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The Cryosphere
Discussions

Interactive comment on "Global application of a surface mass balance model using gridded climate data" *by* R. H. Giesen and J. Oerlemans

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Author response to interactive comments by Referees #1 and #2

We like to thank the two anonymous referees for their positive reaction on the manuscript and suggestions for further improvement. Below is our response to all comments.

In addtion to the changes suggested by the referees, we added two more glaciers to the analysis in the paper. The mass balance profiles for these two glaciers on Svalbard only became available from WGMS recently. One of these glaciers is Kongsvegen and of particular interest, since we already showed measured and modelled results for an AWS on this glacier. We have therefore also replaced the mass balance profiles for

Austre Brøggerbreen in Figs. 3, 4 and 8 by profiles for Kongsvegen.

Referee #1

General comments

1. The title does not fully reflect the content of the paper. To my opinion, it would be more appropriate to use e.g. "Global calibration of a surface mass balance model driven from gridded climate data". I believe the title should reflect that the manuscript is mainly dedicated to investigating the calibration process.
We agree with the referee that a large part of the manuscript is dedicated to issues encountered during the model calibration, which is not reflected in the manuscript title. The title has been changed to 'Calibration of a surface mass balance model for global-scale applications'.

2. I believe the manuscript would benefit from including a global map showing the locations of the 80 glaciers and of the AWS sites. Such a map should be at least placed in the supplement but preferably in the manuscript.
We have added an additional figure to the manuscript showing a map of the locations of the AWS sites and the now 82 mass balance glaciers.

3. I see the major possibility for improvement in revising the "Conclusions and Discussion" section. While the very detailed and thorough calibration goes one step further with respect to previous work and raises a number of important questions that have not been addressed previously, this step is not obviously reflected in the discussion section. To my opinion the manuscript raises the question whether a reasonable calibration for areas with few measurements (e.g. Central Asia) is possible and can be justified. On many glaciers the tuning parameters become unrealistic after calibration (e.g. extreme variability of T_{tip}, ψ_{min} within the regions). Such unrealistic parameter combinations are also present in other global studies (e.g. Schneeberger et al., 2003; Radić and Hock, 2011). This would offer the great opportunity to discuss how to deal

with such calibration results. To my opinion, more space should be devoted to discussing the calibration and the consequences of the difficulties and uncertainties of the calibration. Other aspects, such as for instance comparing climate sensitivities of different models, have already been investigated in a number of studies. Although still being worth mentioning, they are of less importance here.
The discussion has been revised, the comparison of mass balance sensitivities with other studies has been removed and more attention is given to the effect of the calibration procedure on the modelled mass balance and sensitivities. We discuss the problems in Central Asia in more detail and indicate that calibration of the temperature-dependent flux relation in this region is required for a more reliable model performance.

4. In line with the above statement I believe that the authors somewhat miss the opportunity to give a more thorough discussion of the unexpected positive relation of τ and P (most obviously shown on Page 1460, Lines 16 to 19 and Figure 7) but also of T_{corr} and P. Is there a systematic issue with the model, the design of the calibration procedure or do the CRU data have systematic issues? Would it be an advantage to define and prescribe realistic parameter ranges (e.g. for τ smaller than 0 to 1) and if this range is exceeded then the calibration is considered as failed?
The change in parameter values needed to match observed annual mass balance is primarily dictated by the difference between observed and modelled annual mass balance after the precipitation parameters have been calibrated. For unknown reasons, the model overestimates the annual mass balance for glaciers in a wet climate, while it underestimates the annual mass balance for glaciers that receive little precipitation. This results in an apparent relation between τ and P, but this relation should be no means be explained as a physical relation. This is now stated in the text and possible causes for the large correction in dry climates are given.

5. The original model from Oerlemans (2001) uses a strictly linear function for $\psi = c_0 + T_{\text{a}} c_1$. Because the original model also does not include a calculation of the surface temperature, the result is unrealistic very negative ψ during most of winter.

However, these negative values prevented unrealistic runoff events in winter because at cold temperatures very large S_{net} are required to compen- sate negative ψ and induce melt. In the present model ψ is fixed to ψ_{min} for temperatures below T_{tip}. Since ψ_{min} is chosen to be -25 $W m^{-2}$ (set1 and set2 and calibrated for many glaciers) melt events in winter are more likely than with the original model. In contrast to the original model by Oerlemans (2001) the present model includes a parameterization for refreezing that can prevent runoff. However, it seems to me that in the case of ice surface or snow surface with $T_{\text{sub}} = 0^oC$ because of mean annual air temperature (MAAT) $\geq 0^oC$, this mechanism does not work and runoff can occur also in winter. The same might be the case when MAAT is not much below 0^oC and T_{sub} reaches 0^oC already during winter. In reality and also in places with MAAT $\geq 0^oC$ or only slightly below 0^oC, runoff during winter is less likely because of the surface layer being cold in winter and following rather winter temperatures than MAAT. There is some indication in the manuscript (Page 1462, Line 15-19) that such unrealistic melt events indeed can prevent the build-up of a snow pack on some glaciers. What is the impact of the above described mechanism on model results and calibration? Could this be an explanation for some of the issues during calibration? Maybe it would be more appropriate to use winter temperatures rather than MAAT to define T_{sub}?
The original model indeed formulates the temperature-dependent flux as a single linear function without a fixed value at temperatures below zero. As explained by the referee, this is not a realistic representation for low temperatures, but does ensure that there is no melt at these temperatures. To obtain a more representative parameterization for temperatures below zero, we examined the relation between the temperature-dependent flux and air temperature at several weather stations on glaciers. This resulted in the adjusted parameterization with a fixed value at low temperatures, which could potentially produce melt if low air temperatures occur together with large net solar radiation. This is not a common combination, but not unusual on for instance tropical glaciers. However, in reality no melt occurs, but mass is lost by sublimation. Hence, the original parameterization underestimates the mass loss, while the new parameteriza-

tion may overestimate the mass loss at low air temperatures. When modelling glaciers where these conditions frequently occur, more sophisticated methods should be used. To determine the effect of the change in parameterization on the mass balance of the glaciers modelled in this study, we recalculated the mass balance for one of the calibrated cases (τ-cal.). The difference with the original mass balance is smaller than 0.13 m w.e. for all glaciers, for one third of the glaciers the difference is smaller than 0.02 m w.e. (see Fig. 1). Apparently, there is very little melt at temperatures below T_{tip} with both parameterizations. The effect of using winter instead of annual air temperature to compute the refreezing fraction was examined by defining winter temperature as the mean over the winter half year (Oct-Mar (Apr-Sep) for the northern (southern) hemisphere). Again, the changes in the mass balance are small, below 0.16 m w.e. for all glaciers, with a median value of 0.09 m w.e. (see Fig. 1).

Detailed suggestions

1. Page 1446, line 6: I do not fully agree with the statement that only air temperature and precipitation is required for model input. At least some information on τ is needed, also to recognize unrealistic calibrated τ values as shown later in the manuscript.
We partially agree with the referee on this point. The atmospheric transmissivity can be included as a (seasonally varying) variable like air temperature and precipitation, taken from measurements or climate model output. But here τ is taken constant, although it is allowed to vary per glacier/climate. In this sense, we believe it is more comparable to model parameters like snow/ice albedo, temperature lapse rate or the parameters in the temperature-dependent flux formulation. Their values should all ideally be based on available information, but can still be estimated within reasonable limits if this information is not available. The major differences in incoming solar radiation at the glacier locations are due to the seasonal cycle imposed by the glacier latitude, the value used for τ is of secondary importance.

2. Page 1449, line 10: replace "often not" by "rarely".

changed

3. Page 1451, lines 18-19: In principle uncertainties should always be considered, also AWS data are erroneous. Nevertheless, I understand that here the uncertainties are much smaller compared to using CRU data. I suggest slightly revising the statement.
The statement was indeed too strong and has been modified.

4. Page 1453, lines 20-25: These lines raise the question whether the "sophisticated techniques" (page 1452, line 18) used by CRU do consider the effects of different surface properties and in particular if the influence of the glacier boundary layer over ice is reflected (e.g. interpolations from coastal stations on Greenland to grid cells located on the ice sheet). Maybe this question could be briefly addressed since it is of considerable importance when using CRU data for glaciological purposes.
The spline-fitting technique employed to create CRU gridded data set does not take into account the surface properties at the weather stations and grid cells, only the latitude, longitude and elevation. For Greenland, most weather stations seem to be located along the coast, although two stations in the ice sheet interior are also indicated in the referenced paper by New et al. (2002). We added more detail about the interpolation method to the paper and mention that surface properties are not taken into account.

5. Page 1454, lines 10-11: The later frequently used "set1" and "set2" should be mentioned and introduced here in the text rather than defining the names solely in Table 2.
Parameter sets set1 and set2 are now introduced in the text at the location specified by the referee.

6. Page 1454, line 19 (and Appendix line 15): Table 5(?) is not correctly linked
The two links to Table 5 were lost during the typesetting, this was not noticed in the paper proofs. The link has now been restored.

7. Page 1460, line 4 and throughout the entire manuscript: The term "northeastern Russia" is ambiguous because (i) it might also refer to the European part of Russia

which is often referred to simply as "Russia" and (ii) because in Russia 54°N is not necessarily considered north. I would recommend using either the geographically correct "Russian Far East" or directly referring to "Kamchatka".
We appreciate the recommendation of the referee and have changed the region's name to Kamchatka throughout the manuscript.

8. 1463, line 16: this statement is puzzling since some of the Central Asian glaciers have very large C_P. On Page 1462, line 11, these values are explained. Nevertheless it remains unclear whether the statement on Page 1463 refer only to Central Asian glacier that have "realistic" modelled C_P or to all of them.
The statement only refers to the C_P values shown in Fig. 10 for the case τ-cal., which is now stated more clearly in the text. Sensitivities had only been calculated for this case, but for completeness, we calculated the balanced-state mass balance sensitivities for the other two calibration cases as well. The large values for C_P in the case T-cal. remain when calculated for the zero mass-balance situation. This is now added to the manuscript.

9. 1466, line 10: Daily steps are frequently used and probably already offer an advantage.
We do not exactly understand this remark in the context of the sentence referred to. One of the three studies referred to actually uses daily input data, the other two are based on monthly input data. For the two studies with monthly data, it is likely that the differences are due to the model set-up, while for the third study the resolution of the input data may also be a factor. We already mention in the next lines that the temporal resolution of the input data probably affects the results. Using input data with a higher temporal resolution would certainly have advantages over using monthly data.

Referee #2

General remarks

I think that investigating the effects of degrading the quality of input data on mass balance model performance is very useful, as it allows to quantify the sources of uncertainty in modeled mass balances (i.e., model-caused vs. forcing-caused uncertainty). But it is problematic to compare the calibrations based on AWS data with the calibrations based on CRU data, not only because of the time shift between the data sets, but also because of the shortness of the AWS time series. The longest AWS time series comes from Vadret da Morteratsch with 9 years coverage (table 5). Even if there was no underlying trend in the tempera- tures, one can not expect that time series $\leq$ 9 years long are able to capture the climatological seasonal cycle.
We agree with the referee that a direct comparison between the seasonal cycles from the CRU data and the AWS records is not entirely justified due to the time shift and the length of the time series. It is however also not our intention to provide a thorough comparison. We use the CRU climate data as an example of input data not measured on the glacier itself to show how much seasonal cycles can differ and what the effect is on the modelled fluxes and mass balance. Whether these differences are due to a different measurement period, setting of the measurement location or non-representative lapse rates used to extrapolate temperature data to a the glacier site is of lesser importance. To specifically address the comments by the referee, we had a more detailed look at this issue by considering the CRU TS3.1 data set, which gives monthly values for the period 1901–2009, but with a lower resolution than the CRU CL2.0 climate data. Figure 2 (below) compares the seasonal cycles in temperature for the AWS locations shown in Fig. 1 in the manuscript, for the AWS period from the AWS data and the CRU TS3.1 data set. Since the two CRU data sets use a different spatial grid, we also show the seasonal cycles for the period 1961–1990 in both data sets. To facilitate comparison, all temperature data were extrapolated to the AWS elevation with a lapse rate of 6.5 K km^{-1}. For all locations, the temperatures for the AWS period are higher than the 1961-1990 values, which could be due to a general temperature trend, but also to the specific years considered. The seasonal cycle is quite similar for both periods, although the summer months are warmer on Storbreen and the winter months are much

warmer for S5 on Greenland. Still, the main differences between the seasonal cycles at the AWS locations and CRU data, as mentioned in the manuscript are still present. Please note that for the period 1961–1990, the two CRU data sets also do not have the same seasonal cycle, differences are often even larger than with the AWS period. For consistency throughout the manuscript and because this range of uncertainties is common between data sets, we used the CRU CL2.0 data set in all experiments and did not change the CRU input data period for the AWS comparison. We added a statement to the text that some of the differences in seasonal cycles will result from the different periods considered.

I don't think that the results as presented allow the conclusion that the model is applicable in regions with a climate similar to the locations it was calibrated for (p1466 l28-29) - it may be so, but it remains to be demonstrated that the model is able to reproduce the mass balance of a glacier in the same climate who's measured mass balance did not enter the calibration process (it also implies that the model does not strictly depend only on temperature and precipitation, but also on measured mass balances for model calibration). This issue is somewhat related to the current title of the manuscript - I think the authors point to and discuss some very important issues that have to be solved before a mass balance model can be applied globally, but they don't touch on the question what to do with unmeasured glaciers, which have to be included in a global application (e.g. for sea level rise, p1447 l17).

The conclusion that the model is applicable in regions with a climate similar to the locations it was calibrated for, refers to the parameterization of the temperature-dependent flux. When combined with a calibrated winter mass balance profile, the model produces a realistic annual mass balance profile, which suggests that the ψ-relation can be used not only at the point location of the AWS, but also at other elevations and other glaciers. This is not clearly stated in this paragraph, the referee is right by concluding that the model cannot be easily applied to glaciers without mass balance measurements. We have rewritten the discussion to address this problem in more detail.

Detailed comments

p1446 l2: Whether the uncertainties depend on the availability of mass balance measurements is strongly dependent on the model choice - if a full energy balance is available, the uncertainties in the mass balance model will be small, even is no measurement is available.

We do not agree with the referee that a full energy balance model would not require mass balance measurements for calibration. It might be possible to simulate surface ablation in a realistic way, when measurements of the energy fluxes are available for multiple elevations at a glacier. Still, especially in the calculation of the longwave radiation and turbulent fluxes, parameterizations have to be used that need calibration. Usually, such detailed energy balance measurements are not available and measured ablation is used to calibrate one or more unconstrained model parameters. Furthermore, the amount of precipitation received by the glacier at different elevations cannot be modelled realistically with a mass/energy balance model without calibration with measurements.

p1446 l15: The multiplication factor is not introduced here, it is somewhat unclear what it is at this point in the manuscript.

The next sentence gives a better explanation of the multiplication factor, we have shuffled the first sentence to improve readability.

p1446 l21: I would guess the precipitation gradient depends on the magnitude of precipitation - would it be useful expressing it in percent instead of mm/(a m)$^{-1}$? (also, wrong exponent in the units).

The unit is wrong indeed and has been modified. We use absolute values to express the precipitation gradient, because these values can be used independent of the input precipitation data. Values given as a percentage of annual precipitation are only applicable together with the precipitation data chosen to derive the gradient. They need a detailed description on how the percentage was defined, for example before or after a multiplication factor was applied.

p1448 l6: I wonder whether the authors tried to quantify the benefit of including the seasonal insolation signal? Since cloudiness etc, i.e. transmissivity τ, also might have a pronounced seasonal cycle, at some locations it might (at least potentially) even hurt the model performance (see your discussion p1450 l1-4).
We have not tried to quantify the benefit of including the seasonal insolation signal, since this requires a comparison with a different method (e.g. classical degree-day modelling) and this is outside the scope of this manuscript. Since our is based on a simplification of the surface energy balance, we do not see it as an extension of the degree-day method, we regard it as a simplification of more sophisticated energy balance models. As can be seen from the measurements in Fig. 2, net solar radiation has a clear seasonal cycle and contributes more to the melt energy than the temperature-dependent fluxes. Even if τ would have a pronounced seasonal cycle as well, the seasonal variation in incoming solar radiation would not change much, perhaps the maximum shifts by one month. In any case, net solar radiation will remain the major contributor to the melt energy and the model will produce more realistic results when net solar radiation is considered as a separate term in the energy balance.

p1450 l12: "Sect. A" should be "Appendix A".
'Sect. A' has been changed to 'Appendix A'

p1451 l25: 1 May in the northern hemisphere, 1 November in the southern hemisphere.
changed according to the suggestion

p1454 l19: Link to table is broken.
The link to Table 5 were lost during the typesetting, this was not noticed in the paper proofs. The link has now been restored.

p1456 l11,17: Parameter "set1" and "set2" have not been introduced.
Parameter sets set1 and set2 are now introduced in the text.

p1458 l2: This problem might be reduced by calculating the precipitation gradient in % of annual precipitation (see comment above).

A precipitation gradient expressed relative to annual precipitation would have given a better correspondence with the measurements, but would have made the comparison less transparent. For this reason, we prefer to use absolute values.

p1467 l15: Link to table is broken.
The link has been restored.

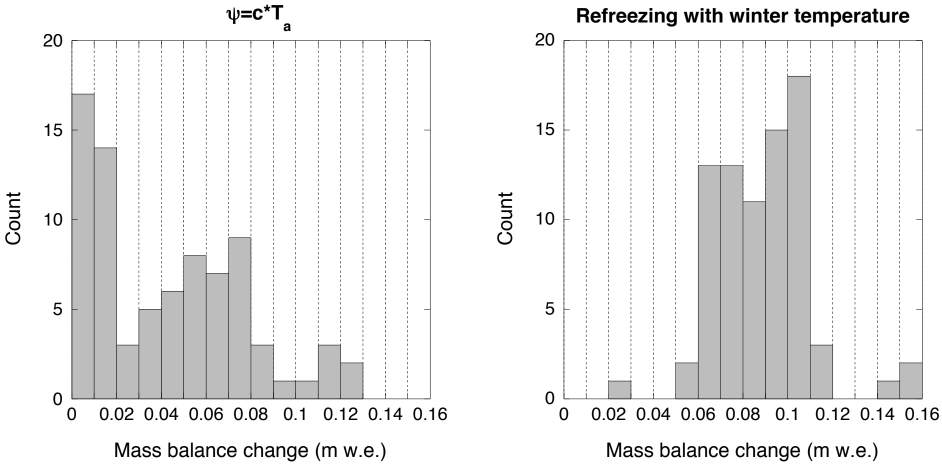
Fig. 6: The dependence of the precipitation gradient on annual precipitation is apparently not very strong - but I think it might still be worth a try of making it relative to annual precipitation.
We calculated the precipitation gradient as a percentage of the annual precipitation on the glacier, but found no improvement of the relation.

Fig. 7: The apparent correlation between τ and P_{ann} seems counterintuitive. Please discuss more detailed.
This apparent correlation should not be explained as a physical relation, it is merely the correction needed to match the observed annual mass balance. The result illustrates that the calibration procedure should not be used to derive relations between parameters. This is now more clearly stated in the text.

Figure 1 (caption): Change in mass balance for all 80 glaciers for the case τ-cal. when the temperature-dependent flux is represented by a linear function without minimum value (left panel) and when refreezing is calculated from winter temperature instead of annual temperature (right panel).

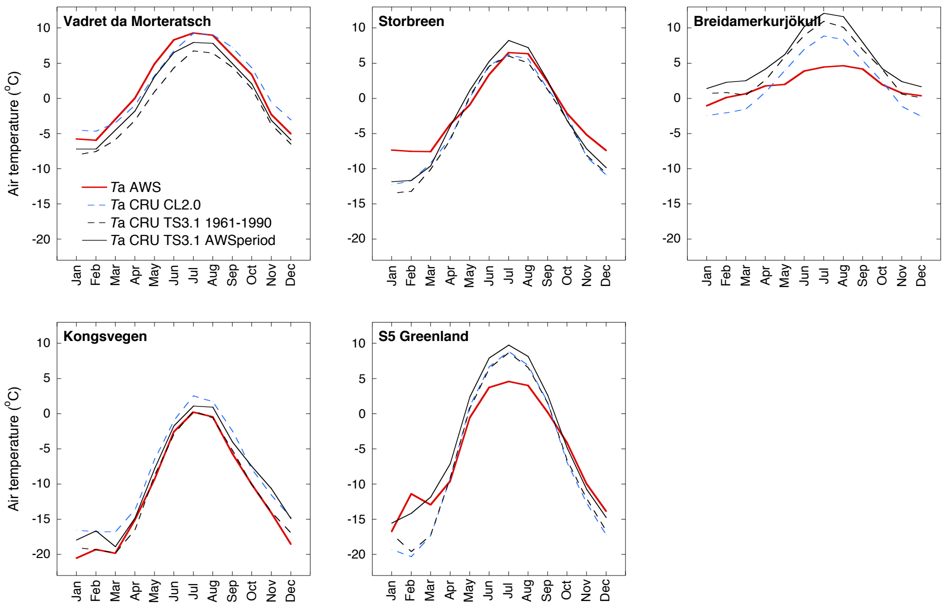
Figure 2 (caption): Comparison of the seasonal temperature cycle for the reference climate period (1961–1990) for CRU CL2.0 and TS3.1 and for the AWS period (varies per AWS) for the AWS measurements and the CRU TS3.1 data set. All temperatures have been extrapolated to the AWS elevation with a lapse rate of 6.5 K km^{-1}.

Interactive comment on The Cryosphere Discuss., 6, 1445, 2012.



ψ=c*T a
Refreezing with winter temperature
Count
Mass balance change (m w.e.)

Fig. 1.



Vadret da Morteratsch
Storbreen
Breidamerkurjökull
Kongsvegen
S5 Greenland
Air temperature (°C)
Ta AWS
Ta CRU CL2.0
Ta CRU TS3.1 1961-1990
Ta CRU TS3.1 AWSperiod

Fig. 2.