# Response to Reviewer 1 (Tyler Pelle)

**In this work, Joughin et al. investigate the impact of using both Weertman-style and regularied Coulomb friction law, in conjunction with a linear scaling to the associated basal shear stress field for each law that enhances bed weakening with proximity to the grounding line, on 200-year simulations of Pine Island and Thwaites Glaciers. In addition, this paper also compares the response of these glaciers to an ensemble of randomly generated ice shelf basal melt volumes. The authors find that the choice of friction law has a relatively minor impact on ice volume changes of this sector and recommend use of a regularized Coulomb friction law when only one friction type is used. In addition, parameterized bed weakening led to significant enhancements in the 200-year global sea level contribution of this region, highlighting that such weakening should be included in future ice sheet model simulations either through this explicit manor, or through an effective pressure dependance. Lastly, the sea level response of the system was found to have a strong linear dependance on the total integrated ice shelf melt volume through the 200-year simulations.**

***Summary so no response.***

**I have to admit that I had a bit of a tough time working my way into this paper because the basal friction overview is quite long and involved, which I think might be a bit much and could be simplified to only include information that is needed to support the results/discussion/conclusions. I also found that visualizing differences in some of the figures was challenging because of the use of different y-axes limits, many of which I think can be made consistent. However, once into the results, I found this work to be an absolute pleasure to read and is full of really wonderful conclusions and insights that would be of wide interest to both the ice sheet modeling and broader scientific communities. It is also clear that this manuscript has been built on a long line of research that the authors have been working on for quite some time, so it is great to see everything come together in such a wonderful way. Below, I provide a number of suggested edits the authors can make to improve the manuscript, most of which aside from restructuring are small-technical corrections that should be easy for the authors to address. Due to the restructuring of the beginning, I suggest major revisions; however, I am very supportive of publication once these comments have been addressed.**

***This summarizes comments that we handle in detail below.***

**General Comments:**

**● Basal Friction Overview section: This section is quite long and I have to admit that I got**

**a bit confused reading through it, which was challenging because it sets the stage for the rest of the paper. There's a lot of analysis and equations presented and I am wondering if it is possible to shorten this section to only what is necessary for interpretation of results/conclusions? After reading through the paper, I think the critical information needed in this section is that** ***a linear scaling that depends on a height above flotation threshold h_{T} (found in previous work to be ~41-46 m for PIG) is applied to \tau_{b}, which is solved for via both RCF and Weertman-style friction law. In this paper, we will investigate how variation in h_{T} applied to both the BCF and Weertman sliding laws, as well as ice shelf melt, impacts future ice loss of PIG and THW.***

> ***To some extent this section is review, but it also provides context for how our implementation relates to other implementations. Since there are a lot of regularized Coulomb friction sliding laws, it is important to examine what the properties are and why they make a difference. As we demonstrate, these laws only provide Coulomb behavior over a small part of the domain (<1%). The real difference they make has more to do with the assumptions made about effective pressure and how that leads to a weakening of the bed as the ice approaches flotation. It's there for the interested reader, but this part can be bypassed by those not interested. We refer back to much of it in the discussion.***
>
> ***We will do another editing pass to see if it can be word-smithed to more digestible.***

**Given that, I think both friction laws that are used in this paper, as well as the linear scaling, should be provided, and other information should be limited as needed for clarity. For example, there are a few forms of Weertman sliding laws presented (eqn. 1, 6) and I am not sure which one is used in the simulations.**

> ***For Weertman we use Equation 1. To make this clear, in the modeling description we added.***
>
> ***"For both the RCFi and Weertman (Equation 1 with $m = 3$), we scale the basal shear stress by $\lambda(h)$, using a range of $h_T$."***
>
> ***The point of introducing Equation 6 is to remind the reader that effective pressure can also be included as part a Weertman type sliding law, causing a similar type of weakening as Equations 3&4.***

**Also, the friction law that combines Coulomb and Weertman (eqn. 4) is not used in the rest of the paper, but stimulates quite a long discussion about h_{f} transition points (L81-89) and effective pressure (L91-109; which is not used in either of the friction laws used in this study since it is subsumed into the friction coefficient solution). While I think this information is really fascinating and I think the authors did a great job on the analysis, I unfortunately don't think it is appropriate in its current place in the manuscript and suggest the authors revise this section and shorten it considerably. Perhaps a lot of this can go into an**

**appendix, with information in the main manuscript saved for only what is most pertinent?**

*We disagree because it's important to demonstrate what the friction law we use has in common with other regularized Coulomb friction laws and how they differ. While these equations have been published elsewhere, we feel this section puts them all in the appropriate context.*

*In general, all of the equations are based on sound theory and are affected by (or motivated by) the effective pressure. If we knew the effective pressure everywhere, things would be much better. But since we don't assumptions are made, leading to different behaviors. We feel its worth spending a few words to explore the implications of those assumptions. There are 6 references to Equation 4 scattered throughout the paper.*

- **Consistency of language and figure axes in manuscript: I noticed many different forms of "PIG and Thwaites Glaciers", "PIG and Thwaites glaciers", and "Pine Island and Thwaites glaciers". In my opinion, I feel like the "PIG and Thwaites Glacier" would be the most correct, but I am very happy for the authors to choose their favorite variation and use only it throughout the main manuscript, supplement, and in figures and associated captions. I pointed out a few places in the manuscript where I noticed this, but I likely did not catch them all, which is why I raised it as a general issue.**

*Good suggestion. We changed the text to remove "PIG and Thwaites glaciers". Now we use either "Pine Island and Thwaites glaciers" or "PIG and Thwaites Glacier" as either is correct. We also removed instances of ...Thwaites... and replaced them with ...Thwaites Glacier.... (there were a couple of instances where Thwaites Glacier was too much to fit in the legend).*

**On a similar note, I also noticed many variations of y-axes limits on figure panels that could be made consistent across the entire figure. This could help visualize differences in figures where there are many intersecting lines and many panels.**

*Some of these we were able to harmonize as suggested. Others we could not either because it compressed the data in some too much (not much point in comparing between plots if one can read individual plots) or it caused problems with legends.*

**Line Comments**:

- **L8: Specify ice shelf basal melt here so readers know you are not referring to melt of grounded ice.**

*Done.*

- **L11: Change "above" tp "upstream of"**

  *Done.*

- **L14: remove "work" – i.e. "our simulations suggest"**

  *That wording was grating! Done.*

- **L26: "continued melt forcing" – I think you are referring to ocean induced ice shelf basal melt based on the citations, but I think it would be helpful for readers if you were explicit about this here. Perhaps here you can say " . . . continued ocean forcing in the form of ice shelf basal melting (hereon referred to as melt)."**

  *Done*

- **L33-34: I think it would improve readability if the definition of variables (\tau_{b} and u_{b}) is confined to the "Basal Friction Overview" section. 2**

  *Done*

- **L40: By models, do you mean basal friction parameterizations? If so, please specify because you use "models" twice in this sentence that means two different things.**

  *Agreed. Done.*

- **L50-54: As per line comment L33-34, I think this would be a perfect place to introduce variables \tau_{b} and u_{b}.**

  *Done.*

- **L51: Is this the Weertman sliding law used in the simulations, or is it eqn. 6?**

  *Yes. In response to reviewer 2's comment, we made this clear in the model description.*

- **L70-90: I think there is a lot of interesting information here but it is quite lengthy. If the main point is that we expect Coulomb sliding behavior near the GL and at some transition point, we expect Weertman sliding, maybe this can be said more succinctly with less analysis? This seems very in-depth for a section that is not either the results or discussion.**

  *We refer back to this section multiple times in the discussion where we describe our work relative to other sliding laws. As described above, we feel it provides an important context with which to frame our results. Other than minor word-smithing we have not changed this text.*

  *We feel it is important to emphasize the extent to which Coulomb conditions occur (<1%) given Equations 3&4 are referred to as regularized Coulomb* *friction. (Granted our version of RCF and RCFi are just over 10%, but they do provide Coulomb behavior over all the fast moving areas).*

- **L81-89: I have to admit that I got a bit lost with some of the analysis here. In particular, I am a little confused about where the value of 45 m came from and why this is only computed for the near GL region (Fig. 1a)**

*To make this more clear, we reworked to*

*"In these examples, Weertman conditions are found everywhere except for the case where Equation (3) is plotted using a height above flotation ($h - h_f$) of 45 m (Figure 1a)."*

*The value 45 m was selected to show the transition from Coulomb to Weertman. We updated the plot to include 40 m as well, by which point the equation yields nearly full-on Coulomb conditions. We also removed the Equation (4) curves since they only show Weertman in these examples.*

**- can a similar value be computed and added to the plots for trunk (Fig. 1b) and inland tributary (Fig. 1b)?**

*No, because these points are meant to represent inland conditions where Coulomb conditions will never occur (unless the ice thins by several hundred meters).*

*We tried to make this clearer by splitting the paragraph and starting with*

*"The reason why these plots largely reflect Weertman sliding is that the transition from Coulomb to Weertman conditions in Equation (4) occurs at $h - h_f = \frac{\tau_b}{\alpha^2 \rho_i g}$, with Equation (3) producing a less abrupt transition at a similar value. Thus, if we assume ~300 kPa as the maximum expected value for $\tau_b$ with Coulomb friction, then the transition to Weertman sliding takes place at locations where the elevation is less than ~67 m above flotation for $\alpha^2$=0.5."*

**Also, in figure 2, are you plotting h-h_{f} as per the equation at the end of line-83? If so, this should be mentioned in this paragraph and also in the figure caption of figure 2. For figure 2, why are the values of h-h_{f} (1, 41, 86, 176) chosen?**

*First, 176 should be 172, which we have fixed in the new plot. Since it is felt that the text is already too long, rather than explaining this in the text, we added the following to the figure caption.*

*"Note the contour values correspond to the value of $h_T$ used in our simulations."*

- **L93-95: There are recent modeling efforts that model upstream Thwaites/PIG effective pressure with subglacial hydrology models (e.g., Hager et al., 2022; Dow et al. 2023) that show low effective pressure far upstream of the grounding line - I**

**would recommend citing them here since they support your claim. It is difficult to know how accurate these model simulations are, but they are likely the best we can do at this point!**

*Thanks for pointing us to these references, which we were unaware of. We have cited them as suggested.*

- **L129: "Close to the grounding line" is defined as h-h_{f}<41, but figure-2 shows that there are numerous regions where the contour of h-h_{f}=86 is nearly superimposed onto the line h-h_{f}=41; why was the value of 41 m chosen?**

*The 41 m value is based on our preferred hT value, which I hope is more clear since we amended the caption.*

*We amended it to "The results show that in the band closest to the grounding line"*

*We are discussing the integrated area between contours, so the fact that the contours are closely spaced in regions doesn't change that.*

- **L135: When you say "as the surface elevation approaches flotation", are you referring to in figure 3 when h-h_{f} approaches 0, meaning when h=h_{f}? In line 78, you defined h as the ice thickness, so is surface elevation accurate? Perhaps just saying "as ice approaches flotation . . ." would be more clear?**

*That was a typo in 78, it should have said elevation not thickness, which we have corrected.*

- **L140-145: While I think it is obvious in eqn. 8, I think it should be reiterated that this linear transformation applied to \tau_{b} evolves in time to the changing ice thickness in your simulations.**

*Changed* **to "…***function produces linear weakening as the surface elevation evolves time similar…"*

- **L161: You have two section-2's (the friction overview and methods section).**

*Fixed. My bad, but it would be nice if they made the template auto-number the headings.*

- **L177: Need a new paragraph space between these paragraphs**

*Done. Another thing a properly structured template should have done by including space rather than needing a blank line.*

- **L178: Do you mean Equation 7?**

*Fixed.*

- **L186: I think it is worth mentioning that your computed fields for the friction coefficient and A do not change in time.**

  *"Both A and $\beta^2$ remain constant with time throughout each simulation."*

- **L197: Is the ice front fixed (i.e. no calving is simulated)? Is this a major limitation given that your 2021a work indicated that ice shelf retreat was the primary driver of recent speedup of Pine Island Glacier? If so, I think this should be mentioned somewhere in the text.**

  *It's mentioned in the abstract. "Based on recent estimates of melt from other studies, our simulations suggest that melt-driven combined sea-level rise contribution from both glaciers may not exceed 10 cm by 2200, though the uncertainty in model parameters allows for larger increases. We do not include other factors, such as ice shelf breakup that might increase loss, nor factors such as increased accumulation and isostatic uplift that may mitigate loss."*

  *And the conclusion "While we can't account for other factors that might increase ice loss, such as full ice shelf breakup (MacAyeal et al., 2003) or partial shelf loss (Joughin et al., 2021a), our results suggest some bounds on melt-driven losses from PIG and Thwaites Glacier over the next two centuries likely will not exceed 10 cm."*

  *We did add:*

  *"The domain extent is fixed and the ice-shelf front does not move, but the grounding line evolves freely."*

- **L199: I'm just noticing this now, but I think it would be more correct to say "PIG and Thwaites Glacier" since PIG stands for Pine Island Glacier. See general note about consistency through manuscript.**

  *Done.*

- **L208-215: This compiled velocity map is very interesting, it would be great to see a figure of it somewhere in this paper! Perhaps the same can be done for the SMB and combined into one figure?**

  *As a figure, it wouldn't be notably different than the plethora of velocity maps already published.*

- **L220: Does SMB vary in time in the simulations?**

  *Added ...(Medley et al, 2014), which does not vary with time."*

- **L240-245: Another interesting pattern in figure-5 is that at higher melt rates (panels c/d), corresponding Weertman and RCFi mass loss time-series seem to align more than for lower melt simulations (in the top two panels, the Weertman**

**lines are all clumped above the RCFi lines). That is, it seems like the choice of sliding parameterization becomes less important when the system is forced with higher melt rates. Do you agree? This could be an interesting point for the discussion.**

*We dug into this a bit deeper prompted by reviewer 2's comments.*

*"As noted above, there are limited areas ($h_o < h_T$) where the bed can strengthen if thickening rather than thinning occurs. Such thickening rarely occurs because the region near the grounding line tends to nearly always thin. For some Weertman cases, however, thickening and advance do occur for sufficiently low melt, which should be reinforced by thickening-induced strengthening of the bed near the grounding line. This would explain why the losses decline as $h_T$ increases for the low melt Weertman cases on PIG, since the area subject to this type of strengthening expands. Whether this should remain a feature of our model is a subject for future research.*

*We think this addresses the comment here.*

- **L250: Please change "Thwaites" to "Thwaites Glacier" here and also through the manuscript. Also, there are times when you capitalize "Glacier", and when you keep it lowercase (i.e. Thwaites glacier, L261) - I think it is correct for it to be capitalized, so please make that consistent throughout the paper as well.**

*We have changed as described above. Concerning capitalization, we use either Thwaites Glacier or Pine Island Glacier, but Pine Island and Thwaites glaciers when both are referred to by their full names.*

- **L263: Change ". . . sensitivity for PIG is -0.24 to -0.51 mm Gt^{-1}yr sle for PIG and . . ." to ". . . sensitivity is -0.24 to -0.51 mm Gt^{-1}yr for PIG and . . ." (you said "For PIG" twice here)**

*Done.*

- **L270: Is the unit supposed to be "a few hundred m/yr" or "a few hundred m", which seems more in line with the values in figure-8.**

*It should be "m", error due to force of habit for someone who works with velocity a lot.*

- **L277: Change "above the grounding line" to "upstream of the grounding line" here and throughout the manuscript.**

*Done.*

- **L281: The notation “Figure 9e&f” seems a little messy, perhaps could use “Figure e/f”? I think this is personal preference and maybe a little pedantic, but I have not seen the “&” symbol used in this manor in a manuscript before.**

  ***“&” means “and” and is commonly used by journals in this way. We can’t find an EGU style guide to resolve the issue, so will leave it to the copy editors.***

- **L287-289: It would be nice to see what some of these melt distributions look like since they seem like a primary control on the spatial distribution of retreat of Thwaites Glacier.**

  ***Rather than including examples here, we added: “(see example profiles in Joughin et al, 2021b).”***

- **L317-318: I would even say that simulations with greater h_{T} values lose less mass than those with higher h_{T} values for these PIG simulations, which is pretty fascinating! I really enjoy your call to earlier work here that investigated this paradox.**

  ***We are taking this as a comment and not a change request.***

- **L346: I’m not sure if it is just the way my computer rendered the PDF, but it seems like there is an unnecessary space in the word “ri ght”, but I’m not sure if this is a typo.**

  ***Weird. It looks fine in the Word doc.***

- **L350-357: I really enjoy this discussion and am excited with the prospect of future work that compares such implementations of bed weakening near the grounding line.**

  ***Thanks.***

- **L359-374: I understand the reduction of section “Basal Friction Overview” might cause issues with this and the preceding discussion paragraph, but I think if you put a lot of the**

  **“Basal Friction Overview” information in an appendix, you can still keep these paragraphs (with references to the equations in the appendix), which I think are very important findings and thoughts from your work.**

  ***For the reasons discussed above, we think the text is best left where it is.***

- **L377: I’ve noticed a few variations of “PIG and Thwaites Glacier” (L377), “PIG and Thwaites glaciers” (L261), and “Pine Island and Thwaites glaciers” (L770). Please pick one and keep consistent throughout the manuscript, supplement, and all figures and figure captions.**

*We have made the capitalization consistent as suggested. Stylistically, we prefer to alternate in places between "PIG and Thwaites Glacier" and "Pine Island and Thwaites glaciers".*

- **L406-417: See note for figure 10 below - In short, I would recommend a different way of showing these results. Given that this paragraph focuses mainly on the associated r^{2} values, I'm wondering if figure 10 could be consolidated into a table?**

*We feel it's better to display the results graphically.*

- **L427-429:I got confused by the phrasing "results lie along a line" and "results that fall well off a line"; it took me a second to realize you are referring to a linear regression line. Maybe rephrase to clarify.**

*Reworded. "For example, plotting results from multiple studies as shown in Figure 10 would help differentiate the cases where different models produce results consistent with the level of melt forcing (e.g., the results lie along a linear regression line with high $r^2$ near 1) from those where the differences are due to some other aspect of the model (e.g., results are not explained well by a linear regression to melt)."*

- **L430-432: Seroussi et al. (2023) found that treatment of ice dynamics was the main driver of uncertainty in the ISMIP6 ensemble through 2100; however, this was across the entire AIS. In line-431, maybe specify that you are only referring to the ASE (i.e. ". . . suggests that differences between models in this sector may largely . . .").**

The existing sentence did refer to the ASE, but we made more clear with

*"For example, the fact that melt is the main predictor of loss in the Amundsen Sea Embayment for the suite of ISMIP6 models (Seroussi et al., 2020), suggests that much of the difference between models in this region may be due to how they treat melt, as opposed to differences in their treatment of ice dynamics.*

- **L440: This is true for the ASE, but would these conclusions hold for other sectors of Antarctica? If you are not sure, it might be worth specifying here that you are referring to coupled ice-ocean models of the ASE.**

*We don't want to clutter the discussion with more detail on this matter. But since you are interested, see Figure 5 in Joughin et al., 2021b.*

- **L502: Consider rewording: " . . . our results suggest that melt-driven losses from PIG and Thwaites Glacier over the next two centuries likely will not exceed 10 cm."**

*Softened it a bit. But we are clear this is not all source of loss, only melt-driven. It now reads.*

*"While we can't account for other factors that might increase ice loss, such as full ice shelf breakup (MacAyeal et al., 2003) or partial shelf loss (Joughin et al., 2021a), our results suggest melt-driven losses from PIG and Thwaites Glacier over the next two centuries may not exceed 10 cm. Two centuries out, however, both glaciers will have lost a substantial amount of ice and will be primed for much more rapid loss if melt rates don't subside. "*

- **L511-512: Please remove hyperlinks – also, I think "comit" should be changed to "commit" in this section.**

*The http addresses are needed. The template is making them hyperlinks, which should get taken care of when the final ms is typeset (some journals seem to keep them as hyperlinks).*

- **L797: Magenta box in panel-a denotes domain for figure 9 (not figure 10)**

*Fixed.*

**Figure Comments**:

- **It can be quite challenging to compare VAF losses between panels in figures 4-7 since the y-axis limits are all different. Where appropriate, can you please use the same y-axis limits? I think this would be very helpful for all panels in figures 4-6, as well as panels B/C of figure 7. Otherwise, all of the curves look fairly similar and it can be difficult to visualize differences between them, which I think is the ultimate goal of these figures.**

*We made some changes, but not all of them. The figures have several lines, which become very crowded when using the max y-range for all plots.*

- **Figure 2: Perhaps it was said in the main text, but it would be good (in the figure caption) to reiterate the friction law that was used to compute the basal shear stress that is shown in panels-A/B.**

*It shouldn't really matter since the RCFi and Weertman should yield the same basal shear stress within minor numerical differences. But we did change to "basal shear stress ($\tau_b$ from RCFi inversion)".*

- **Figure 3: The text and lines in this figure are a little blurry and/or choppy (as with figure 1 as well). Will you output these as PDF's in the final submission?**

*The original figures look great, but we did try to increase some fonts and thicken some lines. I think we provide original figures instead of a word doc for the final.*

● **Figure 8: It doesn't seem like two color bars are needed, but rather you could use one that diverges at 0, with blue (negative) trailing off to the left, and red (positive) trailing off to the right. Also, would it be possible to scale the intensity of the blue so that it uses the same color scale as red (i.e. reaches maximum intensity at -500 m). This should result in very pale shading of blue where you have height above flotation gains, which would seem more appropriate since now, it appears like the gains in the bottom and upstream parts of the domain are stronger than the losses Thwaites Glacier experiences, which is not true.**

> ***The thickening is an order of magnitude less than the thinning, so we did it this way so as not to bury any thickening in the color table. We assume the reader can take the different scales into account (having two color bars sort of forces them to notice the scale difference).***

● **Figure 10: I believe I understand that the main point of this figure is to show that the linear relationship between total integrated melt volume and VAF change holds for various other melt parameterizations, but I think the number of different-shaped symbols is very distracting and is rather uninterpretable for the reader. Many of the symbols are clumped over each other at lower x-values and also, the meaning of the different symbols is not explained in the main text (i.e., I know the different symbols are different melt function outputs, but I have no idea what "mr_1", "mr_4", "80_700", etc. are specifically), and I don't think readers should have to read the supplement to interpret a main-text figure.**

> ***Issue 1), too many symbols. There are a lot, but we feel it's important to show the scatter of the full range of models (using the same scale as requested above would only make the issue worse). For the most part, they are distinguishable, and when they are crammed together it indicates nearly the same result. We wanted to clarify that the linearity with melt is not some fluke of how we normalized melt in the ensemble simulations.***
>
> ***Issue 2), melt functions in the supplement. We think the casual reader can interpret the figure shows a collection of depth-parameterized melt rates that produce a range of melt, which yield losses that respond linearly. Those really interested can see the supplement. And it's not without precedent, as we had to delve into supplements to get some of the parameters used to create this supplement figure.***

**Ultimately, I don't think the symbols matter given that the analysis is not dependent on individual melt functions, so I wonder if the authors can remove the 10 symbol-types and use just one symbol and the different colors to represent the different melt function outputs?**

*We argue one can ignore the symbol types and do this anyway. But if someone is really curious about a particular melt parameterization, in most cases they can find it using the symbols (or at least see it falls in a cluster)*

*.*

**I per line comment L406-417, I think this figure could also be consolidated into a table given that the main piece of information that is used from it are the r^{2} values.**

*Seeing is believing and we feel the regression lines plotted over the points makes a stronger statement.*

- **Figure S1: No change is needed in the text, but I am just curious what metric you use to prescribe your mesh resolution?**

*We plot the Firedrake function*

> `cell_sizes` [source]
>
> A `Function` in the $P^1$ space containing the local mesh size.
>
> This is computed by the $L^2$ projection of the local mesh element size.

*Which should give the long side of each triangle.*

- **Figure S2: In the figure caption, please include the full in-text citation for Barnes and Gudmundsson (2022).**

*Fixed.*