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The Cryosphere Discussions

Interactive comment on "Modelling the evolution of Djankuat Glacier, North Caucasus, from 1752 until 2100 AD" *by* Yoni Verhaegen et al.

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Verhaegen and co-authors present a numerical modelling study of Djankaut Glacier, investigating the evolution of this glacier from the Little Ice Age (1752 CE) to 2100 CE. This is a small, 3 km2, glacier that has some surface debris. The focus of the paper is therefore on the impact of supraglacial debris on mass balance and how this changes over time. I have several major concerns about the work undertaken and the content of the manuscript which are detailed below. There are also numerous minor points for improvement and I have mentioned some of these in my review. My main concern the relevance of using a debris transport–mass balance model to a glacier with a thin and discontinuous debris layer, particularly when glacier length and area metrics are used to evaluate glacier change. As a modelling study of a WGMS benchmark

glacier there is much value in exploring how it will change and quantifying sensitivities to climate. However, the work in its present form lacks the scope and rigour required for publication in The Cryosphere, and the presentation of the manuscript would benefit from additional work, including adding a Discussion section.

Major comments 1. Evaluation of glacier change in terms of terminus position and glacier area. We know that debris-covered glaciers have a different dynamic response to climate warming based on remote sensing observations and numerical modelling, which shows that they lose the majority of mass by surface lowering rather than terminus recession and area reduction. Therefore, the latter metrics that are useful for clean-ice glaciers are poor indicators of the behaviour of a debris-covered glacier. My main concern with the present study is that the debris-cover model is unnecessary given the characteristics of Djankaut Glacier (e.g. large areas of visible clean ice on the tongue, steep slope below the ELA, high velocities, large changes in length and area over several decades, short response time) and therefore introduces a bias to the results. It would be valuable to demonstrate the difference between simulations with and without the debris-cover model to evaluate its impact on glacier change and if the observed change can be replicated without this additional calculation. It appears that this information is contained in Fig. 14, which shows future glacier evolution under different climatic forcings, but this is not discussed in the text and the figure is difficult to interpret; it appears that the debris has no impact on glacier length change until the second half of the century.

2. Value of sub-debris melt calculation. In relation to my point above, I have two concerns about the debris-cover model; (a) the gradient of the exponential function used to scale sub-debris melt is steep using H*debris = 1.15 m (see review Fig. 1), and (b) the thickness of debris on the glacier is similar to the critical thickness observed on debris-covered glaciers elsewhere and therefore likely to both enhance and reduce ablation across the tongue. The glacier model accounts for the impact of supraglacial debris by reducing mass balance, a valid assumption beneath debris that is thicker

than a critical thickness of about 0.1–0.2 m. An exponential function is used to reduce ablation with debris thickness. However, images of the present day glacier including Fig. 1 and data presented in the manuscript (Fig. 5a) illustrate that the debris thickness at the terminus is $\sim$1.0 m in 2010 and was <0.5 m before 1990. As debris thickness decreases rapidly upglacier (Fig. 5c), is the same scaling is assumed then most of the debris layer was <0.25 m thick before 1990 and therefore close to the critical thickness. For such thin and discontinuous debris layers, there is likely to be little reduction in ablation due to insulation by the debris layer (the exponential function used here will only reduce sub-debris melt by <20% compared from the clean-ice value – see review Fig. 1) and instead an enhancement of ablation due to the reduction in albedo of debris-covered ice compared to clean-ice surfaces. The model does include an albedo term but does not use this to adjust for the impact of debris on ablation.

2. Evolution of the debris layer. As observed in Fig. 5, the debris layer has thickened by a factor of 2–3 over the last 20 years. Djankaut Glacier is steep, fast-flowing and thinly debris-covered over a section of its ablation area, and based on this geometry and the presence of large ice-marginal moraines it seems likely that during the LIA and subsequently, the glacier exported the majority of its debris to its margins rather than developing a supraglacial layer. Therefore, the assumption in the spin up simulation that the glacier is debris covered (Section 5.1) may not hold. However, from Fig. 14 it appears that the debris layer has no impact on glacier change until about 1970 CE.

2. Lack of Discussion. The manuscript organisation is somewhat unconventional. After the Introduction, Methods and Model Description, there are four Results sections (not named as such) followed by the Conclusions. There is very limited discussion of the context of results and their interpretation, and no dedicated section for this.

Minor comments by line number Line 13. “retreat” see major comment 1 about terminus recession versus surface lowering.

L15–16. The change in glacier length and area stated here are not meaningful unless

the initial length and area are also given, or these are stated as % change.

L24–25. Vague statement.

L39. Use of “significantly” should be reserved to its precise statistical meaning, whereas here it is used for emphasis and could be replaced with “dramatically” or in this sentence the meaning would be the same if this word was removed.

L45–47. What is the glacierised area and debris-covered area in the Caucasus in km2? This is needed to indicate the context suggested in this statement.

L53. Citations to previous modelling studies of debris-covered glaciers. Please note that Rowan et al. (2015) did not use a simple parameterisation of the impact of debris on mass balance as stated here, but instead made a dynamic simulation of the feedbacks between ice flow, debris transport and mass balance using a higher-order ice flow model. The statement ending in line 64 is therefore incorrect, as previous studies have taken this approach. A citation to Wirbel et al. (2017) should also be included: Wirbel, A., Jarosch, A.H. and Nicholson, L., 2018. Modelling debris transport within glaciers by advection in a full-Stokes ice flow model. The Cryosphere, 12(1), pp.189-204.

L73. State glacier area here.

L96–98. Use metres for debris thickness values here to be consistent with the rest of the text.

L101. “Mean annual air temperature”, and “+” is not needed before the values.

L110. Explain what you mean by “1.5D” or stick with “1D” to indicate a flow line calculation. L112. Do you mean 2D rather than “3D”, i.e. a matrix calculation?

L224. Give value for H*debris, from Table 1, the value used after tuning was 1.15 m, which results in the steep curve mentioned in Major Comment 1. Also it is not clear as written here how this model compared to that presented in Anderson and

Anderson (2016) as mentioned in the Introduction, which used a hyperbolic rather than exponential function to scale sub-debris melt; h*/(h*+hdebris) their Eq. 3 with h* of 0.065 m.

L258 and 260. Unclear as written. What is the meaning of "±" before the values given for H*debris? Do these values range from –0.6 to 0.6 m?

L259 and elsewhere. One of the key references for a previous application of this model to Djankaut Glacier is Rybak et al. (2018), which is cited to justify parameter choices and to give detail about the model. However, this document is difficult to locate and appears to only be available in Russian. I was not able to use this reference to collect information about the model. At Line 259 the citation here is incorrect, as "Rybak (2018)" is not in the reference list.

L363. All the models have different time steps; 3-hourly for the mass balance model, ∼4 hourly for the ice flow model and ∼4 days for the debris transport model. How are the integrated, and what impact do these time steps have on the result when the response time is ∼30 years?

L455–459. What evidence is there for the choice of debris input parameters?

L490. Incorrect statement, see comment on line 53 above.

L508–518. Here and elsewhere, although the written text is generally clear and free of typographic errors, the writing style is rather vague and qualitative, using large lists of variables/controls without indicating their importance, and the meaning can be difficult to follow. The manuscript would benefit from editing to enable clearer, more precise statements to present the study and its results.

The code and data used are described as available on request from the author. I believe the Cryosphere now requires these to be open access in a repository.

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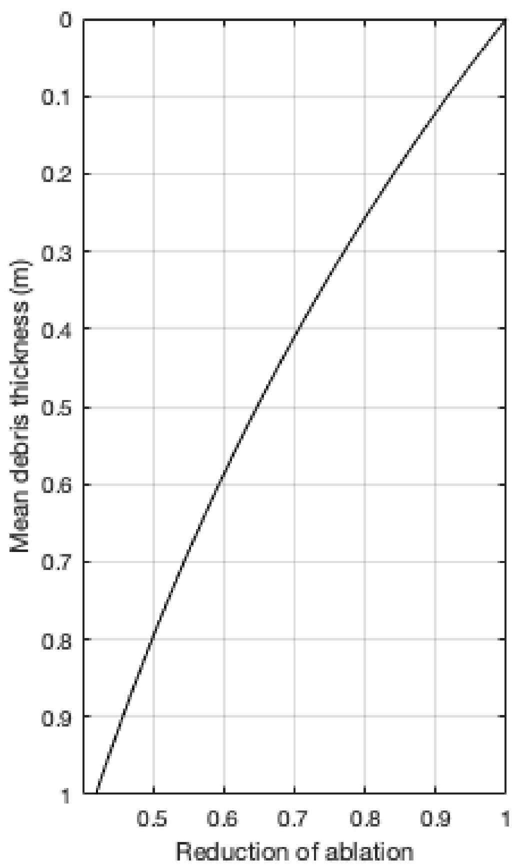

Fig. 1.