Review of Pang et al. (2026) 'ITMSL: an improved ice thickness inversion model integrating basal sliding dynamics for High Mountain Asia (v1.0.0)'

Summary

This paper presents a new ice-thickness-inversion model called ITMSL, and explores its performance across a dataset of 16 glaciers in High Mountain Asia compared to two existing thickness datasets inverted using different methods. The novelty of ITMSL is that it includes explicit consideration of glacier sliding, unlike most previous regional- or global-scale inversion methods that rely on the shallow ice approximation (SIA) and therefore neglect sliding entirely. The authors show ITMSL performs better than the reference datasets on the dataset included in this paper, and that it is generally less prone to producing extreme thickness values, as well as performing much better in faster-flowing areas.

I reviewed this paper when it was initially submitted and had substantial problems with the manuscript at that point, to the extent that I felt rejection was the best option as I thought that the amount of work required to revise the manuscript was too extensive to be completed within a reasonable timeframe. The authors have undertaken substantial further work on the paper and it is much-improved, but I still have some reservations, chiefly about how the results are presented and the effective-pressure parameterisation. I am now considerably happier that ITSML is a useful contribution to the field, so I congratulate the authors on having done the work to persuade me of that, but this could be presented much more effectively, still, I feel. Therefore, I recommend major revisions on this occasion.

Page and line numbers refer to those in the clean version of the submitted manuscript.

Major Comments

- Presentation of results: See below, but I think a lot of the results, particularly those based on glacier-mean quantities, don't present ITSML in the best light. I suggest that the authors consider presenting their results based on a split between slow- and fast-flowing areas, which is done at one point and is much more convincing in showing that ITSML is an improvement over other SIA-based models and doing something novel.
- Effective pressure: It's unclear in the manuscript which choice the authors have made for their values of N, but it is either set to be 0 or equal to the ice overburden, neither of which are very good assumptions to make for mountain glaciers. I suspect this will not, in the end, greatly change the results, but I think the authors need, at least, to clarify and justify which choice they've made and subsequently discuss its potential impacts on the results, or make a more sensible choice and re-run the model.

Minor Comments

- p.1, l.21-22: Repeat of the preceding sentence?
- p.2, l.53-54: Another repeat of the previous sentence?
- p.2, l.54: Bit of a jump here from GPR straight into modelling approaches. Maybe just add a half-sentence introducing the switch from observations to modelling.
- p.3, l.63: The way this is written implies the types of model listed here were all considered as part of ITMIX, when the CNN-based approach of Jouvet et al. (2021) postdates ITMIX and is not part of the suite of models assessed there. Just rewrite this a little to make it clear whether this is meant to be discussing the types of model specifically included in ITMIX or ice-thickness models more generally.
- p.3, l.69: 'robust' in what sense?
- p.4, l.102: Strictly speaking, IGM uses the Blatter-Pattyn approximation (first-order approximation), not full-Stokes
- p.5, l.117: 'were validated'
- p.9, l.205: I'm unclear about whether this means Pw has been set to ice overburden everywhere (and therefore N = 0), or whether N has been set to ice overburden everywhere (and therefore Pw = 0). Either way, this is a very strong assumption that needs to be justified. If the former, physically, this means the authors are assuming all the weight of the ice is supported by basal water, implying that

there is zero ice-bed friction. This is true in some places, some of the time, but I think assuming it as a default is perhaps a bit much. Something like N = 0.9(ice overburden) might be a little more defensible and better representative of the mean state of most glaciers? If the latter, the authors are ignoring hydrology entirely and therefore making it very difficult for any sliding to occur, which would seem a bit of a self-destructive assumption given the point of the model is to include sliding. Please clarify what's happening here.

- Figure 3: I'm very glad the authors have extended the data range a bit here, and I'm aware of the points they make in the response to reviewers on this. Still, as presented in the paper itself, 13/16 glaciers have the final, penultimate or antepenultimate value tested selected for B, which very much gives the impression that RMSE just drops as B increases and that a larger range of higher values for it should be included. I think the authors need to perhaps add a couple of sentences discussing this: presumably the apparent relationship is due to B effectively being a tuning parameter that compensates for other inaccuracies in the model, and that what is being sought is the minimum RMSE within the range of physically plausible B values, not the global minimum RMSE (which is what l.293 currently implies). Or, if the point is to find the global minimum RMSE, B really is just a tuning parameter, and we don't need to worry about the physically plausible range of values, and therefore the authors should show the graphs with the extended axes that are in the response to reviewers.
- p.16, l.351: 'regional'
- Table 3: I think the authors might want to think about their accuracy metrics here. This table shows that for the vast majority of the 16 (11/16) glaciers, ITMSL is out-performed by either Millan or Gantayat, even on nearly all the faster-flowing glaciers where one would expect inclusion of sliding in the model to improve the results. Of the 5 glaciers where this table shows it's got the closest match to the validation thicknesses, 2 are only very slightly in its favour (to be fair, one of the 11 it's not the best on is also only just not in its favour). As presented, it's therefore not convincing me that ITMSL is really making an advance on existing thickness models, which is undermining exactly what Section 4.3, immediately following, is purporting to demonstrate. This is a tension that needs to be resolved to make the paper credible. Presenting Table 3 as mismatches and then showing the average mismatch (maybe ITSML is better there?) might also be more intuitive. I think it's also worth pointing out that the modelled ub/us ratios are relatively similar, varying much less than the observed ice velocities, even for glaciers of seemingly similar thickness. As the ice thickness controls the amount of ice creep, the glaciers with higher velocities for the same ice thickness should have much more sliding and thus much higher ub/us ratios, and this is not seemingly something that the model is recognising. Or, the GPR ice thicknesses are very unrepresentative of the mean ice thicknesses at each glacier. Or the mean ratio isn't very meaningful, which I think is what's actually going on. This is also something that maybe needs to be considered when redesigning the table.
- p.17, l.357: Presumably, these are the correlation coefficients between the GPR thickness and the modelled thickness? It's not entirely clear as written.
- Figure 7: The caption needs to be expanded a bit. At the very least, the abbreviations on the figure axes should be stated in full here, so the reader doesn't have to find them in the text. Based on this figure, I make it that ITSML has the highest CC on 9 glaciers, lowest ME on 4, lowest RE on 3 or 4, and lowest RMSE on 7. I get that ITSML is individually better than either Gantayat or Millan, but I'm not being convinced it's a major advance, especially when it seems to be clearly worse than Gantayat on the most heavily sliding glaciers (assuming the fastest glaciers have the most sliding) where inclusion of the sliding law should really matter. As the discussion then goes on to point out, ITSML doesn't work so well on small, non-sliding glaciers either, so it seems that what it's actually good at is medium-sized, slightly slidey glaciers, which isn't nothing, but also not quite the slam dunk the paper wants it to be. Figure 9 and the associated text do a much better job of presenting the advantages of ITSML, so I'm wondering if it would be better to present results generally in terms of a split between slower- and faster-flowing areas, because the mean figures offer only very lukewarm support for the authors' contention that ITSML is doing well. This would also be more intuitive: including the sliding law shouldn't make any difference in slow-flowing areas, because there's no sliding, but should matter in fast-flowing ones, and rethinking the results section to highlight that might make it more convincing.
- p.22, l.455-462: I feel this section could be more impactful if the authors presented it as ITMSL is producing more moderate, sensible ice thicknesses, rather than very thin or very thick ones, and is

therefore a bit more reliable than at least some of the other models. As it's currently written, that is implied, but not explicitly stated, which means it pulls its punches a bit.

- Figure 12: Again, this isn't super-convincing that ITSML is doing notably better than any of the other models, largely because all it's showing is the ice thickness, not the mismatch. ITSML generally seems to predict higher ice thicknesses, but the figure on its own doesn't provide any information about whether that's at all realistic. I'd suggest doing the same plot, but graphing the mismatches, as the text states (l. 464) that Figure 12 is related to an accuracy assessment on these data. At the moment, all it shows is the data, not anything about the accuracy, which is the more useful part for the reader to know about. Though, to be honest, with Figure 13 there, that might not be that useful either, as that largely shows that.