Review of van den Akker et al, TC MS egusphere-2025-441

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 FEI 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*

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.

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.

L95. Eq 1.4 is sometimes called a Budd law.

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

Figure 1. The asymptotes could be added 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)

L141. \Chi is not a term

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

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

155. Purest -> simplest / most direct?

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.

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.

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

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

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

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.

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).

L580- 600 – clearly would be removed if you remove the FEFI material.

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

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.

Technical Corrections

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

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

L112 ‘asymptote’ is not a verb (usually).