RC 3 – Fabien Maussion (rebuttal by Verhaegen et al. – tc-2019-312)

Thank you for your detailed and helpful comments and suggestions. In the text below, reviewer comments are indicated with colored background, our replies are in plain text and our changes to the manuscript are put in italic.

Response to general comments

General comment 1

>> 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.

It is correct that there are very few or no independent data to validate the model results and we agree that 'validation' is not the appropriate choice of word in this regard. In the revised manuscript this was acknowledged by reformulating the text in several places, basically substituting 'validation' by 'calibration' (e.g. Line 308, Sect. 3.3):

> *"However, as with the mass balance and debris cover model, there are no, or only few, independent data to validate our model results with a sufficient degree of certainty."*

For Line 394 (Sect. 5.2), we removed the word 'validated':

> *"It can thus be stated that the calibrated mass balance model performs well when forced with the observed Terskol climatic data, and that credibility can be assigned to the dynamic calibration procedure."*

The same was done for the statement in the conclusion (Line 495, Sect. 8):

> *"… no artificial mass balance perturbations were needed, ensuring proper model calibration and credibility."*

Furthermore, a small section was introduced related to over-calibration. Apart from a small areal fraction in the highest altitudinal zones (> 3600 meter), where data availability is limited and snow redistribution processes create complex patterns, we think that our calibration dataset is sufficiently long (39 years) to assume that the environmental conditions within the calibration window have some validity for past and future conditions (Sect. 3.1, Line 273):

> *"The calibration dataset for the mass balance model is quite long (39 years from 1967/68 to 2006/07 AD), making it credible to assume that the parameters calibrated to this period have some validity for past and future conditions as well. Apart from the high-elevation areas (> 3600 meter), where data availability is limited and snow redistribution processes create complex conditions, it can be expected that the environmental setting within the calibration window also holds for periods prior to and after the observational period. However, it must be noted that the areal fraction of this high-altitudinal zone is limited (ca. 3% of the glacier area in 2009/10 AD)."*

General comment 2

>> 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?

The dynamic calibration procedure is needed to account for imperfections in the model and the climate forcing datasets, which are generally larger for more distant time periods. Because the glacier is currently still responding to past changes of climate, geometry and dynamics, these imperfections would produce a current glacier state that deviates from the observed one, and this deviation would be carried forward in any projection. The positive aspect of our dynamic calibration is that mass balance corrections were only required for the period before 1967 AD, so that (keeping in mind the e-folding length response time of ca. 31 years) future projections have largely 'forgotten' the older artificial mass balance corrections. We further refer to general comment 2 of the Loris Compagno review (RC 1) and its responses. For clarification, the following was added to the text (Line 395):

> *"Such a procedure is needed to counteract imperfections in the flow model, mass balance model and the climate forcing. The added value of this procedure is to ensure a current glacier state that matches the observed one, as the glacier is still responding to changes in past climate, geometry and dynamics."*

And:

> *"It furthermore implies that future projections are no longer influenced by the corresponding artificial mass balance corrections, keeping in mind an e-folding length response time of ca. 31 years for the Djankuat Glacier."*

General comment 3

>> 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.

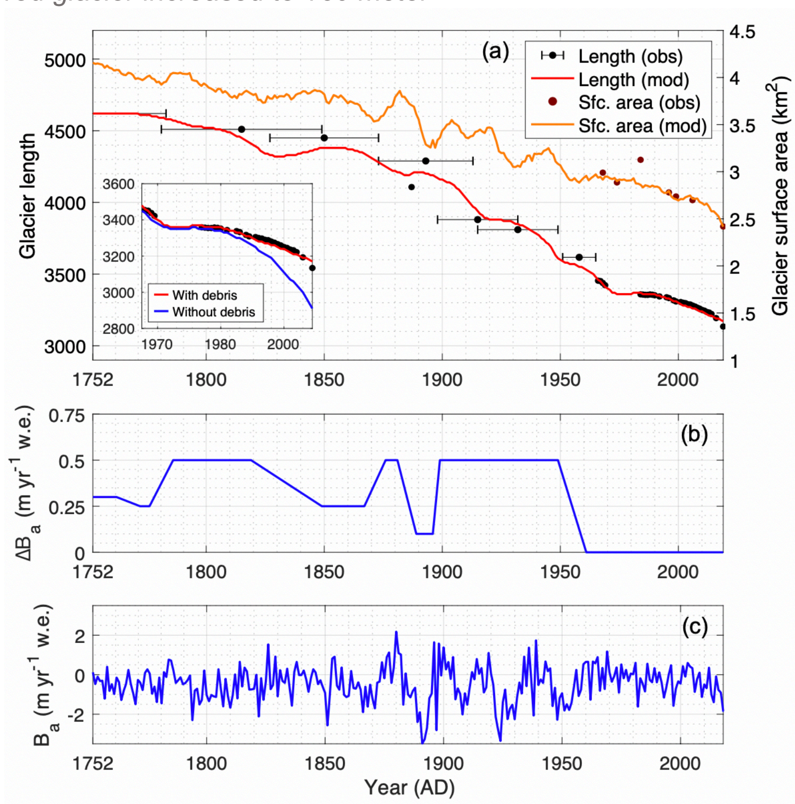
We believe that the extensive debris cover on Djankuat Glacier is a more recent phenomenon, largely linked to glacier retreat exposing debris sources. We therefore assume that the glacier was not very much influenced by debris cover prior to the observational period (1967/68 AD). In that sense, an experiment related to historic debris characteristics was carried out by executing a model run for the historic period, both with and without the debris parametrization. Both model runs exhibited very similar results prior to the observational period. As shown in the new inset in Fig. 9 below, debris played only a minor role prior to ca. 1980 AD, with length differences of only 20 to 40 meter. In this regard, the following was added to the paper (in Sect. 5.1):

> *"As can be deduced from the large lateral moraines in the Adylsu Valley (Fig. 1) and fast-flowing nature of the paleo-glacier tongue in the valley (up to 100 m yr^{-1} around 1752 AD, Fig. 6d), Djankuat Glacier used to export most of its debris to the margins rapidly in the historic period, rather than developing a supraglacial debris cover. Furthermore, debris sources from surrounding topography were likely less widespread in the historic period because the slopes were covered by the glacier itself and were more stable in a colder climate. For this reason, supraglacial debris*

is believed to have been much less widespread prior to the observational period of 1967/68 AD, implying that the glacier was not very much influenced by debris cover in the historic period."

And (Line 409):

"A historic model run conducted with a 100 % clean-ice glacier, shown as an inset in Fig. 9a, revealed that debris played only a minor role prior to ca. 1980 AD, with length differences of only 20 to 40 meter. By 2009/10 AD, however, the modelled length difference between a debris-free and debris-covered glacier increased to 160 meter"



Updated Figure 9. Historic variations of (a) the modelled and observed glacier length of the Djankuat Glacier since 1752/53 AD until 2017 AD, (b) additional mass balance perturbations ΔB_a used in the dynamic calibration procedure and (c) reconstructed time series of the total annual mass balance Ba of the Djankuat Glacier with changing geometry. Observed length variations are derived from lichenometric dating of moraines in the valley, historic documents, field measurements and recent satellite imagery (Boyarsky, 1978; Zolotarev, 1998; Petrakov et al., 2012; WGMS, 2018). An additional model run for a 100% clean ice glacier is shown in the inset in panel a.

To limit paper length, we removed Figs. 7 to 9 of the original paper and replaced it with a single all-encompassing figure regarding glacier sensitivity (see also our response to general comment 3 of reviewer RC 1).

General comment 4

>> <u>Code and data availability:</u> 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

You are right. To comply with TC's data policy, we now make the model code publicly available via GitHub/Zenodo. The model code that served for this research can be found and downloaded from: https://github.com/yoniv1/Djankuat_glacier_model. The code placed here is a 1D coupled ice flow-debris cover model. It uses bedrock geometry together with a parameterized mass balance profile to calculate the ice thickness evolution on a grid with spatial resolution dx for the Djankuat Glacier, and also takes into account an evolving supraglacial debris cover until a steady state situation has been reached. Our code availability statement now reads:

> *"Code availability. The coupled ice flow-supraglacial debris cover model for the Djankuat Glacier used in this research was written in MATLAB_R2019a. It can be downloaded from the GitHub repository at: https://github.com/yoniv1/Djankuat_glacier_model, doi: https://doi.org/10.5281/zenodo.3934612."*

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.

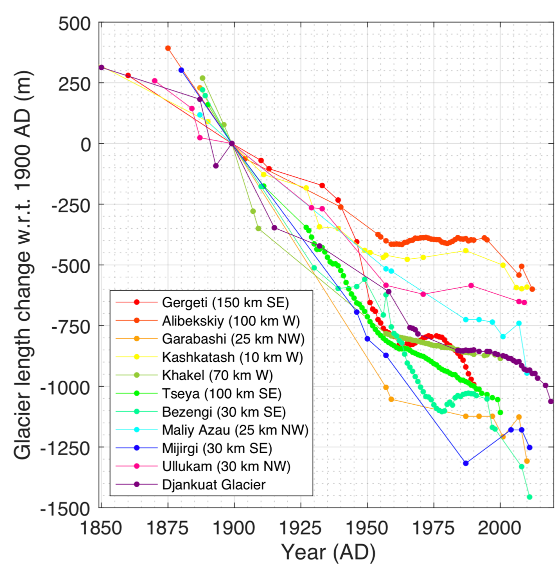
We have replaced the term '1.5D' in the original manuscript and now call the model 'flow line model' in the new text, so that its 1-dimensional nature can be derived explicitly from its name.

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

Done.

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

The figure was updated, where distances and direction to Djankuat Glacier are added. Balance years are changed to calendar years and all length changes are now relative to 1900 AD:



"New Figure 2 in the updated manuscript. Historic length variations of the Djankuat Glacier compared to other glaciers in the Caucasus (Solomina et al., 2016; WGMS, 2018). Approximate distances and direction to the Djankuat Glacier are indicated."

>> 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?

This is a well-known characteristic of glacier retreat. Retreating glaciers thin their ablation areas with little effect above the equilibrium line, which is well reproduced by the model.

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

These are 15-yr moving means. This was added to the figure caption.

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

We used data from 2 AWSs (one in the Adylsu Valley near the LIA extent of the glacier, AWS 1, and one in the glacier ablation zone, AWS 2). We used data from AWS 1 for precipitation comparisons between Terskol and the Adylsu Valley, and data from AWS 2 to derive transmissivity, temperature lapse rates, albedo, and shortwave, longwave and turbulent fluxes. These AWSs were only operational during the summer months (June to September) between 2007 and 2017. This was clarified manuscript as follows:

> *"In 2007, two automatic weather stations (AWS) were additionally installed, one in the Adylsu Valley at ca. 2640 m elevation (AWS 1 in Fig. 1) and one in the ablation zone of the glacier at ca. 2960 m on a sparsely debris-covered ice surface (AWS 2 in Fig. 1). During the summer seasons (June to September) of 2007−2017, a wide range of additional meteorological variables have therefore been acquired by AWS 1 and 2 (air temperature, dew point temperature, incoming and outgoing shortwave/longwave radiation, relative humidity, wind speed and direction, air pressure and for AWS 1 also precipitation amounts) (Rets et al., 2019). The AWSs did not operate outside the JJAS period."*

The location of AWS 1 and 2 were added to Figure 1 of the original manuscript.

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

The following was added to the updated manuscript (Line 135):

> *"…with Δt of 0.0005 years, as determined by the CFL-condition for diffusion problems".*

And Line 221:

> *"…with Δt = 0.01 years, in accordance with the CFL-condition for advection problems"*.

>> Line 147, 179, 183: Which time period was used?

We used the data from the AWSs, which were only operational during the summer seasons of 2007-2017. We refer to comment Line 106 above.

>> Line 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?

The AWSs were not operational outside the June to September window. This was clarified:

> *"Due to lack of AWS data outside of the June to September period, a temperature lapse rate of -0.0049°C m^{-1} was used for the winter half-year (1 Oct – 31 Mar), in accordance..."*

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

We used data from AWS 1 for precipitation comparisons, and data from AWS 2 to derive transmissivity, temperature lapse rates, albedo, and shortwave, longwave and turbulent fluxes. We specifically referred to each respective AWS when data are discussed in the text. For Line 147:

> *"Hence, a direct comparison of measured air temperatures between AWS 2 on Djankuat and the Terskol weather station was found…"*

For Line 150:

> *"In this study, a value for f_e of 1.5 between Terskol and the Adylsu Valley was found after a comparison of precipitation amounts from AWS 1 in the glacier valley."*

For Line 179:

> *"Measurements of the incoming solar radiation from the AWS 2 were used to derive atmospheric transmissivity…"*

For Line 185:

> *"The ice albedo α_{ice} can, according to raw data from the AWS 2, vary between 0.15 and 0.40 depending…"*

And Line 191:

> *"Here, these fluxes, as derived from AWS 2, are added up and plotted analyzed against air temperature following the method…"*

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

Precipitation between the Adylsu Valley and Terskol was scaled using a factor f_e, using AWS 1. For the precipitation gradient, we did not have reliable data to extrapolate the precipitation from the Adylsu Valley over the entire glacier. We therefore used the accumulation profile to tune the precipitation gradient. It is clarified where the factor f_e comes from:

> *"In this study, a value for f_e of 1.5 between Terskol and the Adylsu Valley was found after a comparison of precipitation amounts with AWS 1 in the glacier valley."*

Secondly, it was explicitly stated that the precipitation gradient γ_P is used as a tuning parameter:

> *"… by making use of a vertical precipitation gradient γ_P, of which the latter is used as a tuning parameter due to a lack of data (see Sect. 3.1)."*

>> Line 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.

Done:

> *"It must be noted that the melt term M is only one part of the total runoff RO of the mass balance model (see Sect. 2.5)."*

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

It was done using a linear relationship between the cloud cover and net longwave radiation. The following was added:

> *"These were derived from an approximately linear relationship between the cloud cover and the net longwave radiation balance (Voloshina, 2002), of which the latter was measured by AWS 2 on the glacier surface."*

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

To avoid confusion, the choice of words was adjusted. In the text, this was modified as follows:

"… fluxes, as derived from AWS 2, are added up and analyzed against air temperature…"

>> Line 369: "At first, data from the pre-observational period ..."

Done. We changed *"for"* to *"from"*.

>> Line 370: Terskol time period is already specified in Table 2.

The text *"(1977−2013 with a data gap between 1990−1997)"* was deleted.

>> Line 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?

As mentioned in the manuscript, the data sequence for Terskol over which measurements with a 3-hourly interval are available (1977−2013 with a data gap between 1990−1997) was repeated into the past and future in order to maintain intra-daily and intra-annual variability in the data. These 30-year sequences were copied / pasted until the entire time series had been covered. Afterwards, they were adjusted for the monthly temperature and precipitation data that were obtained with the climatic reconstruction and future projections. We did not carry out a sensitivity analysis as we think the data sequence is long enough to encompass inter- and intra-yearly variability.

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

The text *"(1977−2013 with a data gap between 1990−1997)"* was deleted.

>> Line 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.

The subsection was shortened to:

"Especially during the last few decades, an accelerated warming trend has occurred, as the latest 10-year climatic interval exhibits a mean annual temperature anomaly of +0.5°C compared to the 1981−2010 mean. This makes it the warmest period in the reconstructed time series. For temperature, a clear sequence of colder and warmer intervals can be seen. Changes in precipitation show a sequence of drier and wetter periods (Fig. 8)."

>> Line 393-395 (and 494-495): 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 cannot lead to conclusions about the model performance and accuracy.

See general comment 1.

>> 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?

We have now recreated the future climate forcing directly from available CMIP5 models for the grid cell closest to Djankuat Glacier. We therefore used a multi-model mean approach using 21 Global Circulation Models. We also applied a de-biasing procedure to match the future climate forcing with the past, both concerning the trend and the variability. See general comment 1 from RC 1.