

Interactive comment on “Glacier surface mass balance modeling in the inner tropics using a positive degree-day approach” by L. Maisincho et al.

Anonymous Referee #2

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In this study, the authors evaluate the suitability of a degree-day model to simulate melt on the Antizana Glacier 15 in the Tropics. In order to judge this contribution as it is, I did not read the companion paper in TCD, nor did I read its reviews.

While I don't have much comments about the details of the implementation and of the proposed analyses (which are carefully conducted), I am very much concerned about the scientific value and the significance of this study. In its current state, the motivation of this study is not clear and I also had much difficulty to read the paper with enthusiasm. It is long and contains numerous validation analyses adjoined with further analyses in the supplementary material, all of them without a clear message other than yes, it might also be possible to use degree-day models in the tropics. To clarify my

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position: I am not opposed to degree-day models. They have been useful for decades, and will continue to be useful for the decades to come. However, in 2016, I am not convinced that this message requires an entire paper. The “simple equation based on wind speed to compute sublimation” is a linear function of wind speed multiplied by a regressed coefficient and, as such, not a reason enough to publish a new paper. If sublimation is responsible for only a few percent (<4%) of the mass-balance at Antizana (and in the context of all other uncertainties), does it even make sense to give it such a weight in the paper and the abstract?

I strongly recommend to either find an application for the model or to define a clear research question motivating this study. One of the most interesting questions (why is the model working?) is currently found in the supplementary material, which doesn't make sense to me. In turn, the multiple validation analyses could be condensed and serve the purpose of, for example, providing uncertainty ranges to the model results. I also suggest to change the title to reflect the research question of the study: titles of the type “Modelling of ...” aren't very appealing, unless the presented model is new.

Specific comments:

L51: “local variations in temperature suggests that local warming [...] has played an important role in glacier retreat since the 1950s”. Even if I could read this from the study you refer to, I think it would be useful to be more specific here: covariability between two variables doesn't imply causality: if possible, can you shortly summarize how the glacier retreat was linked to the temperature increase in the study by Francou et al., (2000)?

L148: Pearson correlation between highly skewed variables such as daily precipitation is inadequate. Other metrics based on contingency tables or biases would be more informative.

L169: “the latent heat (LE) and wind speed are indeed closely correlated at a daily time scale”. I assume that LE was computed using the bulk approach, which is indeed a

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linear function of wind-speed.

L317: Sublimation. The bulk approach computes LE out of a linear function of the water vapor pressure deficit and wind speed. If there is a seasonality in the water vapor pressure it might be useful to include it in your sublimation model too.

L331: I am more familiar with the word “deposition”?

Section 5: it would be good to have a thorough discussion about the uncertainty of the SEB data which is used to calibrated the DDM.

Section 5: I understand why the albedo threshold is needed for the calibration, but did I get it right that for the cross-validation procedure, the threshold is then also used to *run* the DDM? If this is the case, this is not correct in my opinion: during the evaluation the model should run in “real” conditions, i.e. without external knowledge about whether the surface is ice or snow covered.

L348: daily melt or daily ablation?

Fig. 4: if possible, I would welcome a visualisation of the same data but showing the inter-annual variations of mass-balance, not the cumulated values.

Table 6: at some point in the many validations one gets a bit lost: I guess that for Table 6 you are talking about the specific mass balance?

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