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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

Anonymous Referee #2

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The manuscript is well written and the figures are clear and support the main conclusions (except maybe one, see general remarks below), but I think readers will be mislead by the title of the manuscript: The authors address the question of how to calibrate a mass balance model correctly in order to evaluate its global applicability, rather than actually applying it globally. There are few papers that investigate the calibration of a mass balance model in such detail as this one, and I think it is justified to dedicate a paper to the calibration process rather than focusing on the application. However, some important steps remain before the mass balance model presented in the manuscript may actually be applied globally, and this should be reflected in the title.

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 temperatures, one can not expect that time series ≤ 9 years long are able to capture the climatological seasonal cycle.
- 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).

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.

- p1446 l15: The multiplication factor is not introduced here, it is somewhat unclear what it is at this point in the manuscript.
- 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).
- 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).
- p1450 l12: "Sect. A" should be "Appendix A".
- p1451 l25: 1 May in the northern hemisphere, 1 November in the southern hemisphere.
- p1454 l19: Link to table is broken.
- p1456 l11,17: Parameter "set1" and "set2" have not been introduced.
- p1458 l2: This problem might be reduced by calculating the precipitation gradient in % of annual precipitation (see comment above).
- p1467 l15: Link to table is broken.
- 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.

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- Fig. 7: The apparent correlation between τ and P_{ann} seems counterintuitive. Please discuss more detailed.

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