Author reply to Reviewer #2: 'Competing processes determine the long-term impact of basal friction parametrizations for Antarctic mass loss'

Author comments in blue

Preamble comment, also copied to the author responses to Reviewer #2:

We sincerely thank both reviewers for their constructive feedback and for suggesting valuable and thought-provoking points for improvement. However, the reviewers offer differing interpretations of the FEFI method, its results, and the conclusions drawn from our analysis. In summary, Reviewer #1 views the new FEFI method as a significant contribution to the paper that could be emphasized further but also notes instances where our results may have been overinterpreted. In contrast, Reviewer #2 agrees with the conclusions we reached based on our results but advises caution in how we present the FEFI method and initialization outcomes, warning that others might cite this work as definitive evidence of deformational flow at the Thwaites Glacier grounding line.

We agree with Reviewer #1 that, in parts of the manuscript, we may have overinterpreted our results. In our author response, we identify several sections where we will provide a more thorough description and analysis of the results and will moderate our conclusions. We also agree with Reviewer #2 that we do not wish for our paper to be cited as proof that the Thwaites Glacier grounding line is dominated by deformational flow, and we acknowledge the importance of clearly communicating the level of confidence in our inverted results. While we would like to retain these results in the main text, as suggested by Reviewer #1, we propose to include several critical notes to clarify the interpretation of the inverted parameters.

This paper presents modelling experiments that explore the 1000/2000-year simulations of Antarctic Ice dynamics under various laws the relate basal friction to sliding velocity and effective pressure. The CISM model used is well -known and has been tested through numerous community benchmarks. It uses a nudging scheme to specify ice thickness, basal friction parameters and other hard to obtain parameters, which looks to work well in general. The overall conclusion is that the gross outcome in terms of sea level rise can be independent of the choice of basal friction law in some circumstances, but strongly dependent under others. I think the authors are correct to reach this conclusion, but have some reservations about some of their simulations.

General comments

The 'DI' experiments are credible and can support the main conclusions, but I think the 'FEFI' experiments are not publishable (yet).

DI experiments. These are the 'standard' simulations, using the mature/ well-known tuning methods associated with CISM, where a basal friction coefficient C(x,y) for each friction law is estimated b to bring the ice sheet thickness into line with observations, and to avoid drift. Although these produce similar VAF(t) for each friction laws, the authors demonstrate that this is a case where differing dynamics adding up to quite similar gross outcomes. Fig 10 in particular shows that the Zoet-Iverson Coulomb limited friction law simulations involve more buttressing (and less basal friction), and the Power law simulations show the opposite behaviour. No doubt with sufficiently high (unrealistic>?) melt rates, the Zoet-Iverson simulations could be denied the buttressing too, and the gross outcome might then differ more. At this point, the authors can conclude that the choice of friction does matter (but you need to look at the detail to see that)

FEFI experiments. These use a modified / novel tuning method, where an additional flow enhancement factor E(x,y) is estimated, to bring the model in line with observed velocity in additional to the DI constraints (where E = 1). This is a good idea, and indeed many groups find they need to estimate E(x,y) at least in ice shelves. As the authors note, this brings a new level of underdetermination to the estimation problem, and scepticism about the results is required. So far, I have no objection.

However, the process here differs from more typical cases in that it is sensitive to the velocity, rather than to horizontal parts of the strain rate. As a result, the interaction between C and E in the tuning process is different and appears to have produced (as the authors note) radical results that are at odds with convention. Nothing wrong with that, but looking at Fig 6, the outcomes are difficult to accept. The enhancement factor itself shows blocks of much reduced / much increased effective viscosity, and that results in (for example) flow which is dominated by SIA-like internal deformation of ice in the trunk of Thwaites glacier. It is true that some authors find that the SSA is inadequate here, but SIA would be a volte-face.

To be fair to the authors, they are not claiming that their FEFI results are plausible, just they demonstrate sensitivity to underdetermined parameters. Hard to disagree! But a primary result in a glaciology (as opposed to an inverse problem paper) should be citable, and I don't think we would be happy to see future papers citing this paper as evidence that Thwaites glacier is well described by SIA Possibilities to address this?

1. Remove the FEFI results and form the natural conclusions from the DI results. Place the FEFI results in a supplement and make a note in the main text.

2. Argue that the FEFI E(x,y) results are credible (a tall order, but possible)

3. Improve the FEFI procedure so that it does produce credible E(x,y) (e.g by nudging to match horizontal strain rates rather than surface velocity?) – but I think that could be a second paper

We thank the reviewer for raising this important and thought-provoking discussion, for offering constructive suggestions on how to proceed, and for supporting our conclusions. We agree that the FEFI results regarding deformation and sliding are unrealistic, and we certainly do not want our study to be cited as evidence that "SIA-like flow can be expected at the Thwaites Glacier grounding line." However, due to the lack of direct observations in the region, and considering that other studies (e.g., McCormack et al. (2022)) indicate the deformational regime is at least heterogeneous, we were hesitant to entirely rule out deformational flow at the TG grounding line based on physical intuition alone. Without sufficient (observational) evidence, such exclusion would also be not justified in our opinion. Still, there is value in exploring FEFI as a what-if scenario that cannot be completely ruled out, as it is common for models using data assimilation as an initialization technique to alter their flow enhancement factor. The features that we found could be present in those models as well.

We want to make it clear that our goal is not to produce the best possible inversion outcome, nor do we want to deliver a state-of-the-art projection (e.g., we do not use a realistic climate forcing). Instead, our aim is to demonstrate that two equally valid inversions (with different advantages and weaknesses), which result in similar present-day ice sheets, can lead to very different responses in an unforced future simulation.

Regarding your option 1) (Remove FEFI from manuscript except some side notes), we cannot maintain our current results and conclusions - which we're pleased the reviewer supports - if we rely solely on DI simulations. The FEFI simulations are essential to our analysis. Hence, we would like to keep those in the main text rather than moving them entirely to the supplementary, which in fact may still lead to a deliberate misinterpretation. As for option 2) (Argue that FEFI results are credible), we agree that this would be a substantial undertaking, and we would prefer to avoid it due to the risk of the paper being misinterpreted as evidence that TG exhibits SIA-like behaviour. Finally, regarding option 3) (Improve the FEFI procedure), we agree this would be an excellent direction for a future study, including further improvements to the FEFI method, such as possibly advecting ice properties along flow lines. However, it is indeed not realistic to include this in this manuscript.

Therefore, we agree with the reviewer that we should clearly state that we do not interpret the deformation-dominated ice flow for Thwaites Glacier (TG) (or the inverted flow enhancement factor more broadly) as a physical property of the ice sheet. Instead, we view *E* as a tunable parameter used to better match present-day conditions, which simultaneously increases the underdetermined nature of the inverse problem. The purpose of introducing this new inversion, alongside the DI approach, is to generate an alternative solution to demonstrate that the choice of initialization method is the primary driver of divergent future projections in our simulations—and to offer a physical explanation for why this is the case. Following this line of thought, we will change our text.

We will rewrite the following paragraph L376-L386 with the following:
'This is particularly striking at the Western TG grounding line, where at present the regionally highest ice surface velocities are observed, which are unlikely to be solely by deformation. Sliding is expected to be the dominant regime of a fast-flowing (Antarctic) outlet glacier from standard ice flow theory, and the SSA is widely used as the appropriate stress approximation to model these regions (e.g. Bueler and Brown (2009); Brondex et al. (2019); Gudmundsson et al. (2023); Morlighem et al. (2024)). However, McCormack et al. (2022) modelled the ice flow regime more extensively and possibly more physically than we do, and found a heterogenous pattern of sliding and deformation close to the TG grounding line depending on the flow law used. Therefore, a mix of sliding and deformation cannot be excluded entirely (see Fig 2 in Mccormack et al. (2022)).

Because our model uses Glen's flow law (which was developed for isotropic, secondary creep) it cannot capture e.g. tertiary creep and ice damage accurately (Glen, 1952; Budd et al., 2013; Graham et al., 2018). By inverting viscosity, we are effectively compensating for this missing process, so the resulting flow-enhancement factor and inferred flow regime reflect model deficiencies rather than intrinsic ice properties. This is particularly true in regions with fast-flowing ice, because our FEFI initialization is only allowed to change the flow enhancement factor in areas where the modelled ice surface velocity errors exceed 25 m yr^{-1}. In addition, FEFI assigns ice properties to fixed grid cells instead of advecting them along flowlines, even though impurities, damage, and fabric anisotropy are fundamentally Lagrangian properties of the material. If the ice is damaged, it will remain so downstream of where the damage was initiated. The FEFI inversion can therefore generate physically questionable enhancement factors that mask upstream errors in the flow regime. For these reasons, we doubt that deformation dominates at the TG grounding line, since the current inversion cannot yet reproduce physically consistent ice properties. Observations could provide clarity on the flow regime of the TG grounding line. In particular, measurements in the vertical structure of the horizontal velocity in critical regions will help to distinguish which flow regime dominates.

Specific Comments

Abstract: obviously, if the paper is revised as I suggest, the last few lines are only weakly supported and so should not appear in the abstract.

We agree, and as replied to reviewer 1, we will remove 'The latter makes it difficult to base general claims on ice sheet modelling results'

L76 T = \beta u = … . The \beta u is not needed – many models do this as part of their implementation, but don't think \beta is mentioned again.

We will remove \beta_u

L95. Eq 1.4 is sometimes called a Budd law.

We will add to L93 '..and is referred to as the 'Budd' law after Budd et al. (1979)'

L114. In all four laws friction increases with speed, with diminishing returns (but tend to a limit in the ZI/Schoof cases)

We will add to L114, '..with diminishing returns.'

Figure 1. The asymptotes could be added to the figure.

Great suggestion, we will add the asymptotes (e.g. Cc*N) to the figure.

Eq 1.7 R_f rather than \Chi _f ? \Chi is dimensionless, but \Chi f is a stress (like the R components)

We agree that this avoids confusion and will change Chi_f to R_f in Eq 1.7 and 1.8

L141. \Chi is not a term

We will rewrite L141 to 'The variable \Chi in Eq 1.7 has a value of 0 for areas that are unbuttressed: the buttressing force then equals the driving stress if the ice sheet ended at that point with an ice cliff'

L145. Sorry, I don't see the logic here.

We will remove 'The former assumption implies that high basal friction just upstream of the grounding line, which will oppose ice flow, is interpreted as buttressing as well'

L148. 'Shelf kill'. In the interests of a less macho phrase, how about 'Shelf removal'. I know that shelf kill has been used elsewhere.

Great suggestion, we will replace 'Shelf kill' with 'Shelf removal' throughout the manuscript

L155. Purest -> simplest / most direct?

We will replace 'Purest' with 'simplest'

L160 – the whole paragraph refers to something in the supplement, but I don't think you need to further show the utility of these well-know buttressing indicators.

We would like to show that the buttressing indicators do not necessarily correlate for complex geometries such as the AIS. We will add to L163: 'little to no correlation could be found when the same method was applied to AIS ice shelves, as shown in Fig S9.'

Section 2.3 – this section seems a little disorganised. In particular, the nudging equations are introduced immediately before a general introduction to the nudging approach. The tables could be moved to an appendix since the parameters are usually defined in-text.

We will move Table 1 and Table 2 to the supplementary material. We will move L217-224 to L181, to start with a description of the nudging before introducing the equations.

L227 – I would like more detail (i.e math expressions) at this point.

We will add a mathematical expression as follows below L230:

$$\frac{dC_c}{dt} > 0 \wedge \frac{dE}{dt} < 0 \ . \qquad (1.21)$$

We will add to L229: (e.g. when modelled ice is too slow and too thin, the basal friction inversion will try to increase the ice thickness by decreasing ice velocities, and the flow enhancement factor inversion will try to increase the ice surface velocities, mathematically shown in Eq 1.21).

L265 – 'other factors' – e.g damage, fabric formation, errors in the temperature field

We will add '(e.g., damage, fabric formation and (local) anisotropy and errors in the temperature field)'

Fig 4 – could the panels be larger/split up?

We will include zooms here and larger figures in the supplementary materials. We will add the inverted ocean temperature perturbation of the whole ice sheet to the supplementary material, and refer to that in L332 and L353. We will include zoom-ins of the velocity misfit in the ASE region, including a discussion on the grounding line flux as mentioned in response to one of the major comments above.

Fig 10 – actually a more general comment – the shelf removal figures are the more useful in your text, whereas the buttressing number only helps you (vaguely) reiterate a known point about the two halves of Thwaites ice shelf / buttressing being greater near the GL. I would remove the buttressing number analysis, so that the right-hand panels of fig 10 can be larger.

We agree with the reviewer that our buttressing analysis with the buttressing number in the current version of the manuscript does not add new insights. We will reformulate our results somewhat in response also to comments from reviewer 1, to use the buttressing number solely for the purpose of quantifying ice shelf strength, and the acceleration number to quantify the basal friction strength. Then the buttressing number is explicitly used in the text to demonstrate differences in buttressing strength. We would then keep Fig 10 as is but rewrite L465-472:

'The right side of Figure 10 shows changes in ice velocity during the shelf-removal experiments at years 250 and 500, comparing Zoet-Iverson sliding (left column) and power law sliding (right column). Following the removal of the ice shelves, grounded ice in the Zoet-Iverson simulation accelerates rapidly—especially at the Thwaites Glacier (TG) grounding line and farther inland—much more so than in the power law case. Two primary mechanisms can slow down a retreating marine-terminating glacier like TG: buttressing and basal friction. At year 500, buttressing at the TG grounding line is stronger in the Zoet-Iverson simulation than in the power law case, as indicated by the higher buttressing numbers. Despite this, the acceleration response to shelf loss (right four panels of Fig. 10) is greater in the Zoet-Iverson case. This is because, in the power law case, basal friction increases with velocity (as shown in Fig. 1), limiting the glacier's speed-up. In contrast, the Zoet-Iverson simulation exhibits less frictional resistance and thus stronger acceleration. We conclude that, during the TG collapse, buttressing is the primary braking mechanism in the Zoet-Iverson case, whereas increased basal friction dominates in the power law case. Interestingly, due to the specific bed geometry of TG and the conditions in the unforced simulation, both scenarios produce a similar contribution to global mean sea level rise—though driven by different mechanisms. '

Fig 11. If you *do* include the FEFI results (I suggest not), then show both DI and FEFI at years 575 and 775 (i.e. four panels).

We will add two panels showing the ice thickness difference in DI at 775 years, and FEFI ice thickness changes at 575 years.

L580- 600 – clearly would be removed if you remove the FEFI material.
We would not like to remove the FEFI material, as stated above to the major comment, so we would like to keep this section in.

L602 – you could compare your E(x, y) with other model results.

We will add:

'Our results generally align with the heterogeneous pattern of deformational flow reported by McCormack et al. (2022) with the notable exception of a localized patch of deformation-driven flow at the Thwaites Glacier grounding line in our simulations. Barnes et al. (2021) examined the transferability of inverted parameters across three ice sheet models and found substantial variation in the inverted rate factor among them. Although a direct comparison with our inverted flow enhancement factor is difficult, since the rate factor also depends on temperature (see Eq. 1.16), the inverted rate factors in the Úa model (see Fig. 4 in Barnes et al. (2021)) vary by up to two orders of magnitude. This is consistent with the heterogeneous patterns we observe in our own results. Similarly, Brondex et al. (2019) show vertically averaged viscosity patterns with lower values near the grounding line compared to inland ice. This may be due to their assumption of applying only the SSA. While their results appear more homogeneous than those in Barnes et al. (2021), spatial variability is still present.'

L615 – I don't agree in this case because the other models mentioned don't differ from one another in the same way as the DI and FEFI models differ from one another. I am sure you are correct to say that differing initial conditions could explain any number of discrepancies between models, I just do not think the FEFI/DI contrast is representative.

We will rephrase L615 to: The results presented in this study illustrate why CISM can be initialized with more or less sensitivity to the choice of basal sliding law. Carrying out these types of experiments with other ice sheet models will enhance our understanding of why some simulations (Brondex et al., 2017; Sun et al., 2020; Brondex et al., 2019), are more sensitive to changes in basal friction laws than others (Barnes and Gudmundsson, 2022; Wernecke et al., 2022), and possibly lead to similar conclusions.

Technical Corrections

Eq1. Use f(x, y) notation to make spatially varying vs constant parameters clear?

We will add $C_c(x, y)$ to show the spatial variability of Coulomb C.

80 $\tan \phi$, not $tan \phi$. In a similar vein, there are frequent italic subscripts in equations that should probably be roman (e.g in eqns 1 & 2)

We will replace those with $\tan \phi$ and we will remove italic subscripts in our equations

L112 'asymptote' is not a verb (usually).

We will replace 'asymptote' with 'approaches a limit'

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