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Interactive
Comment

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 #2

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This paper presents an interesting reworking of data collected on the Antizana 15 Glacier in Ecuador. Data originally used for surface energy balance (EB) modeling has been combined with additional meteorological data to assess the use of PDD modeling on a tropical glacier. The authors argue for the necessity of simplified models to calculate melt over longer timescales than afforded by EB models due to differences in meteorological data requirements. Whilst the subject deserves discussion, the paper needs serious reworking to become a key resource in conversations concerning model choice, and future approaches. I would recommend restructuring of certain sections of the paper, and strengthening of rationale and methods before the paper is accepted for publication.

General comments:

- I would recommend reworking of the energy balance data. On page 2645 (model description), it states that 'conduction into the ice/snow ... was ignored', which I found surprising given that several recent papers from the tropics have argued for the inclusion of subsurface processes, and additionally the incorporation of penetrating shortwave radiation (for example Mölg et al. (2008, 2009)). Additionally, the second author of the present paper led a publication using a more advanced model on blue ice in Antarctica (Favier et al., 2011), and so it should not be too onerous to remodel the data using an updated approach. Whilst I appreciate that the present paper is comparing with already published results, it is difficult to take the results as correct when there are clear issues with the approach.

- I would recommend inclusion of a short section detailing the justification of the use of the PDD method, including demonstrating the potential limitations of ignoring potentially important processes. For example, in Section 4.1. the authors describe a series of results that indicate that melting is not dependent on air temperature alone, but also on heating of subsurface layers, potentially shortwave radiation penetration among others. It would be worthwhile to identify the potential contributions from such fluxes, and to indicate to what extent preexisting conditions have an impact on melt rates, something that was not addressed in the original Favier et al. (2004) paper. Additionally, in the introduction section the authors make contradictory statements about what requirements must be satisfied in order to use a PDD model, and so I would recommend that the authors make a clearer statement about necessary physical conditions (especially page 2640).

- The method section should be restructured. Basic data should be presented before model description. Additionally, it would be helpful to include comments about any post-processing of meteorological data (for example, how was vapour pressure calculated from relative humidity, did the sensors suffer from riming? etc). Whilst I appreciate that the data has been presented before, and is about to be presented in another paper, the

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underlying methods and basic data descriptions should be available to readers without having to read a whole body of work to understand (and verify) the datasets used. Also, the following points should be noted:

i) Stations should be renamed when moved as their data is not continuous or comparable (3.3.1-3). ii) Were all data collected at 5000 m asl elevation corrected? (3.3.1-3) iii) Were other time steps considered to be able to use AWS datasets (eg monthly) instead of reanalysis data? (3.3.1-7) iv) Did NCEP1 or AWS data have a better fit? (3.3.1-8) v) Most glaciological data depends on an unsubmitted paper which makes it difficult to evaluate the validity of the results (largely because this paper has not been through a review process). This is significant as the current paper depends heavily on the unsubmitted paper as a reference. Additionally, it would be instructive to include comments about validation or error checking of data, as well as explaining what is meant by 'significant' differences between measurement approaches. (3.3.2-all)

- Whilst most of the results and discussion section outlines most of the important outcomes of this paper, I would recommend adding a subsection at the start of Section 4. The added section should outline meteorological conditions and data used, so that the reader can better comprehend what is meant by 'Period 1' and 'Period 2', in order to better understand what 'windy' means etc. Whilst this data has been previously published, it would aid understanding of the sections that follow. This section should lead into a concrete statement of why the PDD method is suitable. As it stands, the justification of model choice is weak and hard to follow. This would lead into the current section 4.1 well, so the reader never thinks to question the approach. In section 4.1 I would recommend the following, so as to clarify sources of confusion:

i) What measurements are the albedo thresholds based on? At the station, or over the lysimeter boxes? What is the error? (2651-22) ii) How was sublimation from the boxes considered? (2651-22) iii) The poor correlation of mean daily temperature and measured melting rates suggest there are other important processes acting on the boxes. How were other external factors taken into account (i.e. what is the error?). Did you think to investigate radiative penetration or subsurface heating issues? (2652-5/6) iv) What are the error ranges on the DDFs? (2652-29) v) Fig 4: No relationship appears to be significant. What are the p-values associated with the regression values?

Comments related to Section 4.2 (which follow on from comments above): i) Please include a figure or a statement related to melt amounts, also sublimation values obtained from lysimeters and energy balance modeling (2654-2/6) ii) A clean to dirty ice threshold of 0.45 seems very high. How much sediment or water etc is necessary to define a 'dirty' ice surface? What measurements/observations are these values based on? (2655-1) iii) Should emphasize that the paper is only interested in calculating melt and not total ablation. (2655-17)

Comments on Section 4.3 - 5: i) Sublimation percentage on page 2656 line 18 is useful, but would be helpful earlier in the paper. ii) The PDD sensitivity section should be expanded, and appear at the start of the results section. This analysis can be used to help justify the approach and choice of parameters.

- Finally, I would like to commend the authors on a well written discussion section. The only downfall is that by the time the reader reaches the discussion, they have serious doubts about the model, and some measurements (many of which are clarified within the discussion sections, but should have been described sooner). I would encourage the authors to rewrite the method and results sections to the same level of clarity as the discussion section.

Other comments:

As I envisage that the text will require serious reworking in order to be accepted, I have focused on specific comments that should be implemented in the revised version of this paper.

- Title: I would suggest changing 'interest' to 'use' - Standardize the glacier name. It changes from Antizana Glacier to Antizana 15 glacier to Antizana 15α Glacier, etc,

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which is difficult for the reader to follow.

References

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Interactive comment on The Cryosphere Discuss., 8, 2637, 2014.