The Cryosphere Discussions

The Cryosphere Discuss., referee comment RC1
https://doi.org/10.5194/tc-2021-233-RC1, 2021

Comment on tc-2021-233

Ben Marzeion (Referee)

Referee comment on "Modelling supraglacial debris-cover evolution from the single-glacier to the regional scale: an application to High Mountain Asia" by Loris Compagno et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-233-RC1, 2021

Compagno and co-authors introduce parameterizations for the evolution of debris cover distribution and thickness into a glacier model, applicable on the large regional and global scale. They derive the parameterizations, calibrate parameters based on observations (some of them depending quite strongly on another model), and evaluate the parameterizations using independent observations. They apply the model for projections of glacier evolution in the 21st century in High Mountain Asia.

There is no doubt that the manuscript improves the state-of-the-art of how debris cover is represented (if at all) in glacier mass balance models applicable to the regional and the global scale. The advances presented in the manuscript clearly contribute significantly to our understanding of how relevant debris cover is in shaping the future of glaciers. The manuscript is generally well written and the results are generally presented well, but I also have a relatively large number of minor or technical comments and suggestions.

There are, however, a few issues with the manuscript that are more substantial. I believe that the authors will be able to address them, and I don't believe that the main conclusions of the manuscript will change. But since they could only be addressed in extensive revisions to the text and/or additional analyses (see below for details), I consider them major.

Major comments

- The estimation of debris cover thickness (L91-99) is very unclear to me: (i) why is "observation" in "observations-based mass balances" in quotes? (ii) how is the energy balance model that you apply on each glacier calibrated, ie., and how are the glacier-specific Østrem evaluated? (iii) What is the reasoning behind Eq. 1, and the meaning of the "free parameters" i_debris and k_debris? I appreciate that you cannot repeat the manuscript of McCarthy et al. here, but the description needs to be understandable in principle without going to the reference (even if it was accessible to readers, which it is presently not). Probably I just don't get it, but I am also left a bit puzzled why a temperature-index melt model is applied for the projections if the authors have an energy balance model that can deal with debris cover and is applicable for each glacier, and which the authors trust so much as to not only estimate debris thickness, but additionally glacier-specific Østrem curves.

I have a similar difficulty following L154-166: may the problem is that it remains unclear whether the goal of the equations is to mimic a physical understanding of the debris effect (such that it is possible to explain the "meaning" of the different parameters), or to parameterize the shape of the Østrem curve. Could, e.g., g be called a "melt modification" parameter (or "enhancement factor", as in Fig. S1) for the temperature index parameter? And is the goal of Eq. 3 to create the shape of the Østrem curve for g? It might be helpful to include an example of a parameterized Østrem curve with labels for the different threshold and critical values of h (i.e., a schematic version of Fig. S1 including the names of the parameters – and potentially 2 or 3 different curves for different parameter values).

Finally, more needs to be said on the uncertainties of the estimated debris thicknesses. Otherwise, it is very hard to make sense of the relevance of e.g., the thickness differences presented in Fig. 5.

- I don't quite understand the reasoning behind Eq. 4, specifically of including abs(b_z,t): if we assume a generally negative glacier-wide mass balance (B_(t-9,t)), an anomalous positive local mass balance would increase the fraction of debris cover in the elevation band, according to this equation. Similarly, if the glacier-wide mass balances were generally positive, a local negative mass balance would decrease the fraction of debris cover. I.e., the assumption that b_z,t is always negative in the elevation bands where debris is present seems a bit strong to me, and I'm wondering what the impacts are around the ELA altitude, where the sign could go both ways. The same question applies to Eq. 6. Also, please clarify whether c_lateral and c_thickening are global or glacier-specific parameters. Finally, the choice of ten years as a time scale in these equations is not justified nor calibrated. It may be hard to calibrate and evaluate, but it would be good if the authors point out to which degree this is arbitrary, or whether there is a more substantial argument for ten years.

- Comparison between "explicit" and "implicit" treatment of debris cover: I think one of the strongest points of the study is the possibility to evaluate how "wrong" glacier models are that are not taking into account debris cover explicitly. As is pointed out in the paper – and depending on the calibration scheme of the respective model – they often do take it into account implicitly, through the calibration using observations that include the effect of debris cover. Unfortunately, this possibility is mostly left unused in the manuscript. I would like to suggest to add a focus on this, as it would be straightforward and quite

informative. Some examples of simple analyses: (i) what is the distribution of the differences of DDFs between explicit and implicit treatment of debris cover in GloGEMflow? (ii) How does the performance of the mass balance model change when debris cover is treated explicitly? Does it (significantly?) reduce RMSE or increase correlation of observations, etc.? I.e., how do figures S2 and S3 change when the debris cover parameterization is introduced? I appreciate the difficulty of getting this into the (already quite long) paper, but I would argue it is quite central to address these questions when introducing a new parameterization. To some degree, this has been done (e.g., Fig. S11) – but not for the mass balance model. I can well imagine that there are no significant changes in mass balance model performance – but it would be good to point this out, because I would argue that representing more processes without significantly deteriorating the performance of a model is already a good step ahead, given the limitations of data availability for calibration and evaluation.

Minor comments

- L10: since the focus of the manuscript is on the effect of debris cover, it would be more interesting if the impact of debris cover on projections was given instead of the totally projected mass loss. E.g., "explicitly accounting for debris cover in the projections only has a minor effect on the projected mass loss, but improves the representation of processes on the intra-glacier scale" (or something similar, based on Sect. 7.1).

- Throughout the manuscript: the authors often refer to the "module", but in many places the word "parameterization" would be more correct. I would suggest distinguishing between the parameterization and its implementation as a module of GloGEMflow, in order to increase clarity.

- L27-30: it might be worthwhile here to be a bit more specific by explaining the impact of debris based on an energy balance perspective instead of "ice melting" (i.e., albedo, thermal conductivity, reduced turbulent fluxes of heat and water vapor, etc.).

- L31-32: I would argue that debris cover would evolve in response to any non-zero mass balance, also if there was no "additional" disequilibrium between glacier geometry and climate conditions.

- L39-41: if the relatively constant debris cover in the Karakoram region is to be explained by neutral or slightly positive MB in the region, it is not an exception from what was explained above, which referred to negative MB.

- L43-44: Sentence is a bit unclear: I think what you want to express is that the mass balance profile of a debris-covered glacier may have a local minimum at mid-elevation?

- L71: the references to Marzeion et al. 2020 and Edwards et al. 2021 are a bit strange here: it would make more sense to compare the results with projections obtained from the same model, but without applying the debris cover evolution parameterization.

- L104-117: it is not clear here whether only the debris extents are used for calibration and validation, or whether additionally, debris thicknesses are estimated for different time periods as well, in order to evaluate the modeled thickness evolution as well (I don't think so, but please specify).

- L129-131: suggest to rephase for clarity, e.g.: "… we use 53 members of the CMIP6 ensemble (Eyring et al., 2016) from X different GCMs, covering 5 SSPs (5 members for SSP119 and 12 members for all other SSPs)."

- Sect. 2.3: it remains unclear here whether you use the MB data for calibration and evaluation of the glacier-wide modeled mass balances, or also for the evaluation of the mass balance profiles.

- L131-135: would it be correct to state that you use only anomalies from the GCMs, while keeping climatology from ERA5? The phrasing in the manuscript - "a set of additive and multiplicative corrections" - seems a bit unclear to me.

- L172: this was mentioned before, but why actually are surging glaciers excluded from the application of the debris cover parameterization? Because of difficulties in calibration and evaluation, or because of more fundamental, physical reasons?

- L212-219: it is not quite clear to me which variables are used for the regression here: do you calculate a regression of ELA over time, and then use the slope of the regression directly – or do you have a constant of proportionality (L213 says "proportional to")? I also would argue that claiming "no calibration parameters" is a bit bold, because some choices (e.g., new debris thickness of 1 cm, expansion by Eq. 5) seem a bit arbitrary to me (the authors point that out). It is the authors' choice not to calibrate them, and it may not be possible to do so, and I think they should say so explicitly.

- L294: it is unclear why two values, each with uncertainties, are given for what I would expect to be a range (two values with no uncertainty) or a number with uncertainties.

- Fig. 5: the Figure seems excessively complex from the perspective that the tabulated values (right part of the figure) do not seem to play any role in the analysis of the results. I would suggest to remove this part. Also: I don't understand the definition of DC_frac.

- Sect. 5.1: this should be expanded significantly, see major comment above.

- Sect. 5.2.3: I'm wondering how the difference between activated and non-activated thickness evolution compares to the uncertainty of the estimated debris thicknesses in the "observations" used for evaluation.

- Fig. 8: I don't understand the meaning of the colored numbered circles in panels a and c, and the illustration in panel b. I guess the intention is to point out the relevant processes for different locations and times for changes in debris distribution on the glacier, but since all three parameterizations are active all times, it's hard to get a specific meaning (especially in panel c). I would suggest to remove this. Also: please point out how the maps (panel c) are drawn, given that the model only "knows" elevation bands. I guess the red outlines are simply based on elevation?

Finally: instead of panels f and i, please consider only showing the differences between implicit and explicit treatment; as is, they are very hard to see (which of course is relevant in itself, but maybe not the main point here).

- L395: whether the mass balance gradient is "erroneous" in that case is not clear, unless it is shown that the explicit treatment of debris cover actually improves the model's representation of the mass balance gradient (see major comment above). Also: in Fig. S7, I think the last sentence in the caption should read "higher mass balance gradient with elevation of (b)" (not "(a)").

- L441-443: this statement is not well backed-up by the analyses presented, at least not as categorically as it is given here: there is no evaluation of the impact of the parameterizations on model performance regarding local mass balance, glacier length, or runoff. I agree that the results including the parameterizations are more plausible – but it is not a correct/incorrect-situation, and it has not been shown that the model's performance did in fact improve.

- L458: I would also argue that uncertainty arises from parameters that are not calibrated here; e.g., the time scale for debris cover expansion/thickening, new debris thickness.

- L461: the "little impact" is, however, comparable to the magnitude of switching the parameterizations completely off.

- L464-465: unclear whether these numbers refer to the sensitivity analyses (changing c_thickening and c_lateral) of to completely switching the parameterizations off.

- L528-529: by affecting the MB gradient, debris cover would also impact methods of ice thickness estimation based on mass continuity – wouldn't it?

Technical comments and suggestions

- L1: grammar: not the area is altering the surface mass balance, but the debris cover.

- L4-5: grammar: "the module" was previously called an "approach"; therefore better (instead of "the module"): "... we implement a parameterization into ..." or "We derive a parameterization and implement is as a module into...".

- L8: replace "the model" with "GloGEMflow", in order to prevent misinterpretation with "the module".

- L10: replace "projections in the literature" with "previous studies".

- L11: replace "modelled" with "projected".

- L18: reference to IPCC (2019) should be replaced with reference to Lee et al. (2021), i.e. Chapter 4 of the 6th assessment report.

- L47: add "distribution" after "thickness".

- L57: delete second "by", otherwise it implies that it was the first projection generally for HMA glaciers.

- L66: I’m not sure if you can “extensively calibrate” a parameterization, since there is simply a given number of parameters; I suggest to apply the word “extensively” only to “evaluate”.

- L81: replace “condensed” by “simplified”.

- L75: the number of glaciers should be referenced (e.g., “… all 95536 glaciers contained in the RGI…”

- Fig. 1: (i) maybe a matter of taste, but the overlapping panels make the figure look a bit messy to me; (ii) “red spheres” are red circles; (iii) please indicate uncertainties of V, h_debris and A_debris (if relevant) in panels b-g; (iv) “map source” is the background elevation data set?; (v) why is panel f highlighted with a thicker frame?

- L86: grammar: “a surging glacier” (singular).

- L89: in reference, comma before Østrem is missing.

- L91: wrong parenthesis for references.

- L104: “see Sect. 3.2”

- L129: delete “Interim” (hint: the title of the reference Hersbach et al., 2019, includes the words “goodbye ERA-Interim”)

- L138: “is illustrated”, not “are”

- L251: fix phrasing in reference.

- L252: delete “of”.

- L254: please indicate the default values for the DDFs, in case step 2 of the calibration is

never done.

- L278: replace “spread” with “uncertainty”

- L278: “results’”, not “result’s”

- L290: not the sensitivity of the value is evaluated, but the sensitivity of the results to the value.

- different places in the manuscript: the year 2020 is given as both using ERA5 and CMIP6 data as forcing; I guess the use of CMIP6 only starts in 2021? Or is the transition really within the year 2020?

- Fig. 4 caption: typo “derbis”.

- Fig. 4: panel a: suggest to delete “Identify” from the title; panel c: suggest to label x_axis with c_lateral for consistency.

- Fig. 11: add explanation of dashed horizontal lines to caption. Also point out that this figure covers HMA, not the globe.

- L531: suggest to remove “appreciation”.

- L537: remove “availability”.