

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 #1

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General Comment:

This manuscript offers an alternative to energy and mass balance modelling to reconstruct mass balance over an inner, tropical glacier using a slightly modified degree-day modelling approach. It is clear that considerable time and effort has been put into this research by the authors, in particular the first author who has provided a detailed account of the measurements obtained and how they have been treated. It is extremely challenging working on high altitude tropical glaciers and I do commend the authors for the amount of work that has been done – it is substantial and it is clear that by doing so they have a range of data products available to them, which provides a platform for a range of interesting studies.

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To clarify my position to the authors, I was aware prior to reviewing the manuscript that a version had been submitted previously. I took the approach to first read the current manuscript and to treat it on its merits, followed by a review of the previous comments made on the initial submission. After the first read I felt a little unsure about the motivation for developing the basic ablation model, especially as it is made clear by the authors in the abstract that there are some challenges associated with it, including but not limited to the variability in the melting factors for snow and ice over time (L41), which prevents the model from being used to predict future changes to the glaciers - and that the model should not be extrapolated to other tropical regions where sublimation is important. After reading the previous comments on the initial submission, I can see why this has happened. It is clear the authors have responded to feedback and constrained the applicability of the proposed model but by doing so it weakens the justification for developing it in the first place. The motivation for doing so must be clearer to readers – is this model really going to be useful for other glaciers in Ecuador? My feeling is that degree day modelling is not really suited for low latitude regions, which has already been expressed by previous reviewers, and it is unlikely that the approach will be adopted by others studying tropical glaciers elsewhere. Thus, to be of interest it is critical that the authors provide new information about the physical processes that control the mass balance on Antizana Glacier 15 and explain why the model might allow us to improve understanding about the relationship between glaciers and the climate system in Ecuador. This will be only possible if the data used as input are more carefully scrutinised (see comments below), in particular the precipitation measurements that are not very well constrained in the present manuscript. To help the authors achieve this I provide the following feedback, which might be useful should the paper be considered for publication in The Cryosphere.

Specific comments:

Please note that line number is referred to as (L).

1. Abstract: The motivation for developing the surface mass balance modelling ap-

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proach must be clearly outlined. Are the authors proposing that the model can be used widely on glaciers in Ecuador (L38)? Based on the uncertainties described by the authors (e.g. Section 7.2), it seems unlikely. Do the authors really recommend it to others that only have the input data that are used for the model? My feeling is that even though the authors are relatively “data rich” on Antizana Glacier 15 it was a challenge to get the proposed ablation model to adequately resolve the surface-mass balance. There are a number of reasons for this (see comments below), which in themselves are quite interesting and warrant further exploration.

2. The main purpose of this study is to reconstruct mass balance, and much of the emphasis is placed on modelling ablation using the modified degree-day model. However, the other important part of the mass balance is determining the amount of accumulation. In this reviewer’s opinion, the estimate of accumulation is likely the largest data uncertainty the manuscript. On L101 it is stated that annual precipitation at 4550 m a.s.l. ranges from 800 to 1300 mm a-1. In Mancinati et al. (2015; not cited by authors) the same approximate range is given but it is suggested to be controlled by elevation. Please clarify. If the inter-annual variability is the amount cited it must have a strong impact on mass balance, and equally so if the gradient is this large, but no explanation for this is provided. These values also don’t appear to have had any correction applied to them, and are significantly less than the suggested “corrected” mean precipitation value of 1820 mm reported on L162. I would strongly recommend that the authors provide a Figure that shows monthly variations in air temperature and precipitation to help clarify the issue (e.g. Basantes-Serrano et al., 2016, Fig. 2), or perhaps even better, a frequency distribution of both variables (e.g. see Cullen and Conway, 2015; Fig. 10 and/or 11) that would help clarify how much of the precipitation falls near the rain/snow threshold.

3. L109: I would suggest that the wind variability should also be shown to demonstrate to readers the classification of the two wind periods is appropriate. The mean values are different, but it would be useful to see what months actually control these

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differences.

4. L142-165: As noted above, the estimate of precipitation is likely one of the largest uncertainty's facing the authors and warrants much closer scrutiny given its importance to mass balance. How can the authors justify making a +76% correction on the rainfall data obtained from the rain guages? This would strongly suggest that the rain gauges are failing to capture precipitation adequately. The authors should also include the snow/rain threshold at this point (it comes too late on L294-297) and show what proportion of the precipitation falls as rain versus snow. It is stated on L297 that 70% of the precipitation is solid, but an altitude range needs to be provided (e.g. how much is solid at the top of the glacier, versus at the bottom of the glacier). Again, a frequency distribution of precipitation as a function of air temperature (e.g. see Cullen and Conway, 2015; Fig. 10), and the sensitivity of the threshold on controlling rain on snow versus snow on snow events would be useful. This issue really needs to be resolved, not just for this study but for the broader research being conducted on Antizana Glacier 15, otherwise it really puts a question mark on past and present glacier-climate research. The up-dated mass balance work of Basantes-Serrano et al. (2016, pg. 124) clearly points out that "the vertical gradient of precipitation may be higher than previously estimated and the accumulation processes (including the role of frost deposition) need to be carefully analyzed". They go further by stating (pg. 134) "i.e. precipitation increases with elevation on the glacier", which is in conflict with the assumption that precipitation does "not vary with elevation due to the small size of the glacier" (L162). Given the findings in Basantes-Serrano et al. (2016) are from some of the same authors as the present manuscript, it is critical that consistency is established. One option is that more focus in the present manuscript could be shifted to tacking the issue of how to best represent spatial and temporal variability of air temperature and precipitation over Antizana Glacier 15, which would then provide the framework to explore whether a degree-day modelling approach is feasible or not.

5. L180 and L184: Why do the authors refer to surface energy balance (L180) and

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then surface mass balance (L184) – are they different? The ablation estimated using the energy balance modelling is used to calibrate and validate the “basic” model. Are the authors not concerned that one modelled estimate of ablation is being compared to another, especially as there is a 30% difference between energy balance derived ablation and ablation obtained from “melting boxes” (L198-210)?

6. L191-196: It is stated that albedo measurements are used to obtain information about the “surface state” – basically, to determine whether the glacier is snow or ice covered, which controls what degree-day factors are applied. Does the necessity of albedo measurements to characterise surface conditions prevent this method from being applied to any other glacier without such observational data, and if so, does the suggestion in the abstract that the basic model can be used more broadly for “glaciers in Ecuador” hold?

7. L211-214: I can understand how mass balance might be established from the monthly observations, but it is not clear to this reader how accumulation and ablation are resolved if significant mass loss and gain occurs over the sampling interval.

8. L297-298: It is stated by the authors that the basic model is very sensitive to the chosen lapse rate, which will control the rain/snow threshold and the magnitude of the estimated ablation. A constant lapse rate is chosen but there is evidence that the lapse rate is seasonally variable (L307-309). This seems like a missed opportunity by the authors to carefully assess seasonal variability in lapse rate, and by doing so the spatial and temporal variability of air temperature on Antizana Glacier 15.

9. L317-342: In response to a previous reviewer’s suggestion, sublimation has been included in the model. This is a little problematic given that Basantes-Serrano et al. (2016, pg. 124) point out (as already noted) that “accumulation processes (including the role of frost deposition) need to be carefully analyzed”. This would suggest that not just sublimation but deposition plays an important role on mass balance, so a model that accounts for one (ablation) but not the other (accumulation) is questionable. The

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description of the physical processes controlling sublimation (L328-333) should be removed – these are very speculative, and the final assumption that the gradient is equal to zero contradicts Basantes-Serrano et al. (2016) recent findings based on glaciological and geodetic mass balance measurements.

10. L376-379: The difference in ablation estimates is quite large (almost 1 m w.e.) – I think some discussion as to why is necessary. It is not really adequate to just state the ablation was “slightly overestimated”. The same comment applies for the cross validation results, where the same magnitude of difference occurs (but different sign) (L388-392). Why is there first an overestimate and then an underestimate do you think?

11. L396-397: “The separation between snow and ice was not based on albedo values, but directly from the computed presence of snow at the surface” (see point 6 above). How are the albedo measurements used and please explain how the presence of snow at the surface is computed – is it the difference between the calculated ablation and accumulation at each time step from the amount of snow cover from the previous time step? Given the large step change between snow and ice degree-day factors it is critical that the snow surface is defined correctly, especially as a variable threshold for albedo between years is used (Fig. 2) and there is considerable uncertainty in the precipitation measurements (actual snow that falls).

12. L403-408: Figure 4 does not demonstrate that “the model accurately distinguished the surface states and accurately computed accumulation and ablation”. Figure 4 shows agreement between changes in surface height (y-axis). Please clarify what “worked perfectly” actually means (L408).

13. L447-448: As noted in point 9, assuming sublimation is constant with elevation is in conflict with Basantes-Serrano et al. (2016).

14. L498-503: Please clarify the statement that the calibration is not very sensitive to the albedo threshold. Doesn't albedo control what degree-day factors are applied (see points 6 and 11 – they appear to be in conflict), which the basic model is sensitive to.

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It would seem that a small change in albedo threshold would result in a step change in the melting factor.

15. L504-513: "This analysis showed that without applying a +76% correction for precipitation, as suggested by Wagnon et al. (2009), the agreement between simulated and measured mass balance would have been much worse". This reconfirms earlier comments (see point 4) that precipitation is one of the largest uncertainties in this study and remains unresolved. The scaling used might be compensating for unknown errors elsewhere.

16. L532-L539: The authors should provide further details about why the relationship between incoming shortwave radiation and ablation breaks down at high wind speeds. This implies the role of the turbulent heat fluxes becomes increasingly important, which don't appear to be captured by the model adequately. The surface energy balance data that are available could be used to assess this. A distinction between ablation and melting should be made more clearly in this paragraph, as sublimation is obviously playing a more important role as wind speed increases (ablation but not melting). Given this point is mentioned in the abstract and conclusions, it is critical that it is carefully clarified in the text.

17. Section 7.2: The description of the accuracy of the model in this section is too long and doesn't provide this reader with any more confidence about the approach. For example, on L556-565 it is stated "we found that (except for 4 days), melting was always significant, even when daily temperature was equal to $-1.7\text{ }^{\circ}\text{C}$, but nil at $-2.1\text{ }^{\circ}\text{C}$." This analysis of melt threshold falls short in carefully distinguishing when melt occurs or not, which is important given its importance in the degree-day model used. As already noted, further efforts to show the spatial and temporal variability of air temperature would help remove this uncertainty.

18. L606-611: It is worrying that there is temporal variation in the degree-day factors, and suggests that there are other physical processes that are not accounted for. The

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authors suggest that variations in albedo are a likely cause “since degree-day factors differ with the state of the surface”. This contradicts the calibration results, which suggested the basic model was not sensitive to the albedo threshold.

Final comment: The observational data presented in this manuscript are of interest to readers given the challenges to extract such information from tropical glaciers. However, the manuscript falls a little short in adequately accounting for the physical processes responsible for mass balance using the degree-day approach. It would be much more useful if the authors took a step back and reconsidered their approach by using their expertise and knowledge to fully resolve the spatial and temporal variations of air temperature and precipitation on Antizana Glacier 15, which would provide the framework to validate and/or assess a range of different mass balance models in the future.

Minor technical suggestions

The authors should consider reviewing the entire manuscript to carefully check how they use and refer to different components of mass balance (e.g. melting versus ablation). It might be useful to provide definitions of mass balance, especially accumulation and ablation for the site, given they are likely to occur on any given day of the year.

L29: provide country – Ecuador after Antizana Glacier. L35: shortwave radiation was “intense” – define “intense” – give threshold or clarify. L35: positive “air” temperatures. L99: remove “circadian” and replace with diurnal and refer to annual mean air temperature. Provide the air temperature ranges for diurnal variability and monthly values. L105: Remove simultaneously and continuously – it is very unlikely that accumulation and ablation occur simultaneously and continuously. It is quite likely that they can occur at any time of the year, but simultaneously and continuously implies something quite different. L255: Glacier 15 “is” computed. L348: daily temperature and daily “melting” – what is daily melting? The following sentence refers to ablation. Be consistent between melt and ablation or be very clear to readers what the difference is. L365:

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Remove “logically” or explain why it is logical. L379: to reproduce melting and ablation – is the model distinguishing between the two? Again, be consistent in the usage of the key mass balance terms. L387: which “is” – replace with are. L491-492: It is questionable to use only 9 data points for the linear regression between modelled and measured ELA. L 508-509: “this basic model is able to properly simulate the mass change and the melting” – again, doesn’t the model resolve accumulation, ablation and mass balance? Does it really differentiate between melting and ablation? L517: “we compared basic model melting amounts” – melting or ablation? I appreciate that melt dominates ablation, but you have included a sublimation term in the model so you need to distinguish between melt and ablation. L570: “unbalanced longwave budgets at night” – clarify. L540, L606: There is a jump from Section 7.2 to 7.5

References

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