea, West Antarctica. The study clearly fits within the scope of TC and should be of high interest for the glaciological and broader scientific community.

The manuscript is well written with an overall coherent story. In general, the datasets and observational methods used are thoroughly described and the conclusions are supported by the analysed data and model experiments. Overall I think this is an exciting study with some unique datasets and interesting model experiments giving us an improved understanding of ice-shelf-glacier behavior at Thwaites.

The few major comments I have relate to the numerical model setup and description, as well as the need for a broader perspective on previous literature in the Introduction, and considering wider implications in Discussion. These comments should be addressed by the authors before publication in TC can be considered.

I should mention that my expertise is mainly ice dynamics and ice-sheet modelling, rather than remote sensing and radar glaciology. My comments below reflect this; the Editor should perhaps consider perspectives from a more empirical-minded scientist as well to get a balanced overall evaluation of the paper.

Major comments

Model setup and description
The following comments largely regards Section 2.5 Ice-dynamics modelling. While this section already contains useful information about the model, it is in my view not

yet complete and does not allow the reader to fully understand your model setup and experiments. As it stands, this does somewhat erode the strength of your findings about the importance of the pinning points and the potential effects of future ungrounding. To alleviate this, I list below some concrete suggestions which I hope that the authors will consider.

First of all, it would be nice with a brief starting paragraph describing what the rationale is behind using a numerical ice sheet model. As it is written now, this section starts a bit abruptly with the reader not really knowing where we are going.

- Regarding the friction law (Eq. 5), you should mention that you are assuming a linear Budd-type law, and that this is just one among several possible friction laws in the literature, and whether/how you think using other friction laws often used in Antarctica (e.g. a Schoof or Tsai law) would influence your results, if at all (this could also be done in Discussion).
- Need to mention what the assumption is for the effective pressure (perfect hydrological connection between the ocean and the grounded ice base), what the equation for N is (can be done in-text), and also what processes/factors this formulation of the effective pressure neglect.
- It is unclear to me why you use one ice-flow approximation for the inversion of the friction coefficient beta (2D SSA), and another for the stress balance calculation (3D Higher Order). Isn't the inversion for beta essentially a series of stress balance calculations? In my view, the cleanest and most consistent approach would be to use Blatter/Pattyn (Higher Order) for everything. I assume this wouldn't be too computationally demanding since you're only doing diagnostic experiments. If the authors have a very good reason for not using the same ice-flow approximation across all experiments, this needs to be stated clearly. And if you're going from 2D SSA to 3D Higher Order, how do you deal with the boundary conditions? Are you rerunning in 3D once you have your 2D inversion (and why?).
- The description of the model mesh needs to be more specific. You mention that you're using an anisotropic mesh refined near grounding lines and narrow shear zones. What exactly is the mesh resolution here, and what is the resolution elsewhere? If you're using a 3D Blatter/Pattyn approximation, how many vertical layers do you use? Are they uniformly vertically spaced, or do you have more finely spaced vertical layers towards the bed? See also Minor comments below.
- How is grounded ice rheology chosen/inferred? What ice temperature is assumed, based on what? How well constrained is this? What are associated uncertainties and potential influence on results?

- How is ice-shelf rheology chosen/inferred? What ice temperature is assumed, based on what? How well constrained is this? What are associated uncertainties and potential influence on results?
- A reference to Table 1 in this section is missing; it would be great if you could briefly describe the experimental setup already here. As it stands, you have mostly outlined the model setup, while the details of your different experiments are missing in this section and are instead described in the Results (Section 3.4). One option is to move parts of Section 3.4 that concerns the experimental setup to Section 2.5.

Description and discussion of model results
The following comments regard mainly Section 3.4 Numerical modeling of the ice-shelf response. In general this section is interesting and the experiments are potentially very revealing. However I'm missing key information regarding the model (see above) and the experimental setup. Therefore it is difficult to fully evaluate the insight gained by this modeling exercise.

You artificially increase basal drag to account for lowered ice viscosity in shear zones; is this straightforward or are there some pitfalls in doing this? Would it be possible to do two separate inversions, inverting for ice rheology (stiffness) for floating-ice areas, and for basal drag for grounded-ice areas? I assume this wouldn't be too much extra work, and wouldn't add much computation time either since you're only doing time-independent stress balance simulations. Adding variable ice-shelf rheology could move your model setup in a more realistic direction and potentially strengthen your interpretation of the importance of the shear margins on ice-shelf flow, and perhaps even reduce the need to increase the friction coefficient 'artificially' so much at the pinning point.

A related question about the inversion and velocity calculations. How well do the modelled velocities actually fit with observed ones, for the 2009 and 2019 observations? What are the RMSEs? Do they consistently under/overestimate? Are there some problematic areas? What about grounded vs floating regions? I think this should be included in a supplementary figure, to give the reader an idea of the model behavior. In particular, we're very interested how well the model captures your key regions of interest (the offshore pinning point, the grounding line, the shear zones). On L336-337 for example you state that "… the inversion is successful…" - more specifics would strengthen this section and further indicate whether your inferences about current and future ice dynamics are robust.

I would like to applaud the authors for the nicely constructed experiments to test the influence of the breakup of the Western Glacier Tongue (Figure 8 and S4).

The ’ungrounding experiments’ (Fig 9) are very interesting, but I would be a bit cautious, since your experiments are stress balance (diagnostic) simulations, and we do not know whether this increase in ice velocity will quickly dissipate or whether it will cause sustained ice speedup. Of course if the latter is the case, this will have major implications for the future of Thwaites over the next decades. You say in the Discussion that you see the breakup of the Western Glacier Tongue as an analog for future post-ungrounding of the Eastern Ice Shelf, and you clearly state in the text that you’re indeed only doing diagnostic experiments. For balance, I think you should also state (in the abstract and/or conclusions) that model experiments with evolving geometry and ice flow would shed further light on the future dynamics and stability of Thwaites. To be clear, I think that transient experiments are out of scope of the current paper, and I do not expect the authors to include this in a revised version of the manuscript.

Wider implications

I would like to see some more elaboration on previous work/state-of-the-art in the Introduction, and related to this, a broader discussion of the study’s findings in the Discussion.

The Introduction overall reads well, with a good introduction to pinning points, ice rises and rumples. There is an appropriate background about Thwaites, including both changes over the long-term (Holocene) and the recent (last decades) past. However, as it stands, the Introduction is a little bit too Thwaites-heavy for my taste. There is scope for expansion about other glaciers than Thwaites, and why studying pinning points/ice shelf retreat is vital. There is relevant information about this already in the first two paragraphs, but the rest of the Introduction talks mostly about Thwaites. Of course Thwaites is a key glacier which the community strives to understand, but there are also others (in Antarctica, Greenland, and beyond). What are the particular knowledge gaps in our current understanding of pinning points/ice-shelf buttressing? What have others done before, and what does this study bring that is novel and different? I think by all means the data and findings in the study are novel, it’s just that you can set the stage more clearly already in the Introduction, and then later pick up on this in the Discussion.

Minor and technical comments

In general, more cross-references to figures and other sections would be great to help the reader.

Abstract
L5. "model perturbation experiments" – could be more specific of what kind of model (ice-shelf-glacier model, numerical model, etc)

1 Introduction
L51. retreat rates -> please specify if you talk about retreat rates of the grounding line or the ice-shelf front here (I think you mean the former)

2 Data and Methods
L86-87. Not entirely clear on the use of bed/bathymetry data here. Please clarify which dataset you use where; how are they integrated/combined? (Jordan et al., 2020 and Morlighem et al., 2020).

L92-93. Would be nice to add a cross-reference to Fig 1a here

L95-96. This is useful information – please specify why (mechanisms/processes) a crevassed (smooth) ice base indicates that ice is in disequilibrium (equilibrium) with the underlying ocean

L168. Which observation? Please clarify.

L182-183. Typo; should be "Lagrangian"

L199. Could you be specific on how large this contribution from REMA is, compared to the others mentioned on L196-197?

L203. should mention what the acronym ISSM stands for, as well as add reference (Larour et al., 2012?)

L213. Typo: should be "Shelfy-"

L218-219. "… guided by surface velocity fields…" – this is a bit vague, please be more specific how you construct the model mesh. Do you only use the gradient of the observed surface velocity fields (which one), or also set the mesh resolution manually in some areas? You mention on L223 that the horizontal resolution of the final mesh is 100 m; does this mean that your mesh resolution is uniform over the entire domain?

3 Results

L232. Figure 3d is not discussed explicitly in this section; perhaps it should be at least cross-referenced to point the reader to this useful summary of temporal evolution of observed retreat rates.

L240-263. What would improve the dynamical insight of this section further is to link these observed changes in grounding-line locations to changes in surface elevation or ice thickness (for example those shown in Figure 5c, but also elsewhere on the larger ice-shelf and upstream of the grounding line).

L265-292. Overall nicely written and interesting findings. Could mention and add cross-references to modeling results (section 3.4) to guide the reader further through this section.

L305. Please explain what a sastrugi is for readers not familiar with snow dynamics.

L308-311. Your final point about ungrounding of the pinning point is important as it is one of the main conclusions of the paper, and even occurs in the title. From the numbers you have provided (avg. height above flotation 8.0 +/- 6.5m and avg. surface-lowering rates 2014-2019/2020 of -0.3 +/- 1.5 m/a), I think you should be a bit more careful with stating that the pinning point may unground in less than one decade. Yes this is possible if the true height above flotation is on the lower end of your stated 8.0 +/- 6.5m, and/or if the true surface-lowering rates are on the higher end of your stated -0.3 +/- 1.5 m/a. Unless I misunderstood something, if you take the average values 8.0 m and -0.3 m/a, complete ungrounding will take 26.7 years. This is of course still interesting and potentially somewhat alarming, but not as dramatic as ”less than a decade”. If my understanding of what you have done is correct, I think you can phrase this in a more balanced way (as well as in the abstract and conclusions); you should indeed mention what ”could” happen, while explaining what is the ”best-guess” (average?). I think you can also state more explicitly here (as well as in the abstract and/or in the conclusions) that your inferred timing of ungrounding is merely an extrapolation of recent thinning rates into the future, and not a prediction. Running a numerical model forward in time could obviously shed light on this in more detail.

L313-315. what is the minimum friction coefficient allowed in the inversion, is it 10 (Pa yr/m)1/2 ?

L334. When inferring basal friction coefficients, why do you use the 2011 grounding line for both the MEaSUREs 2009 (Figure S5a) and Landsat-8 2019 (Figure S5b) velocity fields? Why not use the updated grounding lines from 2019/2020? You state on L334 that all simulations use the 2011 grounding line. Please explain what the reasoning behind this is.

L358. I think you mean “model elements” rather than “pixels” ?

4 Discussion
Overall very well written, discusses main sources of uncertainty and suggests next steps for improvement, as well as discussing results in light of previous studies. A wider-scope discussion related to other Antarctic glaciers and implications for our understanding of ice-shelf buttressing and ice-shelf dynamics in general would improve this section even further (related to my comments about the Introduction), see also Major comments above.

5 Conclusion
L485. I think you should state what kind of model, and that simulations are fixed-geometry velocity snapshots, and thus does not establish whether this will be a short-lived or sustained feature

L493. “… possibly the most likely” – this seems a bit vague or even contradictory. And based on what exactly? You could be more specific here.

Figures

In general, figures are nicely crafted and clear.

Figure 1
Missing information on what A and B means in (a) – I think they are airborne radar flight lines? Also, in (a) and (c), the colorbar labels should be m above sea level (m a.s.l.)

Figure 2
The dashed white line for hydrostatic equilibrium is not visible in the legend

Figure 7.
Would be useful if these figures had the same orientation as Figures 1-6 and 8. This also applies for Figure 9.

Figure S1. Missing axis labels and units in all panels.

Figure S4a. In the legend, "ice (m)" is a bit cryptic. I think you mean "ice thickness" ? In the supplementary, I would also like to see plots of the modelled velocity magnitudes and their mismatches with observed velocity fields (2009 and 2019), not only the velocity directions (see comments above).