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

Anonymous Referee #1

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General Statement

Reading the manuscript from Giessen and Oerlemans leaves me with a large number of open questions; and this is meant to be mainly a positive statement. The paper addresses the very important question how a glacier surface mass balance model driven from gridded climate can be calibrated to a global sample of mass balance observations. The paper is almost fully dedicated to the calibration of the model, rather than applying the model to a global assessment of e.g. past or future mass balance. In this sense, the authors make an important step backwards compared to earlier studies. I believe that the manuscript with its focus on global model calibration is an essential contribution to global application of mass balance models. Such a study would have been a prerequisite to the already performed global computations of past and future surface mass balances. Investigating the influence of calibration on modelled mass balance sensitivity is another important aspect of the presented work. As mentioned, reading the paper raises questions regarding the feasibility of a global calibration and dealing with successful calibration resulting in unrealistic tuning parameters. To my opinion the authors could further increase the value of their study by focusing the discussion even more on the interpretation of the process of model calibration and issues therein. Although of excellent graphical quality, Figures are generally small and could be somewhat enlarged. The English seems very good and the manuscript is well structured and clear. I provide a few general comments followed by detailed suggestions:

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.

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.

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.

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?

5. The original model from Oerlemans (2001) uses a strictly linear function for $\psi = c_0 + T_a c_1$. 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 compensate 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 Wm^{-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

$T_{sub} = 0\,°\mathrm{C}$ because of mean annual air temperature (MAAT) $\geq 0\,°\mathrm{C}$, this mechanism does not work and runoff can occur also in winter. The same might be the case when MAAT is not much below $0\,°\mathrm{C}$ and T_{sub} reaches $0\,°\mathrm{C}$ already during winter. In reality and also in places with MAAT $\geq 0\,°\mathrm{C}$ or only slightly below $0\,°\mathrm{C}$, 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}?

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

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.

6. Page 1454, line 19 (and Appendix line 15): Table 5(?) is not correctly linked.

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 $54\,^{\circ}$N is not necessarily considered north. I would recommend using either the geographically correct "Russian Far East" or directly referring to "Kamchatka".

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.

9. 1466, line 10: Daily steps are frequently used and probably already offer an advantage.

References

J. Oerlemans. *Glaciers and Climate Change*. A.A. Balkema Publishers, Lisse, 2001.

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Interactive Comment

C. Schneeberger, H. Blatter, A. Abe-Ouchi, and M. Wild. Modelling changes in the mass balance of glaciers of the northern hemisphere for a transient $2{\times}CO^2$ scenario. *Journal of Hydrology*, 282(1–4):145–163, 2003.

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