

15.05.2022

Dear Francesco Avanzi et al.,

Dear Andrew Wickert,

in the revised manuscript version Avanzi et al. have addressed the review comments that Anonymous Reviewer #1 and Anonymous Reviewer #2 have raised in the first assessment of the manuscript. After my assessment in this second round of reviews, I think the main points of criticism have already been discussed and addressed adequately by the authors.

Having said this, I have just some minor comments I recommend to account for before the manuscript is published. Most of them are of a technical nature, and due to the fact that some statements seem vague or contain "flavored language" (=judgmental/subjective terms). However, overall there are no major statements left that I would particularly disagree with. I am fine with not seeing the manuscript again before publication.

All the best wishes!

In the following, my line numbers refer to the revised submission of the manuscript:

Structural/content comments

I. 218 f., and other places: there are a lot of announcements what could be improved in the model and what is planned future work. This is interesting, but it is not typical "paper style" to mention it distributed in the entire manuscript. Comments like these are content I would expect mostly in the outlook section.

Section 2.5: Maybe I missed it, but where does the initial glacier thickness/outlines come from? I think this should be cited as well.

I. 494: "240m": I think this is the first time where the resolution is mentioned in the text. Shouldn't it occur in a place somewhat more prominent?

I. 643: I am confused here about the reason you give for the positive elevation changes at the head of the glacier: in the definition of h_G in I.419 you say that h_G is glacier thickness. The glacier thickness defined in this way and its change shown on Fig. 10 bottom right thus includes surface height (change) including ice, firn and snow changes. Δh_G might be positive due to the snow distribution pattern on the glacier, or differing elevation data acquisition dates within the hydrological year. However, according to me the positive change is not due to the snow-to-ice conversion (It would be negative, since snow is compacted when turning into ice!).

Conclusions: What I am missing here is a summary of the validation with some numbers that prove how well the model did. You are talking about the fact that this model "channels elements from the state of the art in cryospheric sciences into a parsimonious and computationally efficient model", so, since the manuscript spends quite some paragraphs on validation, a summary of these quality measures should

be found here again (e.g. the efficiencies you have added as response to Reviewer#1's comment on "Evaluation at Torgnon study plot (section 3.3)").

Technical/grammar/spelling/language comments

l.15: "a user manual"

l.30: "about snow-glacier amount": What is meant by this? The amount of snow on glaciers?

l. 34 "applicationS"

l. 47: please add citation from complex flow approaches, e.g. Jouvét & Huss (2019): Future retreat of Great Aletsch Glacier, Journal of Glaciology.

l. 80: "Alpine"

l. 204 ff.: "Our approach is the result of intensive trial and error and yields satisfactory results" -> IMHO it is too early to state this here.

l. 213, 215: THE model time step

l. 501/505: "in principle, a fairly large amount" together with "fine-tuned based on expert knowledge" and "our experience suggests": these are four vague expressions concatenated, which leaves me with a feeling of insecurity. My advice: name the exact (amount of) parameters that can be calibrated using standard (e.g. least squares) methods, and those that have to be guessed. If your experience suggests two main calibration parameters, this statement should be supported by how you come to this conclusion (e.g. by a sensitivity analysis), especially in view of the fact that you deny their strong influence afterwards (see also comment on l200/486/489 of Reviewer#2).

l. 516: "based on preliminary tuning": what exactly is meant by this?

l 548/549: "note that snow-depth assimilation maps are only generated between August and April in an effort to unaffected the simulation of the depletion phase of the seasonal snowpack." What does this mean?

Fig. 5: the position of the panel labels is such that they can really be overlooked easily. I would put them outside of the individual panels in the top left corner.

l. 561:"complicate"

l. 566: "satisfactorily": leave out (judgmental)

Fig.6: I would add "OL" spelled out, at least in the caption. It should be self-evident what abbreviations in the figures mean without having to dig into the text.

l. 596: delete "well"

l.600: unclear to me what you mean by "warm" and "cold" storm

Fig.8: the LWC levels on the colorbar are somewhat arbitrary: why don't you go for a continuous colormap?

l. 627: This sentence is not understandable to me.

l.629: "2012 data" – source?

l.632: "satisfactory": here again: I would state only the correlation result and let the reader judge

l.634: What does "visually higher" mean?

l. 634 ff.: no "the" before the proper nouns

l. 636: first-> former

Fig. 10/11: it's basically impossible to compare the individual figure panels, since the colorbar changes its limits constantly. Please ensure they are the same for all panels.

I. **663**: "When is a..."

I. **670 f.**: "a few hours" -> since you give so precise details about the technical equipment, I would have expected a number here, accordingly. (as a consequence of reply to comment on I.613 by Reviewer #2)

I. **668**: "attractive" -> I would use "particular", or similar

I. **672**: "is a factor here": a question arises for me: a factor with which influence? Is netCDF fast or slow compared to what is possible? How much time does it consume with respect to the overall runtime?

I. **676**: delete "such"

I. **680**: "urgent": if you say it like this, it gives the impression you should have done it before publishing this article. Do you mean "help to resolve rain on snow events in the model"?