Review of Sommer et al. (2022): Constraining regional glacier reconstructions using past ice thickness of deglaciating areas – a case study in the European Alps

Summary
This paper presents a new approach for reconstructing the thickness of glaciers using an additional constraint of DEM-differencing of areas that have become deglaciated within the period of observational record, building on previous work by the authors. They apply this method to the European Alps, reconstructing ice volumes in 1970 and 2003, and show that, deglaciation preferentially occurring in thinly glaciated areas, this induces a global bias in their results similar to that found in the ITMIX2 experiments when using thickness observations from thinner parts of glaciers. They consequently derive two different empirical correction factors for the modelled ice viscosity, one based on distance from the glacier margin and one on elevation, and show that including these substantially improves their reconstruction and leads to better matches between modelled and observed thicknesses than in previous studies, though still with possible large local mismatches owing to the regional-scale calibration of the correction factors. Overall, they find glacier volumes in-line with previous recent studies of Alpine glacier volume, but with the major advantage that their approach could be easily extended to areas without direct thickness observations.

All in all, I think this is a good paper with much to recommend it. Most of my concerns are of a fairly minor nature, though with a single larger one to consider. I feel the paper makes a valuable addition to the literature on estimating glacier thickness and volumes by proposing an innovative method that could be of use globally.

Samuel

Major points

- Applicability to other regions: The authors touch on this briefly at the end of the conclusion, but perhaps soft-pedal this problem a little too much to be considered entirely honest about it. The paper already shows that the large-scale calibration of the empirical factors leads to substantial local variation within the Alps; I thus can't help feeling that it's exceptionally unlikely that the same correction factors would work in an extra-Alpine context. I think some additional consideration of the challenge likely to be posed by trying to apply the method elsewhere needs to be included – nothing much, just another couple of sentences in the conclusion – but the current formulation is unrealistically optimistic, I feel.

Minor points

- p.1, l.13: I might say 'due to the difficulty of undertaking field surveys' or 'challenging field conditions'. Strictly speaking 'challenging field surveys' doesn't really mean what you're trying to get across – it implies the surveys were difficult but have been done, when what you're trying to say is that they're difficult and therefore haven't been done.
- p.1, l.25: I'm wondering whether the reconstructed volume in 1970 is really the right thing for the abstract when you also calculate a modern glacier volume for the Alps. My feeling is that people will be more interested in the modern value and how it stacks up to other recent reconstructions of Alpine glacier volume, or the rate of change between the two periods you've reconstructed, than the volume 50 years ago so I'd suggest re-writing the abstract along those lines, certainly if you've only got space for one highlight result.
- p.2, l.35: You need to spell out what GLOF stands for before using the abbreviation.
- p.2, l.50: 'Contrastingly'
- p.4, Eq. 2: I can't see η used anywhere in the equation, though the text (lines 101-103) implies it should be? As a result, I'm unclear exactly how your viscosity scaling is actually being applied to the flux field to modify the inferred ice thicknesses.
- p.6, l.165: 'slope-dependent'
- p.9, Fig. 1: I'm not entirely sure this figure helps explain things all that well. All the bidirectional arrows make it very challenging to work out where to start and re-reading the caption several times hasn't helped me make a lot more sense of it. I am prepared to accept that I'm not very good at

understanding diagrams, but if you can come up with a more intuitive schematic, that might not hurt. It made sense after I read Section 2.5.1, but not till then, so at the very least move it to after that section of text.

- p.11, l.295: ‘elevation-dependent’
- p.12, Fig. 2: As a general point, ‘dependant’ is a word in English, but it’s the noun form, so a dependant would be, say, your child. If you’re aiming for the adjective, it’s always ‘dependent’. I’ll stop pointing it out now, but go through and replace all instances of it (you almost certainly do not mean ‘dependant’ anywhere in the paper).
- p.15, Fig. 4: I’m not sure a linear regression is all that great a fit, based on the graph. It overestimates in the middle and underestimates at both extremes. I realise this is what the second correction factor is ultimately fixing, but is there any way you could test the impact of using a non-linear regression? Also, what is the dotted black line on the graph showing? I assume it’s α_{tres}, but then it’s got a different value to that quoted in the text at l. 340. Please clarify what’s going on here.
- p.16, Fig.5: Similarly to Fig. 4, you need to explain in the caption what the dotted black line represents. Here, the value matches up with the value quoted at l. 365 for h_{tres}, so I’m confident it’s that, but it needs stating in the caption.
- p.18, Sect. 4.1: I’m wondering if you could be a little stronger here in your assertion that earlier studies might have underestimated Alpine glacier volumes. Given nearly all the recent studies are pointing in that direction, it seems to me that one would have to be extremely perverse to argue that the earlier studies weren’t underestimates.
- p.25, l.541: ‘inferred’ not ‘interfered’
- p.25, l.546: ‘slope-dependent’, not ‘slope-depending’