Review of Wild et al. "Weakening of the pinning point buttressing Thwaites Glacier, West Antarctica"

June 3, 2021

General comments:

The manuscript by Wild et al. presents a very detailed evaluation of a pinning point that is currently buttressing Thwaites Glacier. Using a range of different tools (e.g. remote sensing data, diagnostic ice-sheet modelling, etc.), the authors reconstruct the evolution of the pinning point and its effect on the ice shelf and its overall integrity. They conclude that it is likely that the ice shelf will detach from the pinning point within the next decade. They hypothesise that detachment of the pinning point will lead to acceleration upstream of the grounding line and could lead to the disintegration of the Western Glacier Tongue.

Given the importance of the Amundsen Sea Sector for sea-level rise over the coming decades and centuries, I find the topic highly relevant to the cryospheric community. The paper is well written and Figures are appropriate. I also want to commend the authors on utilising different complementing methods to provide such a detailed analysis. This is certainly a strong point of the study. The study also highlights the importance of pinning points either as ice rises or rumples.

I support publication in TC. I have, however, a few general comments concerning the ice-sheet model inversion, and a number of detailed comments that should ideally be addressed. I hope the authors find my comments helpful.

Specific comments:

Main concerns:

1. In my view, the presentation of the numerical inversion part of the study is relatively weak in its present form, primarily because many details are omitted. This section certainly needs more explanation and I must admit that I am not really convinced of the basal traction field that the authors provide in the supplement (Fig. S5). I

am happy with the fact that the authors describe the ice-sheet model in brief without going into to much detail about the underlying equations, but nevertheless the basic setup including parameters and boundary conditions should be provided. I find the description of the inversion and model insufficient.

- You are saying that you are using SSA to invert for basal friction and then solve BP and FS in a diagnostic run (L214-216). Do you actually use different stress models in your diagnostic runs? This is never mentioned again. If you do use different stress models for inversion and diagnostic run (e.g. SSA for inversion and FS for perturbation), why should this be justified? I found in my work (admittedly in forward simulations) that using SSA for the inversion and FS for a forward simulation certainly results in a larger model drift. The reason being that you are simply not inverting for the same stress model.
- I am a bit concerned that you overfitted your data during the inversion. Your Fig. S5 looks very patchy, so from my experience this is what basal traction inversion fields look like after a few iterations (not converged yet) or if you do not regularise enough (or at all?). In any case, you need to add what kind of regularisation you use and also add an L-curve analysis to convince me that you are not overfitting (see Fürst et al. 2015 or Berger et al. (2016) as an example). A 2D plot of the mismatch between modelled and observed velocities would also be appreciated. This can be added to the Supplementary material.
- I am also wondering why you are just inverting for the basal friction coefficient, but not for ice viscosity at the same time which is actually pretty standard these days when using SSA (see for example Fürst et al. 2015, Cornford et al. 2015). This would also help to identify regions of shelf weakening in response to your pinning point being there (see Berger et al. (2016)) and also how these regions have changed as the size of the pinning point has changed.
- Please also add what kind of ice temperature you employ? Is your setup isothermal? What is your rate factor? All these things are needed for a complete description of you setup.

Technical corrections:

Abstract:
L5 I would change "model perturbation" to "ice-sheet model perturbations" to be more precise

L43 ...emergent ice rise ... This could use a citation

L72 Similar to L5, I would suggest to change it to "ice-sheet model perturbation experiments"

L86 How come the Jordan et al. (2020) map has resolution of 1000 m, but you say in L82 that airborne gravimetry data cannot resolve anything below 5000 m?

L97-105 How do you ensure that you have a smooth transition (no jumps) between the region where you adjust the bathymetry and the background bathymetry?

L130 Equations: Here you write explicitly that **w** is a function of space. Either you do this everywhere (Eq. 4 and 5) or you drop it in all equations. Just be consistent. I think I would prefer the former but leave this to the authors to decide.

L135 I think it would really help to add a table with symbols/constants and values used in the study. Coming from ice dynamical modelling, the letter **q** is reserved for ice flux, so I think adding a table with all symbols and constants would be beneficial. You could then also drop the values in the main text.

L155 Is there also an uncertainty associated with REMA?

L178 How do you get the uncertainty in grounding-line retreat rates? Does that not depend also on the geometry of the region (e.g. less inclined bed being more uncertain?)?

L191 Again, can you briefly say how you arrive at these numbers?

L213 Shouldn't that be "Shelfy"?

L223 How can your mesh be 100 m resolution, if you are using mesh adaptation? I think you should move this to the end of the previous paragraph (L218) and give a max/min resolution range.

L224-225 I would delete the sentence starting with "We set grounded ...". You could set it to 1000 m/yr, but if you are running diagnostic experiments without geometry updates, this does not matter.

L230 I think here or somewhere nearby, it would be good if you could add what kind of boundary conditions you are applying in your experiments.

L254 How do you compute grounding-line retreat rates of ice rises? Is this a maximum estimate or a mean estimate? Would be good to add this.

L311 If I understand the reported data here correctly (8.0 HAF and 0.3 m/yr thinning), I find your formulation in the abstract and conclusions a bit too certain ("will unpin in less than a decade"). I think based on the extrapolation of your data it's more likely

to happen within the next 20-30 years. So I would suggest to add a qualifier to these statements along the the lines "with the potential to detach within the next decade"

L313-315 To interpret this we certainly need for information on regularisation etc. See main concerns.

L320 What is "the isotropic consideration"? Do you mean that your rheology assumes that ice is isotropic?

L324 You need to say somewhere what stress model you use for you diagnostic simulations, but ideally before in section 2.5

L325 Here you introduce the acronym ISSM, but you have used this already before. Please make sure that you introduce acronyms at first mention. I am not sure exactly what the TC policy is, but REMA and I believe MEaSUREs were never introduced. I am familiar with the data products, but I am not sure if journal policies require you to introduce these as well.

L336-337 Based on the evidence that you show, we know that the direction is correct. What about the magnitude? I think adding a 2D velocity magnitude difference plot would be helpful.

L337 When you remove the Western Glacier Tongue, do your boundary conditions change? What boundary condition do you apply where the Glacier Tongue was removed?

L348 Are you still using the same basal friction field or did you rerun the optimisation with the updated velocity dataset? If the former, please add a point of why you think this is justified?

L375 What are "fast horizontal gradients?" Do you mean large? I do not think gradients can be fast.

L377 I just do not quite follow, how can reduced lateral drag result in slower ice velocities? Could you please explain this a bit more?

L387 Why can it not be thicker ice that is being advected? Is there no evidence of thicker ice upstream?

L401 I would change to ...if no other forcings are changed such as ..."

L416-417 The reported limits of the basal friction coefficient? I do not understand this sentence. Also as mentioned previously, you could incorporate anisotropic effects for the ice shelf into the model the same way you do for the basal friction coefficient, if you also invert for ice viscosity.

L432 I agree that it is always good to have multiple complementing methods. However, I feel that it should be mentioned somewhere that your flotation assumption is violated at the grounding line and hence will never give you the "true" grounding line location. Also I found that height above flotation is also quite sensitive to density estimates of the ice column and ocean.

L476 delete vigorous

L488 I would add a qualifier here: "could reach flotation ..."

Figures:

Fig. 2: Could you indicate ice flow direction also in panel (a). The dashed white line is also not visible in the legend

Fig. 3: How did you compute retreat rates? Along flowlines? Maximum retreat? Especially retreat for region IV in panel (c) looks strange. I am also not sure if panel (a) is needed. You could potentially delete this and change the layout to Fig. 5

Fig. 6: The letters U and D are hard to read. Can you change them to black font colour?

Fig. 7: I like these 3D Figures. But can you add either a flow direction arrow or indicate in the caption that 3D Figures are rotated by 90 degrees? Or if this is possible, have the same orientation as all your 2D maps?

Fig. 9: I can hardly see the red region. Since your colour scale includes red, please change to something different.

Sincerely,
Clemens Schannwell

References

Berger, S., Favier, L., Drews, R., Derwael, J., and Pattyn, F. (2016). The control of an uncharted pinning point on the flow of an Antarctic ice shelf. Journal of Glaciology, 62(231), 37-45. doi:10.1017/jog.2016.7

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Fürst, J. J., Durand, G., Gillet-Chaulet, F., Merino, N., Tavard, L., Mouginot, J., Gourmelen, N., and Gagliardini, O. (2015). Assimilation of Antarctic velocity observations provides evidence for uncharted pinning points, The Cryosphere, 9, 1427–1443, https://doi.org/10.5194/tc-9-1427-2015