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.

I am not convinced by this paper as it stands. I have questions about the dataset used in this study, as well as engagement with the wider literature, and I do not believe that the study shows conclusively what it purports to show (see below for specific details). I also have some major concerns about the model parameters and the presentation of the results. Fundamentally, the paper requires very substantial work to meet the required standards, to the extent that I feel I have to recommend rejection, as I do not think it will be achievable within a major revisions timeframe. In particular, an almost entire re-write to follow the ITMIX protocol would be the easiest way to resolve many of my concerns, but at this point, we're talking about an almost entirely separate paper, rather than a revised version of this one.

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

Major Comments
- Engagement with the literature: as pointed out below, the authors' use of terminology is curious. Couching the paper in terms of the SIA rather than laminar-flow theory would make it much more obvious how it engages with existing glaciological literature. As it is, it feels as if the authors haven't really understood where their own paper fits in the wider debate, nor how to position it, as it's written in a very non-standard way for a paper on modelling glacier sliding.
- ITMIX: As a related point, at no point in the paper do the authors mention ITMIX, the Ice Thickness Model Intercomparison Experiment, which is the gold standard in glaciology for evaluating the performance of thickness-inversion models. It is referenced in the discussion, but the actual acronym never appears. This is a curious omission, and again makes it seem as if the authors aren't really aware of the literature on the subject. Especially because, if the authors want to show how well ITMSL performs compared to other inversion models, reproducing the full set of ITMIX experiments or at least a subset of them would be by far the best way of doing so. It would make it much more obvious whether ITMSL is actually adding something or not, and clearly show where it performs better or worse than other approaches. Currently, the authors sort-of half-reproduce ITMIX in Section 5.4, but in a rather odd way that appears to show ITMSL isn't really doing anything much better than other existing models. I would urge the authors to rethink their paper comprehensively, and adopt the ITMIX protocol for a revised version. At the very least, there needs to be substantially more engagement with the two ITMIX papers in order to strengthen the paper's conclusions.
- Choice of dataset: At no point do the authors explain why they select the 16 glaciers in their dataset out of all the possible glaciers in HMA. What is particularly curious is that, based on the information in the paper, it seems very likely that 7/16 of them are barely flowing or sliding, in which case using a velocity-based inversion approach is fundamentally flawed (and the scale of the application is not sufficient to defend it based on the need to have a uniform approach across hundreds or thousands of glaciers). The scientific rationale behind this choice needs to be explained.
- Choice of models: Why do the authors pick specifically the Millan and Gantayat models as their comparators? Again, this is never explained. Why not some of the other models in ITMIX? Why not all the models in ITMIX? This feels like cherry-picking or convenience, but I have no information to work out what's going on at the moment. In particular, if I accept the paper at face value, proving that ITMSL works better than the Millan approach, which was designed to work at a global scale, on only 16 glaciers isn't all that impactful. This aspect would be greatly improved by adopting the ITMIX protocol.

- Model parameters: See specifics below, but I find Figure 3 extremely unconvincing as to the B values for each glacier the authors select being the optimal values. The other model parameters are then entirely skated over, so it's very difficult to get any real sense of whether what the authors have done is sensible.
- Presentation: For a paper that is about showing how well a new approach does in inverting glacier thickness, there is a stunning lack of figures that show ice thickness or useful information (ice velocity, say) more generally. There are a lot of tables, often with some not hugely relevant numbers in them, but very little in the way of the sorts of figures I'd expect to see. To the extent that it feels as if the authors are deliberately obfuscating their results (I'm not saying this is the case, but, if that's not the reason, it's a baffling oversight).

Minor Comments
- Terminology: throughout, the authors talk about laminar-flow theory. In a glaciological modelling context, though, this is more commonly encountered as part of the shallow ice or shallow shelf approximations, so I think it would be worth adding a sentence somewhere in the introduction just clarifying the different terms and how they relate for the purposes of this paper. In a sense, all ice-flow models are laminar, because that's how ice flows – it's not turbulent – and I think what the authors really mean is that they've got a better version of the SIA that incorporates some consideration of sliding rather than neglecting it entirely. So, I'm wondering if there needs to be some more thorough re-writing to make it clearer what's going on
- p.1, l.27-28: GV14 and GV22 are codes purely used in this paper and not the names of the respective models used or anything that would be recognisable to someone who hasn't already read the paper. Which isn't ideal in the abstract. I'd just take the codes out and give only the references.
- p.2, l.50-51: 'requiring substantial manpower and material resources'?
- p.3, l.61: I think there needs to be an 'and' after the brackets to make the sentence make sense
- p.3, l.67: The implication here is that there's an alternative to using some sort of sliding law to represent sliding in a model, which there...isn't? Even using some version of laminar flow is assuming a sliding law. Might be worth rephrasing this a little more definitively.
- p.3, l.79: 'the theoretical basis'?
- p.4, l. 88: 'dynamics'
- p.4, l.89-93: What's the rationale behind this glacier selection? I don't doubt there was one, but it's exceedingly obscure as written. The Himalayas seem rather under-represented compared to the Tian Shan, for instance, so there needs to be some explanation of why this selection was made. Also, a decent chunk of the dataset seems to be rather small glaciers: is there any sliding actually happening on them? Otherwise, why bother including them, given the whole point of the paper is to integrate basal sliding dynamics? Have the authors considered a range of glacier velocities, for instance?
- p.5, l.110: Sure, the average error for the Millan velocities might be 10 m/a, but there is an error map provided with the data and is it 10 m/a on average for these 16 glaciers? Some more clarity here would be useful.
- p.5, l.117: Yes, I agree for most of them, but maybe not for the one where the GPR measurements are from 1980…? I would expect there to have been some substantial thickness changes in 45 years…. This should be at least acknowledged as a slight inaccuracy here.
- Table 1: I would really like to see the mean glacier velocity or some other measurement of how much it's moving included here, as well as mean measured ice thickness or similar in order to assess how representative of glacier sliding this dataset might actually be. Elevation extent, by contrast, seems like it could be safely deleted – I don't feel it's telling the reader anything useful for the purposes of this paper.
- p.7, l.145: 'though'
- p.7, l.149: The word 'accurately' is doing a lot of heavy lifting here. We know that none of the existing sliding relations fully captures sliding with perfect accuracy (otherwise, why would we have so many options?). Also, this paper is not explicitly modelling subglacial hydrology and thus N, so I'm not sure claiming that this is an 'accurate' implementation really stands up.
- Figure 2: 'Surface Slpoe' probably wasn't intended….
- p.10, l.219-220: I think the authors need to specify which parameters were literature-derived (and their values), and which were glacier-specific (and how they were chosen). Otherwise, it's very difficult to get much sense of what's actually going on.

- Table 2: The more interesting question is: are these B values physically realistic? Some consideration of this in the foregoing text would be useful. Also, what's the 'Total' value in the table – the mean?
- Figure 3: I was expecting to see some sort of parabola or L-curve with a clear minimum RMSE at the optimal B value for each glacier. Instead, it looks as if many of the glaciers (principally the smaller ones) have a very unstable behaviour here, and many of the glaciers have the highest or second-highest B value used chosen on a curve that's clearly still trending downwards or is so chaotic there's no guarantee it's not going to suddenly jump lower at the next value. As things stand, it feels to me that the authors have little justification for picking these B values for most of their dataset – they need to extend these curves to test B values above 480 to prove that their preferred values are actually minima, not just a local blip
- p.12, l.238: I think, given this paper is claiming to do ice thickness better, we really need to see the distributed fields for all 16 glaciers, not just one, and then some summary statistics in a table for the other 15. For all I know based on what's shown currently, it might be that half the glaciers have horrendous artefacts that cancel out or something going on!
- Figure 4: 'Mersured' is also probably not intended….
- p.13, l.258: And what's that as a percentage of total glacier velocity? Is that a large contribution of sliding to total motion, or negligible?
- p.13, l.258-261: Frankly, I'd be extraordinarily surprised if sliding velocity didn't correlate well with surface velocity. I think we can take that as read without it needing to be reported. I also think we can safely say the entire glaciological community knows sliding matters for accurate glacier modelling, otherwise we wouldn't have spent an awful lot of time over the last 80 years trying to quantify it. The reason it's often left out of regional- or global-scale models is because many smaller glaciers don't really slide, and it's complicated, so it's easier to approximate it some other way or neglect it entirely. I think I might just delete these lines entirely….
- Figure 5: I would replace panel d with the modelled ice thickness from ITMSL and then produce that four-panel figure for all the other glaciers too. Possibly, some of this might need to end up in the supplementary material.
- p.14, l.270: There's no 'suggests' about it – we know it misses things.
- Table 4: OK, so five of the glaciers in the dataset have a surface velocity so low that I'm almost certain that the velocity observations are likely to be very unreliable. Another two seem likely to barely be getting above the noise-to-signal ratio, so that's nearly half the dataset that's moving so slowly that the velocities provide very little in the way of a useful constraint and where I would be very surprised if there was any measurable basal sliding…. I come back to my previous point: why these 16 glaciers?
- p.17, l.311-325: I think this can be summed up as 'these glaciers are way too small to have any reliable velocity observations or basal sliding'. So, again, why include them as targets to demonstrate the importance of including basal sliding in a model?
- p.18, l.329: Established glaciological what? Theory?
- p.18, l.329-336: This pretty much comes down to the model incorporating basal sliding performs better where sliding is a meaningful proportion of total velocity and doesn't where it isn't, which is perhaps to be expected? Again, it seems strange to pick a substantial proportion of small, non-sliding glaciers to demonstrate the importance of sliding, and it still hasn't been explained.
- p.18, l.341-342: Well, yes, obviously ITSML is more sensitive to basal sliding because the others ignore it? Not sure that's terribly relevant to showing ITSML is better, just that, as a model, it's sensitive to its own inputs, which would be extremely shocking if the reverse were true.
- Section 5.3: This a) isn't anything to do with ITSML and b) everyone already knows that using higher-resolution inputs is going to give you a more accurate representation of the glacier and better results. I think I would just delete this section – it's adding nothing to the paper's main argument
- Section 5.4: The authors appear to be reinventing ITMIX here. But inadequately. And also proving that ITSML isn't adding much to the existing range of thickness-inversion processes. Which is probably not the intention, but is very much what I'm getting.