The Cryosphere Discuss., 8, C1253–C1264, 2014
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The Cryosphere Discussions
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Interactive comment on "On the interest of positive degree day models for mass balance modeling in the inner tropics" *by* L. Maisincho et al.

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Received and published: 12 July 2014

We really thank Mauri Pelto for his specific discussion and comments on our paper, which suggest that this paper may be a valuable contribution for tropical glaciology.

Please find our responses and supplementary figures to Mauri Pelto's comments (between quotation marks) hereafter.

"Maisincho et al (2014) provide a detailed PDD model to assess surface mass balance on Antizana Glacier 15 in Ecuador. The results are validated against both the surface mass balance gradient and the transient snowline. The model is also run using hourly and daily data. The result can be both a well constrained model, but also a model run

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with sufficiently different temporal time step and degree day function coefficients that some detailed findings are realized. At present the level of statistical evaluation of the results both for intercomparison and validation is insufficient. Reviewer 1 and Mauri Pelto write that the significance of our statistics is not sufficiently evaluated. Actually, t-student test had been initially performed with every R coefficients, but they had not been included in the text. We will follow Reviewer 1 and M. Pelto's recommendation, and add significance levels in the text".

However, please note that except for the coefficients of Figure 3&4, every R coefficient is systematically given with the number of points of the studied sample allowing any reader to compute significance. In the paper, except for Figure 3, correlations are considered as significant for p=0.001. Moreover, the coefficients from Figure 3a are not significant at the level of 0.05, but we never considered these coefficients as significant (we wrote that correlations were poor) and did not use these relationships to assess DDF values. The DDFs are obtained with coefficients from Figure 3b, and all these coefficients are significant at $p<0.05$.

In the revised version of this paper we will present the t-student results for every R, to demonstrate that we are not using the term significant inadequately in the text.

"The validation of the sur-face mass balance record using the transient snowline data should be strengthened, as this is the only purely field data set to compare with."

In this paper we performed a 4-level validation: 1. with daily melting boxes; 2. with daily energy balance results; 3. with annual mass balance; and 4. with the ELA and snowline modeling. We treated the ELA and snowline validation at the same level of the other steps and did not want to make a too long section on this point, because the paper was already quite long. In the revised version of the paper we will still provide an in-depth analysis of this specific validation.

"At present it is more of a com-parison. Attention to two assumptions that precipitation is constant with elevation"

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The assumption made on precipitation was largely presented by Favier et al. (2008), who compared data from 5 pluviometers distributed between 4400 m asl (above sea level) and 5000 m asl with accumulation measurements performed at the summit (5740 m asl), showing that no clear precipitation gradient appears with elevation. This statement is likely due to the small size of the glacier (2-km long).

However, this assumption has only low impact on modeling results, because temperature is the first order variable in this PDD approach. Indeed, as demonstrated in the paper with data from a remote station or with NCEP1 precipitation data (which are class C data as written by Reviewer 1), the impact of “uncertain” precipitation data is quite low on the final mass balance modeling,

Nevertheless, a sensitivity analysis on precipitation (increased/decreased precipitation as a function of elevation) will be added in the revised version of the manuscript to quantify this effect on glacier mass balance.

"and sublimation is not important need a better defense."

Sublimation has been thoroughly studied at 4900 m asl by Favier et al. (2004), demonstrating that it represents 5% of the total ablation at this elevation. Of course, melting decreases with elevation, and the sublimation term might have a larger impact at higher elevation, but because sublimation decreases when temperature decreases (e.g. Bergeron et al., 2006), the sublimation will remain limited at higher elevations due to colder temperature. Moreover, on Antizana Glacier 15, the presence of frequent lenticular clouds on the summit suggests that solid condensation is expected to occur at the summit (as observed by the frequent frost deposition there) and sublimation at the glacier snout results from the important foehn effect (Favier et al., 2004). The sublimation gradient is thus unclear but, sublimation may decrease rapidly with elevation from the glacier snout to the summit.

"With better validation, statistical significance testing and assumption justification this paper can be a useful contribution"

As we proposed to Reviewer 1, in the revised version of our paper, we will: 1. more accurately present the significance of every statistics, 2. present an error estimate based on the sensitivity test and on comparison with field data, 3. discuss the physical context of parameters. 4. include a test on the impact of the assumptions made on sublimation and precipitation variations with elevation.

"2638-5: Replace “melting amount” with “melt volume” or “ablation”"

This will be replaced

"2638-19: “were good” is not sufficiently quantitatively descriptive. This type of qualitative evaluation is used many times in this paper and is not a useful measure of statistical significance."

We will include uncertainty range of the method in the paper as proposed in our response to Reviewer 1.

"2639-23: The following statement cannot be justified “However, a direct link between higher temperature and increased ablation has never been clearly demonstrated”. In fact this has been repeatedly demonstrated in the Andes and other settings. So maybe something else was meant."

This has been rephrased because the word “direct” was confusing: However, a physical link between higher temperature and increased ablation has never been clearly demonstrated

"2640-12: Need to specify that the Andes is where short wave radiation is by far the most important."

This remark is relevant and will be added.

"2643-18: Valuable and important step that using daily as well as hourly values in the PDD model."

We do not understand what Mauri Pelto exactly wants us to clarify here.

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"2644-7: It is reasonable to assume precipitation is constant with elevation, but is there any reference or data in support of this Urrutia. and Vuille (2003) indicate significant variation, though not specific to the Antizana."

This has been discussed in the introduction. Please refer to Favier et al. (2008).

"2649-10: How is a 50 cm cylinder of ice obtained and then placed amidst a bigger box of surrounding ice? If it is not left as a whole unit there will be increased surface contact with the area versus glacier ice and melt rates will be inflated."

We tried to insert the big ice block and then to fill the holes with smaller ones in order to reconstruct the surrounding natural surfaces as much as possible. We thus intentionally kept dust on the ice. Natural surfaces could obviously not been perfectly reconstructed and this has an impact on results, but please note that the surrounding surfaces are almost always impacted by cryoconite hole, and roughness is though quite important. Nevertheless, comparisons between melting amounts from melting boxes and from surface energy balance data (see attached Supplementary Figure 1, which is an update of Figure 3c in which the 1/1 line was included) suggest that this effect is reduced, since measured melting is generally lower than the modeled one, likely because initial liquid water is retained by/between the small ice blocks due to capillarity. As a consequence, both processes may almost compensate each other, but we are not able to give a strict uncertainty range due to these effects.

"2649-16: VBP- is not the typical term for this usually mass balance profile or mass balance gradient, both are noted by Cogley et al (2010) but not vertical balance profile."

We will change this term adequately.

"2650-28: Should be transient snowline."

The term transient will be added.

"2653-6: This is important to cite what values were obtained from other glaciers in this setting."

We can cite for instance (see Radic and Hock, 2011): Ram River Canada (51.85°N 116.18°W) for which: DDFsnow = 6.3 mm w.e. °C day-1 DDFice = 9.7 mm w.e. °C day-1

Djankuat Russia (43.20°N 42.77°E): DDFsnow = 7.1 mm w.e. °C day-1 DDFice = 10.5 mm w.e. °C day-1

Bondhusbreen Norway (60.03°N 6.33°E): DDFsnow = 7.7 mm w.e. °C day-1 DDFice = 10.7 mm w.e. °C day-1

Svartisheibreen Norway (66.58°N 13.75°E): DDFsnow = 6.0 mm w.e. °C day-1 DDFice = 9.8 mm w.e. °C day-1

Finally, we observed that our values are close to those obtained by Azam et al. (2014) on Chhota Shigri Glacier, India, where they find: DDF for debris 3.3 mm w.e. d–1 °C–1 DDF for snow 5.3 mm d–1 °C–1 DDF for ice 8.6 mm w.e. d–1 °C–1

"2655-19 “this significant correlation” what specific correlation is referred to?"

We were referring to the correlation between “T/melting and the SEB melting (r = 0.76, n = 376 days, Fig. 5a).”. This will be clarified in the text.

"2656-17: How reasonable is the assumption that sublimation is negligible? Favier et al (2004) note the significance of sublimation on Antizana during windy days. This is an argument that has to be addressed."

We already addressed this point in the introduction of this response to Mauri Pelto’s comments.

"2656-26: The model fit is quite impressive in the elevation range where surface mass balance measurements are made. There is an underestimation of the model for accumulation above 5300m where field measurements exist in 2001, 2004 and 2006. A brief discussion of this and likely causes would be appreciated anytime in the next page."

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We do not understand this comment of Mauri Pelto. First of all because accumulation measurements are available for each year not only 2001, 2004 and 2006; and also because the model never underestimates the accumulation (refer to Figure 7 were the measured and modeled data are shown).

"2659-13 Add transient before snowline. The transient snowline is a variable that has been long discussed in mass balance observation. This is a key validation measure since unlike the balance gradient it is a specific data point. Hence it is the most reliable validation, and I commend the authors for the efforts in building this record. If it is given more attention I am sure the model results will be even more compelling. Mernild et al (2013) talk about the utility of transient snowline variations in mass balance assessment."

We totally agree with this comment and this explains why we included a validation step with this data in this paper

"Why is n=91 when earlier it was stated there was 712 snowline observations?"

In this figure, for clarity, we present the “15-day average snowline elevation”, meaning that several transient snowline elevations are accounted for each point. If we consider daily snowline values, the correlation coefficient is R=0.75 for 712 observations, showing that the relationship is still correct also at a daily timescale.

"In Figure 10 it is less important to show us the full time series for which the correlation coefficient is provided than to indicate a particularly representative period in richer detail. Such as 2007 and 2008, there are a few places where there is considerable offset that could then most likely be attributed to a specific observed snowfall event."

Larger differences between modeled and observed snowlines in 2007-2008 were due to larger uncertainties in the 15-day average snowline elevation estimates. Indeed, the automatic camera had several failures and photographs were taken manually during field trips. However, field trips are only performed twice or three times each month, and

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the 15-day averages were thus computed with one or two photographs only. When the photographs were taken, the observed snowline was not always representative for the 15-day mean snow line elevation. In this case, the resulting 15-day average snowline did not correctly fit with the 15-day modeled one.

Another explanation is related to the very large variability of snowline elevation during these two years (supplementary Figure 2). The model sometimes shows very large variations which are less visible in the observed snowlines. These modeled variations are due to small snowfall events, which are largely dependent on precipitation measurement uncertainties. Indeed, small precipitation differences can yield the model to give a persistent low elevation of the snowline or not.

Even if the focus on 2007-2008 gives interesting additional information (supplementary Figure 2), we believe that Figure 10 of the submitted paper is more relevant because it allows observing variations over a long period marked by large variability. To facilitate the reading, we also prefer to show the 15-d averages.

"We could also get a better sense of the standard deviation which should be reported."

Standard deviation are 61 m for 15-day average snowline and 75 m for the daily snowlines (712 observations). Standard deviation value will be included.

"2659-20: This should be treated as a more important validation versus comparison as stated earlier. Quantifying the fit should be with more than just correlation coefficient."

We will present the standard deviation of differences, and analyze typical situations in 2007-2008 where the differences are the largest.

"Also since there were"

Missing comment?

"2661-15: A good discussion and analysis but the reliance on one station for comparison to Izobamba does not yield confidence. Can a better regional reference for the precipitation comparison come from the regional Andean climate work such as Vuille et al (2003) or Haylock et al (2006)."

Izobamba is the WMO reference station on the plateau, and thus it is assumed to be the best station close to Antizana Glacier. Time-series from other stations close to Antizana should be used more cautiously. Anyway, our interest in this approach was to demonstrate that temperature variations largely control the mass balance variations. Precipitation is a second order variable to explain mass balance changes, and a low quality dataset has only a limited impact on modeling results

"2662-18: This implies how critical a rise in the freezing level would be. A reference to this and the observations of this fact of rising freezing levels on Quelcaya Ice Cap from Bradley et al (2009) would be important"

This remark and reference to Bradley et al. (2009) will be added

References:

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Supplementary Figure captions:

Supplementary Figure 1: Comparison between measured melting and modeled melting with the surface energy balance

Supplementary Figure 2: a) Comparison between variations in the observed 15-day average snowline elevation (blue) and the modeled 15-d average snowline elevation (black), over the period 2007-2008. b) Same as a) but for daily snowline elevation. Observations are blue lines and modeled are red triangles.

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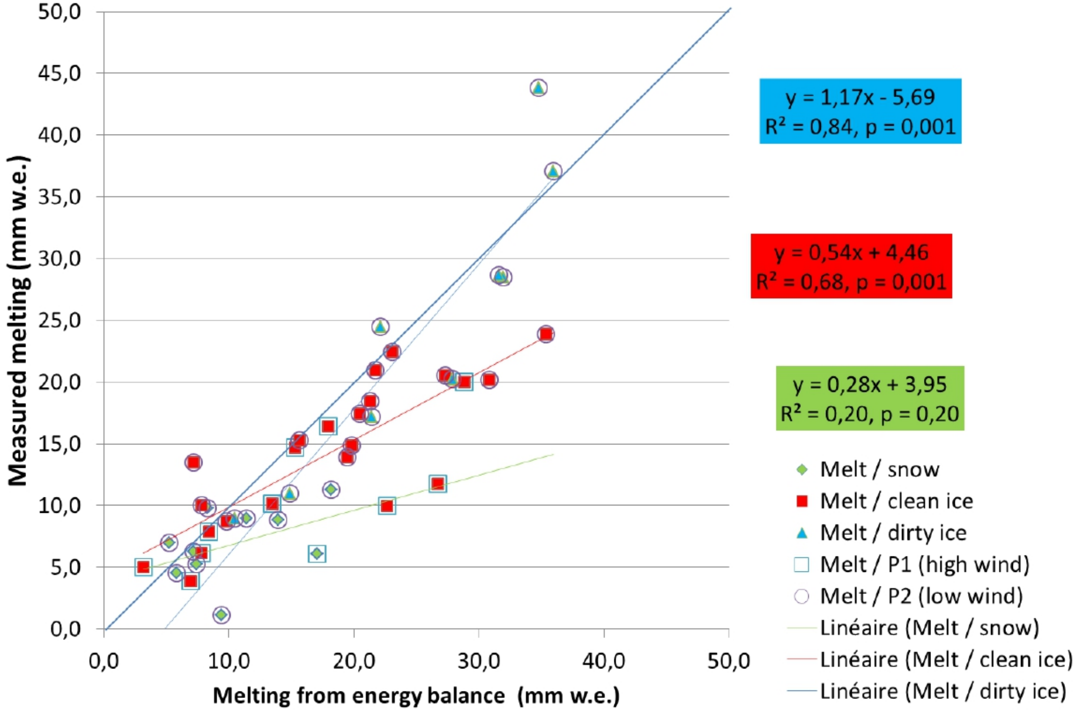

Fig. 1. Comparison between measured melting and modeled melting with the surface energy balance

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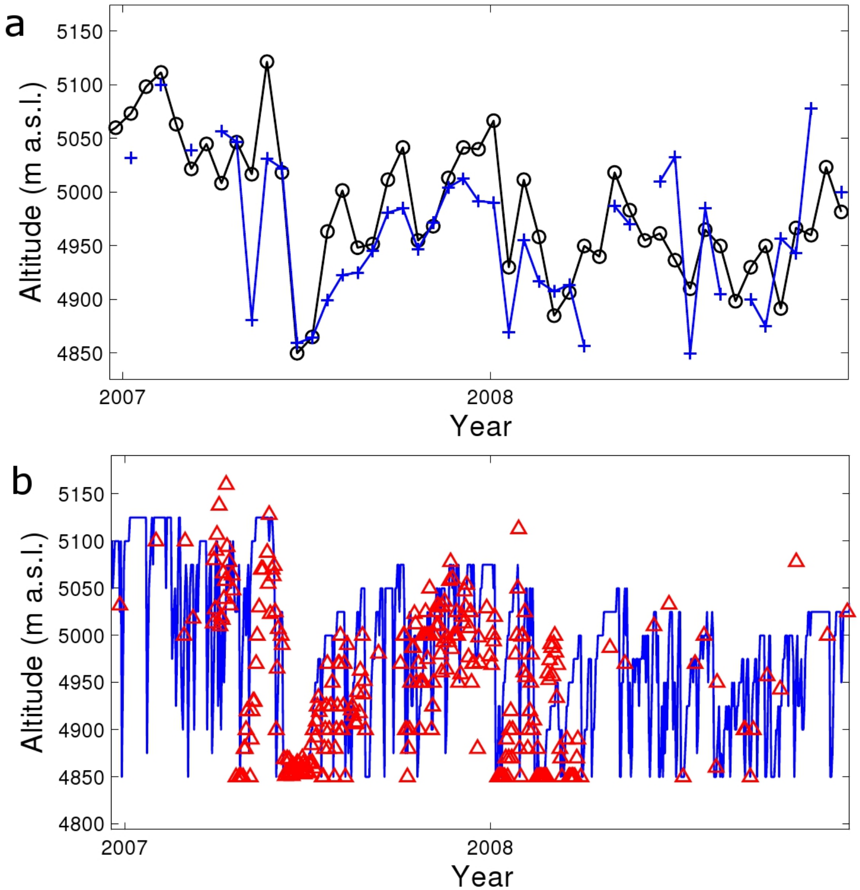

Fig. 2. a) Comparison between variations in the observed 15-day average snowline elevation (blue) and the modeled 15-d average snowline elevation (black), over the period 2007-2008. b) Same as a) but for dail