We thank the anonymous referee and the editor for their time and care in reading our manuscript and for their encouraging comments and constructive feedback. The manuscript has been updated to reflect the referee's comments.

We note an additional minor change to the manuscript:

- The unit of the sliding coefficient c has been corrected: it was previously incorrectly stated as MPa yr^3 m^{-3}, but has now been changed to the correct unit of MPa $\mathrm{yr}^{1/3}$ $\mathrm{m}^{-1/3}$.

Our responses to the referee's specific comments are provided below.

Author response to RC3

Referee comments in blue
Author comments in black

> Smith et al. (2026) assess the performance of a cost function minimization inversion to recover the ice thickness of Bhote Kosi glacier in High Mountain Asia using the Instructed Glacier Model (IGM). They perform multiple sensitivity tests to find optimal values for weights of different cost function terms, and validate the results against a recent dataset of observed ice thicknesses.
> The manuscript is well-written and of great clarity. I find that the authors have done a good job of incorporating the previous reviewers´ comments. I therefore only have a few small general comments which can probably be addressed through small modifications of the text at the authors discretion.

Thank you for your thoughtful response and your constructive comments which will help to improve the manuscript.

> The authors discuss the limited interpolative power of thickness observations in their setup. One aspect that could be added here is that approaches based on mass conservation should (in theory) suffer much less from that since a cross-sectional line of thickness observations together with velocity observations should constrain the mass flux to downstream locations pretty well.

A paragraph will be added to Sec. 4.5, stating the existence and benefits of such methods, and clarifying our reasoning for using a different method.

> IGM allows additional cost function terms and control variables to be included in such inversions (e.g. using surface elevations in the cost and as a control, flux divergence, . . .). Perhaps the authors could briefly mention that and discuss why they have decided not to include/test that. In relation to the previous point, could some form of mass conservation have been included to increase the interpolative power of thickness observations?

A paragraph will be added to Sec. 2.1 to explain our choice not to use IGM's additional cost function terms and control variables.

> In the discussion, the authors elaborate nicely on why increasing the regularization cost must lead to a reduction in the ice volume – an issue that seems quite fundamental with the setup chosen here. To add to that discussion, I wonder if the authors could offer any advice/future directions on how to tackle this problem to readers facing similar issues?

Indeed, this is an important issue to be aware of, and one of our main conclusions is that the regularization parameter must be chosen carefully to ensure accuracy of the estimated thickness field and a realistic ice volume. In Sec. 4.1, we will add a paragraph suggesting one possibility for avoiding this effect, although we note that this is beyond the scope of our study.

> Fig. 1: Consider colouring thickness observations by their value. That would give readers a direct clue as to what thicknesses to expect and thus also allow them to judge the magnitude of errors better

Fig. 1 will be updated to colour thickness observations by their value, and a colour scale will be added.

> Consider providing a colour scale in Fig. C1: readers are referred to that figure much earlier than Fig. 6, so they haven't seen that colour scale yet

A colour scale will be added to Fig. C1.