We thank reviewer#2 for his/her constructive review of our paper. Our response to each comment is made hereafter. For the reader convenience, the reviewer's comments are highlighted in bold.

As a summary of our response, we:

1. Clarified the motivation of the paper
2. Reorganized the paper and put more emphasis on the processes justifying that temperature and melting are correlated
3. Reduced the length of the paper and of the complex validation steps
4. Reduced the sentences on the interest of sublimation, even though we believe that this improves the final results.

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.
Reviewer#1 made the same remark, as a consequence our response is common to both reviewers. We strongly decreased the strength of our conclusions after the first review of our paper, because one reviewer was clearly opposed to the use of the degree-day model in the tropics. But we believe that there are many motivations to study ablation with a very simple model, as a degree-day model.

In particular we believe that validating the degree-day model in one region, and understanding why this kind of model works, is clearly important in any region of the globe (see for instance (Sicart et al., J. Geophys. Res., 2008; van den Broeke et al., Geophys. Res. Let., 2010; Six and Vincent, Ann. Glaciol., 2014; Azam et al., Ann. Glaciol., 2014., Matthews and Hodgkins, J. Glaciol., 2016)):

1. *Studies on the degree-model allows validating the basic hypothesis of large scale glacier wastage analysis (e.g., Radíc and Hock, Nature Geoscience, 2011; Marzeion et al., The Cryosphere, 2012) largely cited in the IPCC AR5 report for instance. This hypothesis is that "PDD models may be applied at a global scale".*

   Indeed, as already written in our response to reviewers of the previous version of the paper: "One should also notice that in the interesting analysis from Radíc and Hock, Nature Geoscience (2011), which was based on a PDD approach, the authors were offering glacier wastage projections for tropical Andes (region 14: South America I) without DDFs specific to this region and consequently considering DDF retrieved from other places. As a consequence, offering DDFs values and uncertainty ranges for tropical glaciers is also of interest for large scale approaches. Such DDFs values were not available before". We can reinforce this statement, reminding the existence of other similar more recent global scale studies, as for instance studies from Marzeion et al., Science (2014) or Huss and Hock, Front. Earth Sci. (2015). Of course low latitude glaciers will not have any significant impact on the sea level rise, but demonstrating that PDD models are adapted within the inner tropics reinforce the main assumption of these large scale studies which is that "PDD models may be applied at a global scale". If this assumption is not verified, then results from the mentioned papers are clearly questionable.

   This point is even more critical, because even if degree-day models are very simple, they are generally incorrectly used in global scale studies. Indeed, as demonstrated by van den Broeke et al. (2010), or in more recent publication by Matthews and Hodgkins (2016), a typical PDD

approach using Temperature Threshold for melting at 0°C is not supported by observation. Our paper confirms this conclusion even under the low latitudes. This demonstrates that there are still researches to perform in order to correctly use the degree-day approach.

2. *Performing glacier wastage projection in Ecuador with an energy balance model is not necessarily more accurate than with a degree-day model.*
   Indeed, in Ecuador, ablation is almost entirely controlled by the net shortwave radiation and hence by cloud cover. However, modeling of changes in cloud cover or precipitation at a regional scale is largely more challenging than modeling temperature changes with current generation Atmospheric-Oceanic General circulation models (as for instance in the CMIP5/CMIP6 exercises) (e.g., Favier et al., in press). In spite of the quality of surface energy balance models their use may be not justified for glacier wastage projections if important changes in the cloud cover are expected within the inner tropics, whereas a model based on temperature variation may be quite robust. Thus, using a surface energy balance is not necessarily more justified than a degree-day model. Conversely *getting models of different complexity is interesting for ensemble modeling giving the uncertainty range of glacier projections. For this task, simple model must be validated with current climate.*

3. *Performing studies at the scale of Ecuador is impossible with a surface energy balance even if improvements in measurements techniques, computer resources, energy balance models and downscaling approaches have been made during the last decades.*
   Such kinds of studies are important in Ecuador because glacier retreat has direct consequences for the local water supply to Quito (waters from glaciarized catchments are collected in the framework of important power plants (see la MICA power plant and water supply station, or the larger "rios orientales" project)). Getting information on present and future water contribution from glaciers is timely for the economic validity of these projects. Modeling glacier behavior is thus fundamental. A distributed SEB modeling is still complex because the small size of glaciers (2 km x 200 m for Antizana glacier 15) implies an important downscaling step to get meteorological information with a horizontal resolution of about 100 m. This is clearly hard for one glacier, but clearly impossible for many glaciers in different regions. Getting a simple approach is thus really important in Ecuador.

The first interest is thus to demonstrate that degree-day models are adapted in the inner tropics. Since the use of this model is very polemical in the tropics, reaching this conclusion was a key issue.

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.
We simplified and reduced the size of the Section on the model validation in the new version of the paper.

To clarify my 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.
We thank reviewer#2 for being honest on his own feeling (so did the Reviewer#1 of the first version of our paper who was opposed to use PDD models in the tropics). We observe that Reviewer#1 also found necessary to give his own opinion on the interest of these models in the low latitude regions (he

is opposed to use PDD models). Since this remark is similar to the one from reviewer#1, our response will be the same.

These opposite, a priori, but honest opinions reflect that our paper is necessary because two communities have been opposed on this question for many years.

It is quite remarkable to observe that the opposition to the use of degree day models mainly results from one publication from our research team (Sicart et al., 2008). This conclusion on Antisana Glacier 15 was based on an analysis of a 6-month dataset only and at one point. Ecuador was just a secondary study site in comparison to the Saint Sorlin glacier (French Alps), Zongo glacier (Bolivian Andes) and Storglaciären (Scandinavia). The mentioned analysis was based on a time period (180 days), too short to significantly correlate daily energy fluxes and air temperature for the Antizana Glacier 15. Actually, we extended this correlation analysis to 530 days and obtained significant correlations between temperature and energy fluxes. Our correlation coefficients between daily energy fluxes and air temperature are presented in Table S2. To illustrate that our correlation coefficients justify the interest of the degree day models in the inner tropics, please note that we found similar correlation values on Antisana Glacier 15 than those presented for the other investigated regions in Sicart et al. (2008). For instance, in Sicart et al (2008) publication, it is stated that for Storglaciären "The importance of H in controlling the melting energy on Storglaciären, compared to Zongo and St Sorlin, is evident in Figures 1–3. These results support the conclusions of Braithwaite (1981), who attributed the high correlation between temperature and melt rates to the sensible heat flux because of its large variability and its close correlation". If we refer to Table 4 of Sicart et al. (2008), data for Storglaciären, are from July 9 to September 2, 2000, that is for n = 57 days, and correlation between daily temperature and the SEB is r=0.63, while summer daily temperature ranges between 2°C and 9°C approximately. We do agree with these values and conclusions which are accepted by the community. However, the correlation between the SEB (i.e. Melt) and Daily temperature at Antisana Glacier 15 is 0.62 for 530 days when daily temperature only ranged within 3.5 degree interval. As a consequence, if the correlation given in Sicart et al. (2008) explains why the PDD can be applied at Storglaciären, we believe that the same conclusion should be made on Antisana Glacier 15 as values are comparable.

Because it was crucial to have robust conclusions, we did not limit our analysis to this correlation analysis and performed a much deeper investigation: 1) we use a 9-year dataset, 2) we made a cross validation procedure between 2 periods, 3) we validated the model not only at one point but also with a distributed approach, and 4) we validated the model with the snowline elevation and the ELA. All our results confirm that our degree-day model is robust on Antisana Glacier 15.

Actually, we believe that conclusions from Sicart et al. (2008) study about Antisana Glacier 15 were likely impacted by the short time period they used and because this period (September 2002 to March 2003) was marked by low variations in temperature and in the surface state of the glacier at 4900 m a.s.l.. Indeed, ablation values were almost always around around 25 mm w.e. d^{-1} (std = 9.7 mm w.e. d^{-1}) and temperature was almost always around 0.4 °C (std = 0.6 °C) (see our Figure 3): if no changes are observed in ablation and temperature, it is very hard to find a significant correlation between both variables.

Our study is thus a demonstration that using a longer time period gives different conclusions and that the PDD model may be used in Ecuador. This demonstration is a first step to support such kind of model as potential tools for further research (for a first order assessment of water resources for example), or to study glaciers of the same climatic zone where almost no data are available which is the case for almost all the glaciers in Venezuela, Colombia and Ecuador.

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?
We are surprised by this remark because we don't believe we were giving important weight on the sublimation equation in this paper. However, we removed sentences on sublimation in the abstract and reduced the weight given on this process elsewhere in the text.

However, please note that including this process was requested by a reviewer of previous version of the manuscript, and that we observed that accounting for sublimation allows improving our results on the mean modeled climatic mass balance. Indeed, the mean modeled mass balance between 2000 and 2008 (0.06 m w.e. a^{-1}) is already slightly more positive than the mean observed geodetic mass balance (-0.12 m w.e. a^{-1}) for 2000-2008. If we remove sublimation, the difference is larger (the modeled mass balance is then 0.32 w.e. a^{-1}). This suggests that including this process was a relevant suggestion from previous reviewer.

I strongly recommend to either find an application for the model or to define a clear research question motivating this study.
We defined more clearly the interest of the model in the introduction and conclusion:

a) First motivation was to demonstrate that the PDD approach is adapted to analyze the surface mass balance of the Antisana Glacier 15, and in the same climatic zone. This demonstrates that results from Sicart et al. (2008) publication were not robust, likely because the study period was too short and incorrectly chosen (i.e. without significant variations in ablation).

b) Second, our study contributes to validate the basic hypothesis of large scale glacier wastage analysis which is that "PDD models may be applied at a global scale".

c) Third, getting a simple model such as a degree-day model, is useful for glacier wastage projections or for studies in regions were field data are lacking.

d) our correlation analysis gives important information to understand why temperature and melting are correlated in the inner tropics.

As a conclusion, we clarified the interest of the paper in the introduction and conclusion, and changed the structure of the paper in order to give more weight to the information currently available in the supplementary materials.

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.
This was initially done in order to get a more fluid reading. However, as written the previous paragraph, we now put more focus on this aspect in the revised manuscript.

In turn, the multiple validation analyses could be condensed and serve the purpose of, for example, providing uncertainty ranges to the model results.

This section was condensed

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.
We propose the following Title:
"Revisiting previous conclusion on the use of the degree-day model in the inner tropics"

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)?
Our paper provides exactly what is missing in order to explain this relationship. The mass balance observation period is currently not sufficiently long to perform a covariability study between T and the mass balance over decades. As a consequence, Francou et al. (2000) only wrote that the trends were in agreement. We suppose that the reason is the same as the one given by Francou et al. (2004) in order to explain the link between ENSO and T, (i.e. albedo varies between El Niño / La Niña oscillation due to changes in the occurrence of solid precipitation on the glacier, related to warmer condition during El Niño). But there is no proof for that, except if melt is related to temperature through the net short wave radiation and the albedo, as suggested in our paper.

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.
This comment is correct and the correlation value was removed.
In this response we present a figure showing the cumulative frequency distribution of daily precipitation at P2 and P4 in 2002 (very similar figures are observed for other years). This figure shows that data present a very similar distribution (Figure R1). Moreover, please note that the use of data from P2 concerns only 6% of the data, and has only a low impact on our results.

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 linear function of wind-speed.
The reviewer is correct in saying that we are comparing two linear functions of wind speed, but actually this clearly justifies our Equation rather than it contradicts the use of a linear relationship based on the wind speed. Actually, we only try to compute sublimation using a simpler equation than the bulk method.

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.
The reviewer is correct, but long term accurate humidity data are clearly hard to obtain. Thus we preferred using an Equation based on the wind speed only. However, note that our bulk method has been calibrated with direct measurements of sublimation with lysimeters during 43 days (when melting boxes were present) (see Favier et al. (2004) for details) and that these measurements account for the seasonality in water vapor pressure.

L331: I am more familiar with the word "deposition"?
We are not native English speakers but this word is generally used as the opposed process to erosion in aeolian snow transport studies (e.g. Amory et al., 2015). It may be not adapted for frost. We will remove it.

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.
The accuracy of sensors is described in Table 1, but it is generally assumed that the accuracy of radiation fluxes is ±10% on daily sums. The uncertainty of the turbulent heat fluxes depends on the validity of computation assumptions (i.e. the Bulk Method). Litt et al., 2015 observed on the Zongo Glacier in Bolivia that "Random errors on the fluxes derived from the aerodynamic profile method are mainly due to air and surface temperature uncertainties. The errors can be large on short time scales, but they result in small errors on the fluxes averaged over the field campaigns". Finally, the accuracy of sub-surface melting calculations is difficult to assess but values of subsurface melting amounts are generally a few percent of total melting amounts.

However, the SEB on Antisana Glacier 15 is mainly controlled by the net shortwave radiation (which explains 95% or the variance of melting) and the accuracy of melting computation is expected to be quite similar to the one of the net shortwave radiation, i.e. ±10%. This is confirmed by the values of the correlation coefficient (r = 0.91) and of the slope (1.01) of the regression line between the computed and the measured melt (with melting-boxes). The good agreement between the monthly modeled and the measured monthly mass balance is also a confirmation of the accuracy of our SEB computations (r = 0.90, n = 18, p = 0.001, E = 0.79).

A paragraph has been added on the accuracy of the SEB in the manuscript.

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.
This comment is only partially correct because precipitation, and hence accumulation, are not perfectly known. Thus, a validation of the model in "real" conditions would include additional uncertainty related to precipitation amounts. If we were computing the surface state directly with the model during the validation step reviewer#2, our results would account for the model quality and for additional uncertainty caused by the snow accumulation modeling which largely depends on precipitation correction.

However, we accounted for this comment, because the best "real condition" modeling is expected to be achieved when the precipitation correction allows to accurately reconstruct the temporal variations of the observed surface state. In the new manuscript, we calibrate the precipitation correction in order to obtain the best score between observed ablation (with the SEB) and the modeled ablation (with the degree-day model). This yields the following results:
Over 530 days, we observed that the best correction of P4 data is +122% (r = 0.70, E = 0.43), but results are still accurate with a +76% correction (r = 0.67, E = 0.37). This validation demonstrates that a very large precipitation correction is required to accurately retrieve the surface state, and suggests that the value of +76% is a minimum correction to apply.

Moreover, please note that the validation with monthly mass balances (section 5.4 of the previous version) was already done in order to verify that the model correctly reproduces the mean surface state at the monthly scale.

L348: daily melt or daily ablation?
Here we refer to daily melting.

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.
The link between monthly values is expressed through the correlation coefficients between monthly observed and modeled mass balance values. Conversely, we believe that our figure is really useful because it suggests that the mean observed surface state was accurately reproduced by the model. Indeed, observation showed that in 2000, at the beginning of 2006, in 2007 and in 2008 the surface was characterized by important snow covers, and the model clearly reproduced lower ablation amounts exactly at the same time (Figure 4). The model also correctly reproduced when the ablation amounts recovered high values, demonstrating that the modeled snow cover disappeared at the same time in observation and in the model.

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?
Yes, we are talking about the climatic glacier-wide mass balance.

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Figures
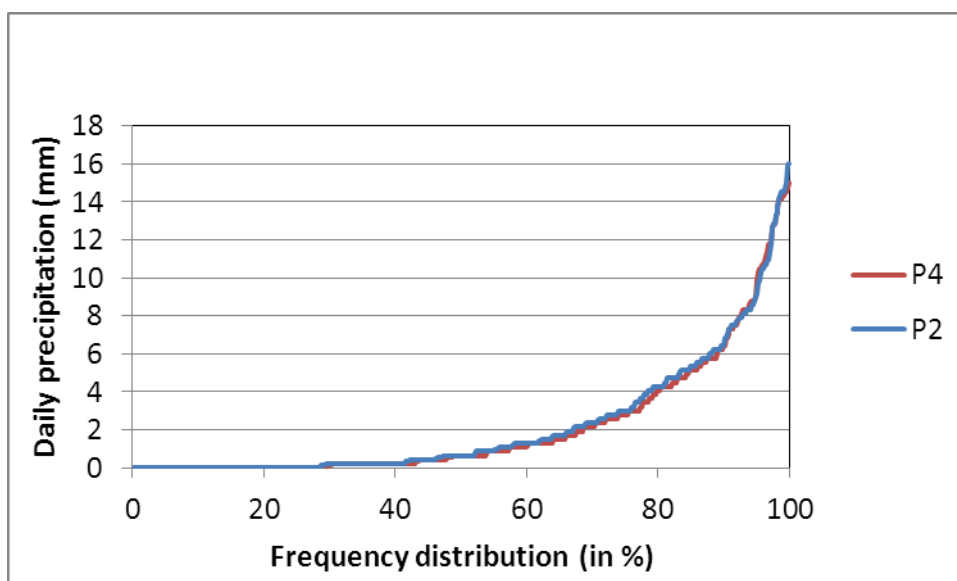
Figure R1: Cumulative frequency distribution of daily precipitation at P2 and P4 in 2002.