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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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1 General comments

In this paper, Verhaegen and co-authors use a flowline model to simulate the evolution of Djankuat Glacier from 1752 to 2100. I am late with this review because of the current circumstances, my apologies. This review takes the comments from two other reviewers into account, and I would also like to acknowledge the help of Matthias Dusch (University of Innsbruck), with whom I discussed the presented manuscript since it is relevant for our own research.

The manuscript by Verhaegen and co-authors is well written and comprehensive. A

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Discussion paper

substantial effort has been made to describe the models and data used, but it must be said that given the number of parameters and data sources, it is very difficult to make a thorough review of all model aspects. One could argue over certain model details (such as the choice of a 3 hr time step for the MB model for example), but I think that it is not very relevant here. Instead, I would like to make four main comments:

- **model validation**: at several places in the manuscript, the authors say that the model was validated, for example: "*It can thus be stated that the model performs well and underwent a successful validation to within acceptable accuracy*". I argue that a model is validated when its capacity to reproduce the "unseen" is assessed (past and future evolution, or unobserved variables). A model is useful when model predictions are associated with an uncertainty estimate. As it is now, the model has a very large number of free parameters which are calibrated to match observations almost perfectly. Per design, the study does not allow validation with independent or out-of-sample data (e.g. cross-validation). I don't think that it is possible to change this aspect of the study at this stage, but I would like to see the problem of model uncertainty and over-calibration discussed in the manuscript, and the statement that the model has been successfully "validated" should be changed to "calibrated". I think that the consequences of parameter equifinality are most likely to be seen in the sensitivity experiments of the debris cover parameterization and the future projections.
- **added value of the past simulations**: the model is dynamically tuned to fit observed length changes, with a time varying bias parameter. I am aware that this has been done before (and will be done in the future), but I have to ask: in the end, what is the added value of such a simulation? What do we learn from it, that we didn't already know from length change observations alone? What are the implications of the dynamic parameterization for the future projections?
- **debris cover parameterization**: in my opinion, the true added value of this study

lies in the coupling of a debris parameterization with the flowline model. I think it would add great value to the manuscript to extend the sensitivity analyses to the past glacier simulation as well (which, as it stands, is of very limited usefulness). How is the past glacier evolution changed by the inclusion of debris cover? In order not to make this paper even longer, I would suggest to remove Fig. 7 to 9, which are quite qualitative.

- **Code and data availability**: you write: "*the refined debris cover implementation can be used for comparable glacier models in future research*". I agree! But it would be considerably more useful if the code and data used in this study would be made freely available under a proper software license and in a public repository. Platforms like zenodo.org will preserve the version of the model as it is at the time of this publication. And it will create a DOI to make it citable for future research. See TC's data policy: https://www.the-cryosphere.net/about/data_policy.html

Altogether, it is still a nice study and a useful addition to the literature.

2 Specific comments

Abstract L10 : I would prefer not to use the term "1.5D". I never understood what the "0.5" is referring to: the widths? The vertically integrated velocity? Should a 2D SIA model then be called a 2.5D model? I think that a "SIA flowline model" is explicit enough.

Figure 1 : If possible, indicate the location of AWS Adylsu Valley

Figure 2 : It is misleading to compare length changes like this, because they all have a different zero baseline. It would be much better to plot them all as relative length change since year X (e.g. since year 2000).

Figure 6a and chapter 5.1 : Could you elaborate on why the 1752 steady-state glacier has a longer and thicker tongue while the ice thickness above 3000 m a.s.l. is more or less equal to the 2009 glacier?

Figure 10 and 11c : I assume the black lines are rolling means. Please specify years.

L 106 : Please specify which data period and parameters are available at these stations.

L 134 : The time step is fixed at 0.0005 years. Did any stability considerations or tests go into this choice?

L 147, 179, 183 : Which time period was used?

L 149 : Was the winter temperature lapse rate solely chosen based on the reported ELA temperature by WGMS (2018) or was AWS data used as well?

L 150, 179, 191 : It is often not clear if data from the AWS Djankuat or the AWS Adylsu Valley is used.

L 150, 250 : Is the precipitation scaled to match one of these AWS? If yes which one?

L 162 (Eq. 7) : It might be worth noting that this melt term is only one part of the total runoff of the mass-balance model and that the rest is derived in the next chapter.

L 187 : Can you please specify how the fractional cloud cover is parametrized?

L 192 : From that sentence I would expect a Figure similar to Figure A1 of Giesen and Oerlemans (2010).

L 369 : “At first, data from the pre-observational period ...”

L 370 : Terskol time period is already specified in Table 2

L 373 : How was the available data repeated into the past? By just copying the entire time period? Shuffling of individual days/months/years? Were any sensitivity tests made in that regard?

L 374 : Terskol time period is already specified in Table 2 and line 370.

L 378-388 : This paragraph (and also L 396-400) with the listing of different dates and periods is a bit cumbersome to read. Maybe it would be better to indicate these periods in the anyway mentioned Fig. 10 and be more concise in the text.

L 393-395 (and L494-495) : The mass-balance and debris cover models were calibrated for the period 1967-2007 with the use of multiple tuning parameters to fit the observed surface mass-balance. The fact that no further dynamic calibration via mass-balance perturbations was necessary for this period can not lead to conclusions about the model performance and accuracy.

Future glacier evolution : Like other reviewers, I do not understand how the GCM climate is used in this study. Why is the linear change necessary, why not applying the GCMs delta T and delta P directly?

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