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The Cryosphere Discussions

Interactive comment on "On the interest of positive degree day models for mass balance modeling in the inner tropics" *by* L. Maisincho et al.

Anonymous Referee #1

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The present paper attempts to demonstrate the usefulness of the PDD approach for mass-balance modeling in the inner tropics. The intended prospect of the authors is to generate a model that can be used in the future for paleoclimate reconstructions or glacier impact assessments in future climates. I have a mixed opinion about this study. On the one hand, I appreciate the authors' efforts to collect field data at the study site, which makes Antizana Glacier certainly a key spot for tropical glacier studies in equatorial America. On the other hand there are some serious flaws in the modeling approach and, in my opinion, this study isn't yet developed enough and comes a bit too soon. My recommendation to the editor is therefore rejection, and the comments below shall clarify this assessment.

(A) QUESTIONABLE PHYSICAL BASIS

Any detailed energy balance study on mountain glaciers I am aware of - from low to high latitudes (and not just in the tropics!) - basically shows that net shortwave radiation is (a) the dominant energy source AND (b) the most variable energy balance term (in absolute numbers). Air temperature will therefore play a role for almost any mountain glacier (except for those clearly above the mean freezing level altitude), since air temperature changes are able to disturb this dominant term in the energy balance through altering the precipitation phase and thus the surface albedo. However, a central thought in the PDD approach concerns the annual cycle in air temperature at mid and high-latitude sites, which causes great differences in PDDs between winter and summer. This central foundation is not met in the tropics, where annual temperature cycles are small and distinct moisture seasonality promotes the importance of sublimation in the mass-balance too (a term not considered in the PDD approach). The fact that the authors can reproduce measured mass-balance estimates to a certain extent is certainly the somewhat unique environment of Antizana Glacier, where relatively moist conditions are found throughout the year. I therefore disagree with the statement on page 2664/L5 that the results may promote "widespread application". Equatorial glaciers in Africa will certainly be less suited for PDD modeling, since there are pronounced dry seasons, and the majority of tropical glaciers are in the outer tropics of South America, again with well pronounced dry seasons. Even in the less strong "dry" period on Antizana the PDD approach fails, as noted by the authors (although the quantitative importance is limited due to the particular conditions at this site). In summary, my opinion is that the PDD approach is in principle not suited for the tropics by definition, philosophy, and physical background. New concepts and ideas are required to apply simple models to the tropics. It is disappointing that the present study is not heading in such a new direction.

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(B) FLAWS IN THE STATISTICAL PROCEDURE

(1) A central criterion for the model is the correlation between temperature (daily mean or cumulative values) and daily melt amounts (Fig. 3). In addition to the technical problems in this regard (see item 2 below), my impression is that the authors are overconfident in their interpretation. The key here concerns page 2652, where the authors state that the correlation between cumulative temperatures and measured melting (Fig. 3b) is similar to the results from a full energy balance model (Fig. 3c). However, for the most reliable case (blue; where the likelihood is high that comparable surface conditions were found at the AWS and the melting boxes, i.e. ice) the R2 is much higher for the energy balance model (0.84) than for the PDD approach (0.58). Assessing these two numbers as "similar" seems a questionable interpretation. Moreover, the mean daily temperature (Fig. 3a) performs obviously worse than the energy balance approach (Fig. 3c).

(2) Even more important with regard to the central assumption that there is a clear correlation between air temperatures and melting, is the absence of a statistical significance test. If there are rather few data points for a regression analysis, as is the case in Fig. 3 for the different colors, a standard significance test must be added to check whether the regression is "real" or by chance. I requested the data for Figs. 3a/b and made such a test (based on t statistics). For mean daily air temperature, the correlations with snow and dirty ice cases do not pass the 95% confidence level and are therefore not significant. Their respective p values are 0.25 and 0.13 (i.e., >0.05). Hence, the existence of the claimed correlation between air temperatures and daily melting is not fully supported by statistics.

(3) Most likely due to issue (2) above, the term "significant correlation" is used incorrectly throughout the manuscript. Whenever your model is based on statistics, you must use the term "significant" only for a statistically significant relation (i.e. for one that passes the significance test). In this paper the authors seem to use "significant" as a synonym for "strong" or "clear" or "real". For example, on page 2653 the authors state "Except during Period 1 when high winds and low moisture induced high turbulent

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heat fluxes, the cumulative hourly positive temperature values were significantly correlated with incident shortwave radiation and with the net shortwave radiation..." Due to the missing significance test, the reader can only guess what the authors mean by "significant". Is a R2 higher than 0.4 significant for the authors? This is not in line with generally accepted statistics. Please change your wording in this respect.

(4) Another issue tied to the low number of data points in the various classes of Fig. 3, is the fact that all data points are used for calibration but none are used for evaluation. Therefore we learn nothing about the uncertainty in the relation between temperatures and melt. Finding test data is not trivial for a small sample size, but a standard procedure in this case is so-called cross-validation, which has also entered glaciology literature more recently (e.g. Marzeion et al., 2012). This allows you to quantify the uncertainty in the regression, in this case uncertainty in the DDFs. Such an uncertainty assessment is a great addition to any statistically-oriented model.

(5) The present model does not quantify the expected uncertainty (the 20 lines discussion of Section 5 are insufficient in this context). In particular if model application in forthcoming studies is intended outside the time of measurements (for paleo or climate scenario problems), which the authors indicate in the paper, a useful model must include an uncertainty quantification.

(6) Finally, while the initial DDFs are calibrated from cumulative hourly temperatures, the final model uses mean daily temperature as input. This is (a) inconsistent and (b) a weak foundation since mean daily air temperatures fail the statistical significance test (see item 2 above). If you know from the start that cumulative half-hourly or hourly temperatures will be hardly available for future applications, you should not use it.

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(C) QUESTIONABLE MODEL QUALITY

(1) Due to the problems outlined in Section (B), some model parameters seem ques-

tionable. Most of all, the "optimal" threshold between clean and dirty ice is finally increased to 0.45, which seems very high to me. An ice surface with an albedo of 0.4 is usually not that dirty. For example, Oerlemans et al. (2009) shows that dirty ice measurements feature way lower albedos. In general, the authors do not dicuss the optimal model parameters in their physical context. This also concerns the precipitation correction factors: 0.71, 0.74 or 0.5 (depending on data input type). While, in contrast to the albedo parameter above, a precipitation correction factor <1 seems reasonable to me from a physical perspective (since convection will play an important role in the precipitation generation at that site), it should be discussed what <1 or >1 means in the real world, and whether this makes sense.

(2) A central difficulty for the reader to assess model quality is the fact that the authors compare their final results to mass-balance measurements that are not published yet. These measurements are cited as Basantes Serrano et al. (2014), but the reference list shows that this paper doesn't yet exist. Hence it is hard to understand how good the basis of the model evaluation is, since the underlying measurements have not been peer-reviewed so far. I am sure that the circumstance of the non-uniform distribution of point measurements on the glacier will be important for generating observed glacier-wide mass-balances.

(3) Also, the root-mean-square difference must be given in a comparison like Fig. 7, to get a feel for the error. If the error is at the order of the interannual variability, the model will need improvement.

(4) In Fig. 9 some improvements are achieved when DDFs for ice are changed in certain years. This necessity will be a major limitation for model application to paleo or future climate questions (since one cannot know which DDF is better in what year). As indicated above, there should be an error estimate for each DDF. The model as it stands now has definitely low predictive skill (for the past and future).

To summarize (B) and (C), in my opinion the model is not suited for the intended ap-

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plication before (a) an error estimate is included, (b) proper statistical methods are used in the model construction, and (c) model parameters are discussed in a physical context.

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(D) SUGGESTIONS FOR THE AUTHORS

I very much hope the authors take my comments above as constructive critique, since I support their general idea to produce longer-term mass-balance in the tropics from simple models. However, please throw some new, exciting ideas in the glaciology community! - instead of trying to make an old concept (PDD) work in an environment which, by nature, it is not designed for. With a lot of fitting some agreement will always be achieved with various input data (Fig. 7, 9, 10), but the real value of a simple model is a robust physical basis, and this is certainly not the PDD approach for the tropics.

Some specific suggestions are:

(1) I agree with the authors that hourly values make no sense in a simplified model and daily ones are better, but I would even go with monthly input values. This temporal resolution is sufficient in the context of estimating past climates from glacier extents or future mass-balance from climate projections. And measurements are even more common at monthly scale than at the daily one. As long as you resolve the annual cycle (for which monthly steps suffice in the framework of a simple model), you should be fine.

(2) Run an energy balance-based mass-balance model in distributed mode (i.e. for the entire Antizana glacier). If this works, it will greatly help to develop a simplified model later. For instance, the energy balance-based model will reveal the physical processes that shape the VBP of the glacier.

(3) I re-call an approach from Kaser and co-authors (e.g. Juen et al., 2007) who also tried to construct a mass-balance model for the tropics using a minimal input (monthly

temperature and precipitation). However, in order to consider the tropical climate features, they tried to relate the various glacier energy fluxes to the two input variables. I have not followed the evolution of the Kaser approach in recent years, but for me it is an example how new flavor can be added to simple models.

(4) I would not use precipitation from NCEP or other reanalysis as input! Precipitation is a so-called class C variable in reanalysis and is known to have big problems, especially for mountain regions. If it is intended to run a model with reanalysis input, I would try to express measured precipitation as a function of more reliable reanalysis variables (e.g., temperature, humidity, air pressure). This means I would construct a statistical downscaling model for precipitation.

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OTHER COMMENTS (INCLUDING TECHNICAL REMARKS)

(1) P2642/L17: "interannual climate variability" (add the the temporal reference)

(2) Equations 1 and 2: There is something wrong with the unit; you have to multiply the factor LR [K/m] with the elevation difference (z - z_ref) [m], in order to obtain a temperature unit. This means there must be a space between LR and (z - z_ref) in the equations.

(3) P2644/L14: –8 is lower (not higher) than –6.5. You mean a "stronger" gradient.

(4) P2648/L8: "grid cell" instead of "pixel"

(5) P2655/L15-18: You mean taking sublimation NOT into account? The model does not have a sublimation term.

(6) P2664/L8-9: Please be careful with statements like "... temperature is generally assumed to be the only variable that climate models correctly reproduce ..." – First, what exactly does the term "correctly" mean in this context? Magnitude, acceptable error, or variability? Second, what you can say is that temperature is most probably the

variable that climate models reproduce best. Third, climate is not only temperature and precipitation. Climate models often do a good job with regard to geopotential heights or air pressure, and thus with regard to dynamics on the synoptic scale.

(7) Table 2: 30 min, hourly or daily data? Please specify the time interval for the correlation results. Enter "not available" for the correlation AWS-G1 versus AWS-M, to clarify the records do not overlap.

(8) Fig. 3b caption: As noted by the authors in their comment along my data request, the x axis label should be changed. I suggest "mean cumulative temperature" with the unit °C per 30 min.

(9) Fig. 6 is not really necessary (text description suffices).

(10) Please stick to the glossary of glaciology for glaciological terminology. E.g., you should specify at least once that you study the climatic mass-balance, and sometimes I wasn't sure which type of accumulation (solid precipitation, net, ...) is meant.

(11) "Negative" air temperatures do not exist. Please use below 0°C or below melting point. The same problem is true for "positive", but in the special context of PDD modeling the term "positive" might wok.

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