

Author's response to the comments received for tc-2021-31

The following pages contain a point-by-point reply to the comments provided by the editor and the referee that commented our third submission.

Each of the editor's comment (**EC**) and referee's comment (**RC**) is numbered. If a comment contained several points, we numbered them, and address them individually in our author replies (**AR**).

Editor comments

[EC 0.01] Dear Loris Compagno and co-authors,

(i) Your manuscript received two very detailed and critical reviews that both highlighted the novelty and innovation in the treatment of debris in regional/global scale glacier models and the related extensive modelling results/investigations and they confirm that the manuscript would be a very valuable contribution to TC. Although both referees were in general very positive, they also raised, besides the minor technical corrections, a few major issues that should be improved before publication. **(ii)** In brief the most important points were

- a) clarification and more details in the methods (in particular the debris thickness data and the parametrized oestroem curve)
- b) issue of omitting dynamic debris thickening/thinning (velocity) effects (add at least a discussion on this)
- c) better evaluate the role of debris cover inclusion (omitting, explicit, implicit)

As the revisions were rather substantial the revised version went to review again (to one of the previous reviewers), and I myself as the editor, also read and checked the revised version again. The re-review clearly stated the manuscript substantially improvement and almost all major issue were addressed well (see detailed reviewer comments further below), which I generally confirm. However, besides a few very minor points, the referee states that a few more substantial points seemed to remain which concern the used debris thickness datasets of McCarthy and are re-iterated below. I generally confirm the referees view on this and in addition.

[AR 0.01] **(i)** We thank the editor for the very positive feedback and for the comments. **(ii)** Below, we have addressed all issues raised by the editor. The manuscript was updated accordingly.

[EC 0.02] **(i)** I have one further more substantial point: i) (raised by rereview, see also list below): The referee is still concerned with the impression in the text that the McCarthy dataset (debris thickness) is based on first principle and does not require any calibration, which I agree with the referee is not true, as there the derivation uses besides the principle of mass conservation for example an EB-model which surely had to make some parameter choices (and probably some calibration). These parameters may be based on literature or have been calibrated (not by you but in the McCarthy paper), thus, the statement that no calibration is needed should be revised and clarified.

(ii) I have one more further more structural comment on this paragraph on the explanation of the derivation of the McCarthy debris thickness: you start with saying '...obtained through a simplified surface mass-balance inversion procedure...' which I understand is based on the principle of the mass conservation using remote-sensing data as input (dh/dt , velocities). But you then first mention the EB-model and only afterwards the mass conversation which I find a bit confusing. I would suggest to move the mass conservation principle (using ...data) right to the beginning and then later explain the use of the EB-model for calculating debris thickness from the inverted SMB, as this is the order you do it (or am I completely wrong here?). something along the lines of: '...obtained through a simplified surface mass-balance inversion procedure similar to (Ragettli ...) that is based on the principle of mass conservation using surface velocities and thinning rates as

input. It then uses an energy-balance model, meteorological data to iteratively solve for debris thickness'

[AR 0.02]: (i) We agree with the editor and with the reviewer (cf. AR 1.02 a) that this sentence was odd and that it could be misunderstood. Therefore, we reformulated it as follows:

L.104-105: 'Due to the physically-based nature of the procedure, the energy-balance model and the Østrem curves (see below) are not explicitly calibrated, but use model parameters that are based on literature value.'

(ii) We changed the sentence based on the editor's as suggestion:

ll.97-101: 'The debris thickness maps are based on McCarty et al. (2021, preprint), who used a simplified surface mass-balance inversion procedure similar to Ragetti et al. (2015) and Rounce et al., (2018). In a nutshell, the procedure uses the principle of mass conservation to infer on local glacier mass balance from surface velocities and thinning rates, and then iteratively adjusts the debris thickness to ensure consistency between the so-inferred mass balance and the output of an energy-balance model driven by meteorological data. More specifically, the procedure uses DEMs, glacier thickness, [...]'

[EC 0.03] ii) (raised by rereview, see also list below): the referencing to unpublished work of McCarthy et al. (see line 97,...) is an issue and as it is essential for being able to trace the explanation for the debris thickness dataset. As the referee suggests either you wait until the McCarthy paper is accepted (which would likely delay things) or alternatively (which probably makes more sense) you refer to the preprint version link and make clear in the reference that it is preprint.

[AR 0.03]: Due to the fact that McCarthy et al. is still under revision, we decided to use the preprint as citation. Both in the references list and in the text, we make clear that we are referencing a preprint.

In references: 'McCarthy, M., Miles, E., Kneib, M., Buri, P., Fugger, S., and Pellicciotti, F.: Supraglacial debris thickness and supply rate in High Mountain Asia, Communications Earth and Environment, <https://doi.org/10.31223/X5WW5B>, 2021, preprint.'

In text: 'McCarthy et al. (2021, preprint)'

[EC 0.04] iii) an other point by the editor: the issue raised by referee 2 of neglecting explicit consideration of dynamic debris thickening/thinning. I understand that including such explicit dynamic effects are out of scope of this manuscript and to some degree these effects are indirectly included in the parameterization. This is now well discussed in the discussion chapter (7.2 lines 501-509) which is useful. However, I think it would make the paper much clearer when you would say more explicitly already in the method explanation of the debris evolution parametrization (section 3.2.3) that dynamic thickening or thinning due to velocity changes (spatial or temporal) are not explicitly modelled but implicitly included in the debris parametrization through.....

[AR 0.04]: We added this information also in the Methods section:

l.260-262: 'Combined with $b_{z,t}$, the long-term glacier-wide mass balance $B_{(t-9,t)}$ implicitly accounts for ice-dynamical processes, e.g. thickening or thinning due to spatial and temporal changes in ice flow velocity (see section 7.2 for a detailed discussion).'

[EC 0.05] Overall this manuscript is now close to publication but the few more substantial points above (i to iii) and the mostly minor editing issues listed below (from re-review and the editor) should be addressed by the authors before acceptance of the paper.

I thank the authors for their collaboration and detailed response of how to address the points raised by the referees and congratulate them for the paper.

Andreas Vieli, editor
19 march 2022

[AR 0.05] We thank also the editor for his collaboration and valuable suggestions.

Minor comments by editor

[EC 0.06] Line 4: '....and implement IT (not IS) as a module...'

[AR 0.06] Corrected

[EC 0.07] Line 95: delete the comma between 'glacier-specific' and 'Oestroem curves'

[AR 0.07] Corrected

[EC 0.08] Line 236: do you mean 'number of elevation bands h'? rather than amount, because in eqn (6) you use the number symbol.

[AR 0.08] Corrected. Yes, we meant 'number of elevation bands h'

[EC 0.09] Line 254: odd sentence 'It constrained based on observations (see section 4.2).' maybe now redundant?

[AR 0.09] We deleted the sentence as indeed, it was redundant now.

[EC 0.10] Fig. 6 caption: I appreciate that the caption is now shorter and easier to read, but you should still state what the numbers are in the y axis (RGI glacier numbers???) and clarify that the results are in relation to the 'thickness tuning factor' (I assume this is the $c_{\text{thickness}}$ parameter?).

[AR 0.10] We added the missing information in the figure caption. We also changed 'thickness tuning factor' into ' $c_{\text{thickness}}$ ', so that it is now consistent with the text (cf. AR 1.04)

Fig. 6: 'Difference between observed and modelled debris thickness for the period ~1981-2008 (circles) using different thickness tuning factor ($c_{\text{thickening}}$). Glaciers ID's refer to RGI6.0 (RGI Consortium, 2017)'

[EC 0.11] Acknowledgments: it would be good to acknowledge the referees input.

[AR 0.11] We added the editor and the referees in the acknowledge:

LI: 597-598: 'Finally, we are grateful to the Editor Andreas Vieli, and the two reviewers Ben Marzeion and Leif S. Anderson for their numerous constructive remarks and suggestions.'

Re-review comments

[RC 1.01] (i) The authors have done very thorough and comprehensive revisions, and their replies (and changes in the manuscript) have clarified almost all my points. The additions to the manuscript (e.g., new Fig. 3) are also very helpful.

(ii) I am still concerned with the reliance of the manuscript on a yet unpublished paper (see below for my only remaining "major" comment). I don't have a simple solution for this, but I would recommend against publishing a manuscript as long as a key reference is not publicly available.

[AR 1.01] (i) We thank the reviewer for the very positive feedback and for the second review. (ii) Below, we have addressed all issues raised by the reviewer. The manuscript was updated accordingly.

Major comment

[RC 1.02] The description of how the debris-cover thickness is generated is much improved, and I thank the authors for making available the McCarthy et al. manuscript.

(i) I disagree with the claim that "Due to the physical nature of the procedure, the energy-balance model and the Østrem curves (see below) do not need calibration" (L115 of tracked-changes version). Agreed, the model used in McCarthy et al. is a much more complex model, and it resolves many more physical processes. However, it clearly includes (a relatively large number, I would argue) parameters; be it emissivity of debris, bulk thermal conductivity, debris heat capacity, then the turbulent fluxes which themselves depend on parameterizations, etc. The authors' claim that these parameters are not calibrated but taken from the literature misses the point: if, e.g., the surface roughness length of debris is taken from the literature, then presumably that literature used observations to calibrate the parameterization of a logarithmic wind profile.

Two thoughts on this issue:

(ii) (a) My point might be seen as nitpicking. To some extent I would agree, but the risk of readers misunderstanding the authors' claims as implying that the model in McCarthy et al. is based on first principles is considerable, and this could induce a wrong understanding of how models work. Models are always approximations and are always depending on simplifications (or parameterizations - no matter what you call it). There are, however, many different levels of complexity in models, and different complexities are needed for different tasks.

(iii) (b) It is a problem that I feel the need to start reviewing another (so far unpublished) manuscript to understand and review this one. Neither do I want this, nor can it be in the interest of the authors. I also see a risk that one paper gets published, and the other one doesn't (or potentially, after substantial revisions).

(iv) I therefore still see the dependency on McCarthy et al. as critical, and I don't think the manuscript on hand should be published before the McCarthy et al. manuscript is generally accessible. The authors will need to decide whether to cite the EarthArxiv version (implying they have to rely on a somewhat volatile source) or wait for the publication.

[AR 1.02]

(i) The sentence was poorly formulated, and we agree with the reviewer and the editor (cf. AR 0.02 a) that the sentence created more questions than answers. We reformulated the sentence into:

L.103-105: : *'Due to the physically-based nature of the procedure, the energy-balance model and the Østrem curves (see below) are not explicitly calibrated, but use model parameters that are based on literature value.'*

(ii) We agree with the reviewer. We hope that with the reformulated sentence (cf. AR 1.02 a), the risk of misunderstanding no longer exists.

(iii) We understand the reviewer's concern and fully agree that the situation is unfortunate. When preparing our article, we were not expecting the submission- and review-process of the work by McCarthy to be so slow. Based on the reviews that the article received in the meanwhile, we are however more than confident that the situation in which one paper gets published and the other doesn't will not arise. Moreover, what we know by now is that the methodology by McCarthy won't need substantial revision. Since the publication date of the McCarthy article is not yet set, however, we decided to cite the EarthArxiv preprint version (cf. AR 0.03).

(iv) See above and AR 0.03: we have opted to cite the EarthArxiv preprint, clearly labelling it as such.

Minor comments

[RC 1.03] Description of i_{debris} and k_{debris} : please add units to the parameter values so that eq. 1 works out.

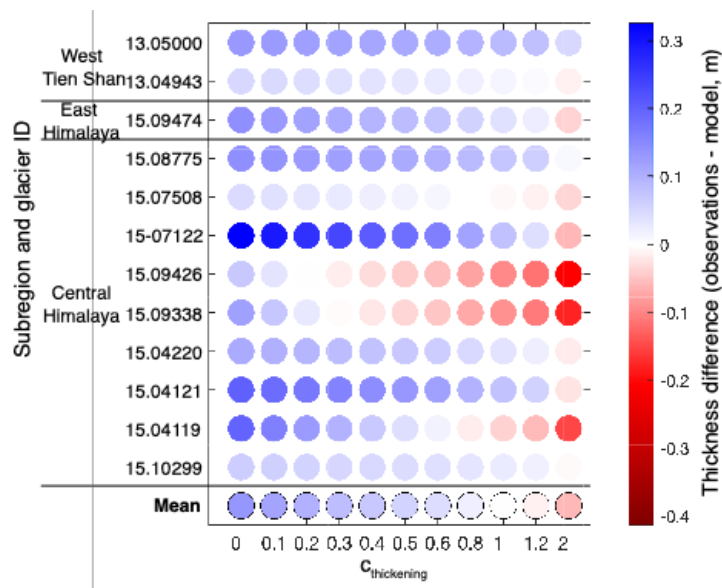
[AR 1.03]

The units of i_{debris} and k_{debris} are ‘m w.e./a’ and ‘m’, respectively. The manuscript was updated according:

ll. 113-114: ‘where b is the local surface mass balance (m w.e. a^{-1}), h the debris thickness (m) and i_{debris} (m w.e. a^{-1}) and k_{debris} (m) are glacier-specific calibration parameters without specific physical meaning.’

[RC 1.04] Fig. 6: Since uncertainties of the debris cover thickness are available (Fig.1), it would be good if significant deviations between model and observations would be emphasized (e.g., by adding a black ring). This will help understanding the range of suitable values of $c_{\text{thickening}}$ (also, please make the x-axis label consistent with the variable name in the text).

[AR 1.04] We changed the x-axis label into ‘ $c_{\text{thickening}}$ ’ to ensure consistency with the text.



[RC 1.05] Fig. R1: majority of glaciers in WGMS clean ice: good point. But apparently, there are debris-covered glaciers in the sample, otherwise the numbers should be exactly the same. Do a meaningful number of data points remain if the clean-ice glaciers are filtered out? I really think the evaluation of how the new parameterization affects model performance would be very helpful, and could be a strong point of the paper!

[AR 1.05] We went manually through all HMA glaciers present in the WGMS database, and there are only two candidate which are partially debris covered: RGI6.0-14.15990 (Fig. R1a) and RGI6.0-13.43277 (Fig. R1b). The remaining glaciers are not debris covered. However, both glaciers (1) do not have a fully debris covered tongue and (2) have WGMS mass balance measurements only for elevation bands (no point measurements available). Since we don't know if the WGMS elevation-band data refers to the clean-ice or debris-covered part of the tongue, we feel that such a comparison between observed and modelled mass balance could be misleading.

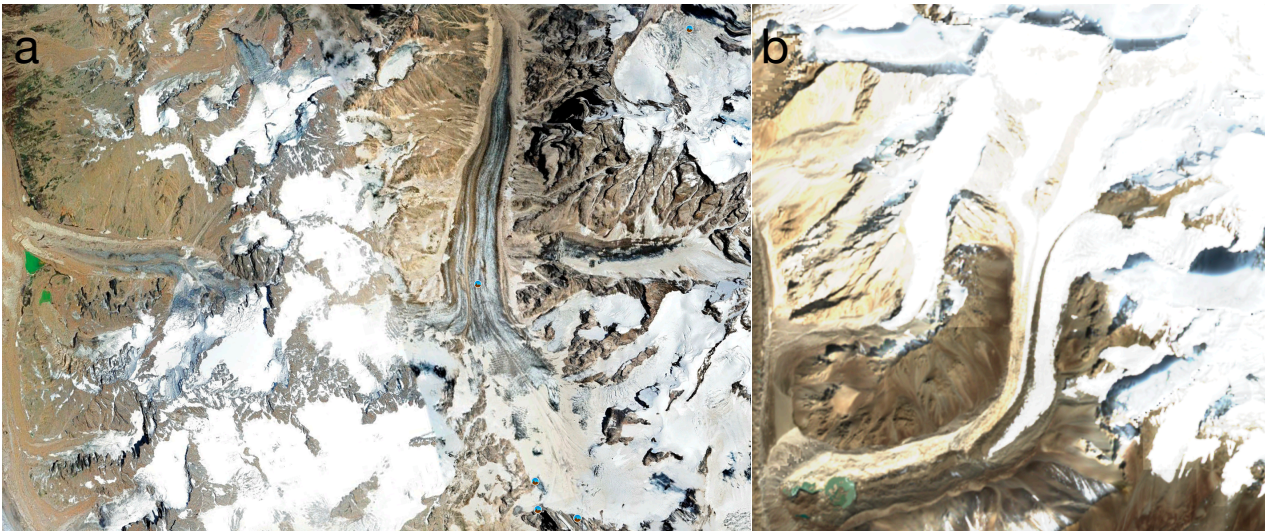


Figure R1: satellite image of (a) RGI6.0-14.15990, (b) RGI6.0-13.43277. Source: (a) Google Earth, (b) Sentinel Hub.

[RC 1.06] -Fig. S3: remove "annual" from title.

[AR 1.06] Corrected

[RC 1.07] -AR1.21(i) the colored numbers still seem to be in the plot?

[AR 1.07] This was our mistake. We now deleted the colored numbers.

[RC 1.08] -AR1.23: Sorry to insist here, but it has also not been shown that the explicit treatment of debris-cover leads to a more "adequate" capture of quantities like local mass balance, glacier length, or runoff. To make this point, e.g., for the mass balance, an improvement of the model performance on debris covered glaciers (cf. Figs. S2/R1) would have to be shown.

[AR 1.08] We changed the sentence into:

L480-482: *'Indeed, quantities such as the local mass balance, the glaciers' ice flow velocity and mass turnover, the glacier's length change or water runoff, are different when explicitly accounting for supraglacial debris and its temporal evolution.'*