First of all, we deeply thank reviewer#1 for his/her constructive review of our paper. In particular, we acknowledge that he/she made a really important and rigorous work as confirmed by the reading of other papers published by our team on the Antisana Glacier 15. Our response to each comment is made hereafter. For reader convenience, the reviewer's comments are highlighted in bold.

Reviewer#1 pointed out critical remarks that relate to 3 main points:

1. the motivation of the paper
2. the actual precipitation amounts
3. the physical processes were not sufficiently analyzed and the sublimation gradient was not clear.

We addressed these 3 points as follows:

1. We give various interests of the paper, and we propose to include them more clearly 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.

2. In this letter we clarify that there is no contradiction between Basantes-Serrano et al. (2016) paper and our paper. We justify why a) the precipitation correction (76%) is clearly necessary and gives the actual precipitation amounts on Antisana Glacier 15, and b) no significant precipitation gradient is needed to justify the accumulation amounts proposed by Basantes-Serrano et al. (2016). We also clarify that the correction already accounts for the separation between snow and rain. We have included more information on this point in the paper.

3. In our paper, we actually give new information on the accumulation and ablation physical processes on the Antisana Glacier 15:
   a) a new SEB modeling at one point reveals new information on the role of subsurface processes (subsurface melting and conduction), which had never been analyzed before where the ice is temperate every day.
   b) this SEB modeling improves the relationship with ablation observations.
   c) this modeling confirms that precipitation correction is absolutely necessary to model the mass balance at 4900 m a.s.l.,
   d) we have included a Table with correlation coefficients between energy fluxes (Table S2) showing that the net shortwave radiation and the albedo are the key variables of the surface energy balance.
   e) the use of Table S2 gives the way to justify why the degree-day model may be applied on Antizana Glacier 15. This is explained through the link existing between temperature and the net short wave radiation.
   f) we demonstrate that melting already begins when daily temperature is below 0°C.
   g) Finally, in this letter we provide the results of a distributed SEB modeling performed by L. Maisincho in his PhD thesis (Maisincho, 2015) confirming that sublimation gradient is almost constant with elevation, and that variation of ablation with elevation is essentially

due to changes in albedo resulting from changes in the occurrence of solid precipitation with temperature. This point is in agreement with our degree-day modeling.

As a conclusion, in the new version of our paper, we define more clearly the motivations of our investigations, the actual precipitation amounts and gradient with elevation, the physical processes involved in the mass balance, the interest of the sublimation routine, and the absence of a sublimation gradient with elevation. Finally, we shortened the paper as requested by the reviewer#2.

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.

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?

As written by reviewer#1, compared to the first version of this paper, we clearly attenuated the strength of our conclusions and limited the applicability of the proposed model as a feedback to reviewer's comments of this first version. However, even if we believe that the model is suitable for glaciers located in the inner tropics, it is scientifically rigorous to suggest that the degree-day factors should be accurately calibrated before any use elsewhere.

Nevertheless, there are many motivations for the present paper.
First, as written by reviewer#1, this paper offers 1) a synthesis of the available meteorological data set on the studied catchment, and large amount of data have never been published before, 2) an alternative to energy balance modeling to reconstruct mass balance in the inner tropics, when surface energy balance data are not available or for paleo-glaciology studies. Actually, the second point is really interesting 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., 2016). 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.

My feeling is that degree day modelling is not really suited for low latitude regions. Which has already been expressed by previous reviewers,

We thank reviewer#1 for being honest on his own feeling (so did the Reviewer#1 of the first version of our paper). Reviewer#2 also found necessary to give his own opinion on the interest of these models in the low latitude regions. However Reviewer#2 believes that it is interesting to use such PDD models. These two opposite opinions demonstrate 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 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.

and it is unlikely that the approach will be adopted by others studying tropical glaciers elsewhere.

We agree that it may not work outside the inner tropics, i.e. where the turbulent latent heat flux is a major contributor of the energy balance, and/or where the role of the net shortwave radiation is less important in the final mass balance. This is the case in Bolivia or more generally in the outer tropics where the incoming longwave radiation ($L_{\downarrow}$) is also a key variable during melting period and where a very long dry season is observed. On this point we agree with Sicart et al. (2008).

Thus, to be on 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.

We believe that these processes were already described in Favier et al. (2004). However, in this paper we go a step further and demonstrate that Favier et al. (2004) approach needs to be improved to get really accurate computation of the melting. We also produced an analysis of relationships between energy fluxes, melting and temperature, given in Table S2. Our results show that the main conclusions of Favier et al (2004) are still true, and that the surface energy balance is almost totally controlled by S, and by the albedo. This conclusion is the base of the link between snow precipitation phase and ablation used in Favier et al. (2004) and Francou et al. (2004) to explain the link between the ENSO and the glacier mass balance variations. Finally, in our response to reviewers, we show results of a distributed surface energy balance modeling (Maisincho, 2015) which demonstrates also the crucial role of albedo and of S in the spatial variations of the SEB, which is in agreement with our paper.

This will be only possible if the data used as input are more carefully scrutinised (see comments below),

Here we just want to recall that precipitation amounts are not required for the validation of the degree-day model vs. energy balance model in Section 5 because we only compare ablation (melting + sublimation) amounts from the two models. Precipitation is only used in section 5 and 6. Actually, our calibration and validation of the degree day model at 4,900 m a.s.l. (using albedo variations) is thus independent from this key question of precipitation amounts. However, the reviewer is true on the fact that giving clearly the real precipitation amounts on this site is a key issue, and our paper helps to clarify the situation on this point (see 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 approach must be clearly outlined.
Please refer to our response on motivations before.

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.
We believe that the PDD model is adapted elsewhere within the same climatic zone and that this conclusion gives the opportunity to use this tool 1) in poorly documented areas, 2) as a first order estimate of ablation. However, it is scientifically rigorous to suggest that the degree-day factors should be accurately calibrated before any use elsewhere.

Moreover, our uncertainty study clearly tries to describe honestly that sublimation gradient and precipitation amounts are the most important uncertainties of our model (as noted by reviewer#1). Nevertheless, using a surface energy balance (SEB) model would not necessarily be more accurate. Indeed, a SEB modeling would reduce the uncertainty in the sublimation gradient but would still be impacted by precipitation uncertainty (as well as our PDD model).

To elucidate the important question on the sublimation gradient, we present below the results on the distributed SEB modeling of Antisana Glacier 15 from Maisincho (2015) between 2002 and 2008. Results show that sublimation is almost constant with elevation (see attached Figure R1), which is in agreement with our paper. The attached Figure R1 also demonstrates that the main changes in the energy balance are occurring on the net shortwave radiation, as a consequence of albedo variation with elevation.

Concerning precipitation amounts, in our paper, we demonstrate that Wagnon et al. (2009) corrections on precipitations are justified, because this correction is in agreement with 1) direct observations of snow accumulation on plates, 2) results of the surface energy balance modeling of not permanent snow (Wagnon et al. 2009) and of the glacier surface, 3) results of the modeling of the climatic mass balance with the degree-day model, 4) accumulation data at the summit. These four points suggest that the correction is robust.

As a consequence, we believe that we are rigorously constraining the main uncertainty sources of the model, and that the actual uncertainty of the model on Antisana Glacier 15 is mainly related to the uncertainty in parameters (melting factors, Temperature threshold and LR).

Do the authors really recommend it to others that only have the input data that are used for the model?
Yes we do, but it is well-known that degree-day model parameters are not easily transferable from one region to another or even from one glacier to another within the same region, even though many times there is no other option than doing it. Indeed, many authors working at regional scale or global scale consider this transferability hypothesis as true. But any parameter transfer should not be done without calibration and/or validation. We want to highlight this point in our paper, even though we believe that applying Antisana parameters to other glaciers in the inner tropics will allow quantifying the melting with a reasonable accuracy.

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.

The main purpose of the study is to revise the Sicart et al. (2008) results (at one point) on Antisana Glacier 15, and check if the ablation may be reconstructed with the modified degree-day model. Then, we also verified that the model may be used at the scale of the glacier and then we modeled the climatic mass balance.

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.

This comment is only partially correct, because we performed a validation step of the degree day model vs. energy balance model (Section 5) which is totally independent from precipitation amounts. This point is totally crucial because, this section is the main validation step of the model (validation of the ablation at 4900 m a.s.l.). As a consequence, validation is independent from both accumulation and precipitation data, demonstrating that this comment from reviewer#1 does not affect our main conclusion on the validity of the degree-day model (at 4900 m a.s.l.).

Moreover, Basantes-Serrano et al. (2016) clearly constrained the actual accumulation amounts at the summit, and Wagnon et al. (2009) clearly demonstrated that a correction of precipitation was required.

Here, we accounted for these crucial points and go a step further in the validation of their assumptions:

1) we considered Basantes-Serrano et al. (2016) mass balance and accumulation values,

2) we confirmed that the precipitation correction proposed by Wagnon et al. (2009) is needed to model the surface mass balance at 4900 m a.s.l. using the SEB modeling (see our comment below on missing information on Figure S2),

3) we accounted for Wagnon et al. (2009) precipitation correction to compute the accumulation values at the summit (i.e. assuming a zero gradient in precipitation) and the climatic glacier mass balance values. With this assumption, accumulation values at the summit are in agreement with Basantes Serrano et al. (2016) data.

On L101 it is stated that annual precipitation at 4550 m a.s.l. ranges from 800 to 1300 mm a-1. In Manciati 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.

The reported range (800 – 1300 mm a^{-1}) represents the inter-annual variability given by uncorrected precipitation.

1) Our precipitation gauge network (Figure 1) is rather dense for such a high altitude catchment and the study of raw precipitation data shows no evidence of vertical gradient of

precipitation (Favier et al., 2008). Please note that Manciati et al. (2015) did not account for the impact of topography (hills, depressions) on precipitation measurements, whereas Favier et al. (2008) account for the differences in surface topography in their analysis of precipitation gradient.

2) Actually, it is well known that precipitation is one of the main uncertainties in high mountain hydrology, and precipitation gauge suffer from systematic undercatch, especially in the case of solid precipitation. Wagnon et al. (2009) did a thorough work to quantify this undercatch in one hand, and Basantes-Serrano et al. (2016) confirmed that raw precipitation data must be increased in order to be in agreement with the accumulation values proposed in their study. Actually, the mean corrected precipitation (i.e. 1820 mm a^{-1}) from our study agrees with the accumulation data given by Basantes-Serrano et al. (2016). Consequently, the true precipitation input on this catchment is now better approached by the value given by Wagnon et al, and with no altitude gradient.

We agree with Reviewer 1 that previous version of the paper did not define the real precipitation amount falling on Antizana Glacier 15. Actually, we demonstrated that the corrected data is very likely the exact precipitation amount on this site, but we did not write it clearly because, even though this correction is accurate, it leads to "manipulate" the raw data, which means that final precipitation is the result of a model and is not a direct measurement.
We clarify this point in the corrected version of our paper.

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.
Importantly, please note even though a 76% correction seems to be very high, it already accounts for the rain/snow distribution for each precipitation event.

A figure showing the monthly variations of temperature and precipitation is already available in Basantes-Serrano et al. (2016) (see their Figure 2) and we do not find necessary to reproduce this figure here. Concerning the frequency distribution of precipitation, we propose a figure in attachment with this response to help the reviewer to verify that precipitation is solid for 95% of the precipitation occurrences at 4900 m a.s.l. (Figure R2). We also applied the correction proposed by Wagnon et al. (2009) and computed the cumulative solid precipitation between 2000 and 2008. Solid precipitation amounts represent 94% of the total precipitation amounts. However, the paper is focusing on the degree-day model and we prefer not including this figure in the text because it would lead to a more confusing message in the paper.

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
We propose to present a figure in the supplementary materials showing the monthly wind speed average and standard deviation (called Figure R3 in this response). Please note that this separation is only used in the Table with correlation coefficients, and we account for the variability of the separation through the calculation of correlation coefficients for low wind days of Period 1. As a consequence, this separation in period 1 and Period 2 has no impact in our conclusions.

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?

We do agree that precipitation is the largest uncertainty on the studied catchment, but it is also the case in many regions. Measuring snow precipitation in windy conditions is a very hard task. For instance, the 16.5% difference between the P4 rain gauge and the Geonor rain gauge mainly results from the difference in the measuring surface area, and by the presence of a windshield to decrease the impact of the wind on measurements during snowfalls.

As a consequence, precipitation gauges are absolutely inefficient in very harsh conditions as for instance in Antarctica (Eisen et al., 2008; Favier et al., 2013). Precipitation gauges are not correctly designed to properly collect solid precipitation in a windy environment (precipitation is systematically underestimated) (Forland et al., 1996; Lejeune et al., 2007; Wagnon et al., 2009).

Here we applied the correction proposed by Forland et al. (1996) and validated by Wagnon et al. (2009) with direct observations and with an independent SEB modeling of the snow cover at the Antisana. The correction of precipitation was made as follows: 1) we consider that P4 gives systematically 16.5% less precipitation than the Geonor rain gauge (this value was the mean difference between both sensors between 2005 and 2012), and next 2) we apply the correction proposed by Wagnon et al. (2009).

The correction applied accounts for the frequency distribution of solid/liquid precipitation. Indeed, for each precipitation event, precipitation phase (i.e. the distribution between solid and liquid precipitation, in percent) is first assessed according to temperature, and then a correction is applied to liquid and to solid precipitations according to wind speed (see Wagnon et al. (2009), Lejeune et al. (2007), and more particularly by Forland et al. (1996), which accurately describe the method).

The snow/rain separation does not depend on a threshold but is as follows:

If (temperature > 2.4°C) Then precipitation is only rain (phase = 0)
Else If (temperature < 0°C) Then precipitation is only snow (phase = 1)
Else If (temperature > 2°C and temperature < 2.4°C) Then Phase = -(0.25 * temperature) + 0.6
Else Phase = (((-0.0273 * temperature^ 5) + (0.1606 * temperature^ 4) - (0.1653 * temperature ^ 3) - (0.3053 * temperature ^ 2) - (0.0145 * temperature) + 0.9927)

Even though a 76% correction seems to be very high, for specific snowfalls, the corrected amount of precipitation recorded by the precipitation gauges has been compared to direct field measurements (snow depth and density) by Wagnon et al. (2009), confirming that a correction was required, even though these local measurements on plates were site specific and did not allow getting the exact correction a the scale of the glacier. The SEB modeling of the snow cover (Wagnon et al., 2009) also confirmed that the proposed precipitation correction was necessary to accurately model the observed ablation and snow duration.

Moreover, we did not remind in the text and figure caption that the modeled surface elevation presented in figure S2 was performed with a correction of precipitation (the best fit between modeling and measurement is obtained if precipitation is increased by 120%). If we do not apply any correction, the measured point mass balance modeling is not reproduced (see the proposed new Figure S2, called Figure R4 in attachment). Importantly, the computed monthly mass balance values were still highly consistent with measurements assuming the correction proposed by Wagnon et al. (2009) ($r = 0.89$, $n = 18$, $p = 0.001$, $E = 0.73$, Figure R4). This confirms that raw precipitation measurements must be corrected, and that the correction proposed by Wagnon et al. (2009) is accurate. We also observed that the climatic mass balance is correctly modeled with the degree-day model only if the precipitation correction is considered (see our new Figure 4).

Finally, in our response to Reviewer#2, we propose a new validation of the model used in “real” conditions (i.e. here the model is also used to reproduce snow accumulation and surface state) during 530 days. This modeling shows that a 76% correction of precipitation is justified if we want to correctly reproduce the surface state with the degree-day model.

This would strongly suggest that the rain gauges are failing to capture precipitation adequately.
We agree but such measurements are still useful once a correction is applied.

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.
This equation results from observation of snowfall frequency on Antisana Glacier 15 (Wagnon et al. 2009). The equations are now included in the supplementary materials.

It is stated on L297 that 70% of the precipitation is solid, but an altitude range needs to be provided
Indeed thanks. The elevation will be added directly in the text. Importantly, we reanalyzed the frequency analysis from L’Hôte et al. (2005) and observed that snow precipitation frequency was larger at 4900 m a.s.l. (95%).

(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.
Please see our previous comment. The correction method accounts for this (see above). This correction is performed to get accurately the total precipitation amounts at 4900 m a.s.l.. Then, during the degree-day modeling, we perform a separation of snow/rain according to more simple temperature threshold (solid precipitation is assumed if the air temperature is below a threshold (Tsnow/rain = 1 °C). We verified that, on total precipitation, this separation gives similar proportion of snow fall than the full method proposed by Wangon et al. (2009). As a consequence, changes in the frequency distribution of snow/rain according to elevation are accounted for in our study.

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.
We totally agree with this remark. But please note that our results are in perfect agreement with (Wagnon et al., 2009) and (Basantes-Serrano et al., 2016), confirming that raw precipitation are largely underestimating the actual precipitation at the study site.
We propose to include a specific section on this point in the corrected manuscript.

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).

Indeed, this point needs a clarification. Basantes-Serrano et al. (2016) do not take into account any precipitation correction and gradient in their calculations, and mentioned that this assumption should be revisited, and that "the accumulation processes (including the role of frost deposition) need to be carefully analyzed."..

Indeed, one of the main conclusions in Basantes-Serrano et al. (2016) is to demonstrate that accumulation from snow pits was underestimated by about 60-70%. This conclusion suggests that accumulation is larger than raw precipitation values measured in the foreland and that a positive vertical gradient is needed to obtain accumulation values in the upper reaches if raw precipitation data are representative of the actual precipitation amounts. However, they also mentioned that the precipitation in the glacier foreland is known to be underestimated, by about 50% according to Wagnon et al. (2009) (please remind that in Wagnon et al. (2009), they were assuming the precipitation amounts given by the Geonor rain gauge, not by the P4 used in our study), suggesting that raw precipitation data should be revisited.

Basantes-Serrano et al. (2016) did not state which of the two options is the more realistic. Our paper allows going a step further and confirms that the correction suggested by Wagnon et al. (2009) is needed to 1) correctly model the surface mass balance at 4900 m a.s.l. using a SEB modeling (see the new Figure S2, called Figure R4 in attachment), and 2) correctly model the glacier-wide mass balance and accumulation at the summit using the PDD modeling if no precipitation gradient with altitude is considered.

We clarify this point in the revised manuscript

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.

We agree with this remark and we propose to include more information on the precipitation amount (and gradient) on Antisana Glacier 15.

5. L180 and L184: Why do the authors refer to surface energy balance (L180) and then surface mass balance (L184) – are they different?

This remark makes sense, we compute the surface energy balance in both situations. We now write "surface energy balance" in both cases in the text.

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)?

This remark makes sense, and the value of 30% has to be corrected in the text. Indeed, the 30% difference was referring to the modeling made by Favier et al. (2004). However, in the supplementary material we demonstrate that Favier et al. (2004) approach is not sufficiently accurate, and we write: ”Our results were in better agreement with measured melting amounts than results from Favier et al. (2004), with a correlation coefficient of r = 0.91 (instead of 0.86). The regression line was also closer to the 1:1 line (slope of 1.01 instead of 0.89)”. Actually, with the Favier et al. (2011) approach the difference is reduced (20%).

Finally, please note that 1) the melting boxes present large uncertainties and should not be considered as the only way to validate models because: 1) part of the liquid water is retained by/between the small ice blocks due to capillarity, 2) melting may occur below the first 20 cm of snow/ice, and 3) the surface state in the box and under albedometer may differ if we consider the patchy distribution of snow on the surface. The potential uncertainties of the SEB modeling justify why we first calibrated / validated the degree-day model with the SEB and melt boxes at 4900 m a.s.l. but next used supplementary validation data:

1. The monthly surface mass balance at 4900 m a.s.l. between 2000 and 2008,
2. The glacier-wide climatic mass balance between 2000 and 2008,
3. The snow-line and the ELA locations between 2000 and 2008.

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.

Reviewer#1 is right but for the calibration/validation step only (Section 5). This procedure with albedo was done, in order to calibrate and validate the model on ablation data only, assuming that the separation between surface states was accurate and based on field observation, because performing a “real” modeling of snow accumulation would depend on precipitation correction. This procedure was applied because we wanted to remove the uncertainty in snow accumulation modeling resulting from the precipitation uncertainty.

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?

It does not prevent the use of the model elsewhere. However, this method is a good way for the calibration of the degree-day factors elsewhere. This point is included in the discussion

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.

We do not really understand this comment. The mass balance Bn is obtained with stakes data, and snow accumulation is obtained using solid precipitation estimations (measured with the corrected precipitation data as described before). Assuming that Bn = Accumulation – Ablation, this equation gives the ablation. This equation is true even in case of mass gain or mass loss.

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.

The seasonality of the lapse rate values on the Antisana Glacier 12 was already introduced in the text (L307-309), using observations from different AWS. A sensitivity test was added to assess the impact of the seasonal variability of LR on the mass balance modeling (see Table 6), showing that the induced uncertainty is low. However, assessing the spatial and temporal variability over a longer period and/or at a larger spatial scale requires long term observations or a very high resolution atmospheric modeling (please remind that the glacier is about 2 km long and 200 m large). Such modeling is out of the scope of this study.

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 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.

Reviewer#2 also finds that including the sublimation in this model is not really justified. However, please not that including this process was requested by a reviewer of the first version of the manuscript, and that we observed that accounting for sublimation is giving more realistic mean climatic mass balance with the model. 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. However, note that the year-to-year changes in sublimation are not significant in comparison with changes in melting, and including this simple sublimation model does not change correlations between annual measured and modelled mass balances.

Concerning the comment on frost deposition suggested by Basantes-Serrano et al. (2016), please note that their assumption does not refer to any energy balance modeling but is an hypothesis to explain why accumulation estimates from rain gauge and snow pits are underestimating the actual accumulation at the summit (i.e. the accumulation suggested by the geodetic mass balance) if raw precipitation data are assumed to be correct. Conversely, in our paper our assumptions on the sublimation gradient is absolutely not speculative: it is the result of an average of the physical processes controlling sublimation described in the paper, and this conclusion is in agreement with the distributed SEB modeling from L. Mainsicho PhD thesis (Maisincho, 2015) (see attached Figure R1) showing that sublimation does not significantly change with elevation.

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).

We removed the word "slightly" when it was not adequate and remove other words which were inadequately suggesting that results were "very" good.

Why is there first an overestimate and then an underestimate do you think?

The calibration in 2002-2003 leads to high degree-day factors giving elevated amounts of ablation, these coefficients yield high ablation in 2005 compared to measurements. Conversely, the calibration with data from 2005 leads to low ablation values in 2005. This implies that the computed ablation (using these coefficients) is also logically low in 2002-2003.

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
The albedo is only used in the calibration step at 4900 m a.s.l. (Section 5). In this calibration/validation step, we wanted to model ablation of snow or ice independently from snow precipitation corrections. As a consequence, we did not perform a "real" modeling with the model in order to reproduce presence of a snow cover. Since we did not have any continuous visual observation of the presence of snow under the sensors, we referred to the surface albedo.

and please explain how the presence of snow at the surface is computed –
For the "real" modeling performed in Section 6 and 7, we computed both the ablation and the snow accumulation. In this case, the presence of snow was assumed if the amount of snow in mm w.e. at the time step j-1 was positive (see line 287). The amount of Snow Sj at time step (j) was computed as follows:

If $S_{j-1} + ACC_j - m_j - sub_j > 0$ then: $S_j = S_{j-1} + ACC_j - m_j - sub_j$
Otherwise : $S_j = 0$

Where ACC_j is the snow accumulation at the time step j, m is the melting and sub the sublimation. We added these equations in the text.

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).
As explained before, the calibration was made with the albedo (to separate snow and ice) and was independent from precipitation amounts.

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).
We attenuated this sentence, however, we do not agree with Reviewer#1 and we explain why this figure shows that the surface states are accurately reproduced.
Indeed, another option may be to plot the duration of the snow cover for each month in the observation and in the model. However, with monthly observations, there is no way to define if snow was present during 1 day or over the entire month. As a consequence, snow measurements at stakes do not inform on the mean surface conditions that the model should reproduce.
Conversely, in our figure, observations 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. The model also correctly reproduced when the high ablation amounts were recovered, demonstrating that the modeled snow cover disappeared at the same time in observation and in the model.

Please clarify what "worked perfectly" actually means (L408).

We attenuated the word “perfectly”

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

14. L498-503: Please clarify the statement that the calibration is not very sensitive to the albedo threshold.
We attenuated this sentence. However, note that Table 5 shows that results of the calibration done with 2002-2003 data using the albedo threshold of 0.56 instead of the best value of 0.49. The difference between these degree-day factor values and those resulting from the best calibration (that is with an albedo threshold of 0.49) is less important than the difference observed between the best calibrations in 2003 and in 2005 (line 1 and line 3 of Table 5). This means that differences between ablation conditions for two distinct years have more impact than considerations on the albedo threshold. Actually, reviewer#1 will observe that previous Table 5 was presenting the sensitivity analysis when differences in albedo threshold and in Temperature threshold are included. Thus, we now also provide additional values accounting for changes in the albedo threshold only.

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. 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.
We agree with the reviewer that the value of albedo threshold is key, and this sentence was clarified: the differences in the degree-day factors values associated to changes in ablation conditions for two different years are larger than those resulting from the albedo threshold value used during the calibration process (assuming that albedo threshold value remains within typical range: [0.49, 0.56]).

Final comment: The observational data presented in this manuscript are of interest to readers given the challenges to extract such information from tropical glaciers.
We thank the reviewer for this comment and our paper clearly offers access to the unpublished database.

However, the manuscript falls a little short in adequately accounting for the physical processes responsible for mass balance using the degree-day approach.
We find that reviewer’s comment is not clearly objective here. As written in point 3 of our summary of this letter:

1) we present a new SEB approach at 4900 m a.s.l. which gives important conclusions on the role of subsurface processes.
2) we present a similar Table with correlation coefficients as given in Sicart et al. (2008) demonstrating that their study was based on a too short observational period. The current study also confirms results from Favier et al. (2004) on the key role of the net shortwave radiation and of albedo in melting processes.

3) we used this table in order to understand why the Temperature and the mass balance are well correlated in comparison to other places where the degree-day model is largely used and accepted as a good tool to study ablation (for instance, Storglaciären).
4) In this letter, we show new results from the distributed SEB modeling at Antisana Glacier 15 performed by Maisincho (2015). These results confirm that S and albedo are crucial to explain the mass balance variations, suggesting that the processes explaining the link between T and Melt are possibly true at higher elevation. They also confirm that sublimation do not vary significantly with elevation. A publication on the distributed modeling of the surface mass balance of Antisana glacier 15 is currently under preparation, but including more information from the SEB in this paper would lead to a very long paper, and is clearly impossible here. Results from this PhD thesis are now cited in the text.

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.
We believe that this remark is not true, but demonstrates that more details have to be given on precipitation amount at Antisana Glacier 15. Indeed, our modeling allows demonstrating that precipitation must be corrected using the method proposed by Wagnon et al. (2009). Moreover, in the paper we already present results from distributed temperature measurements performed on Antisana Glacier 12.

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: 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
All these comments were considered.

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Figures

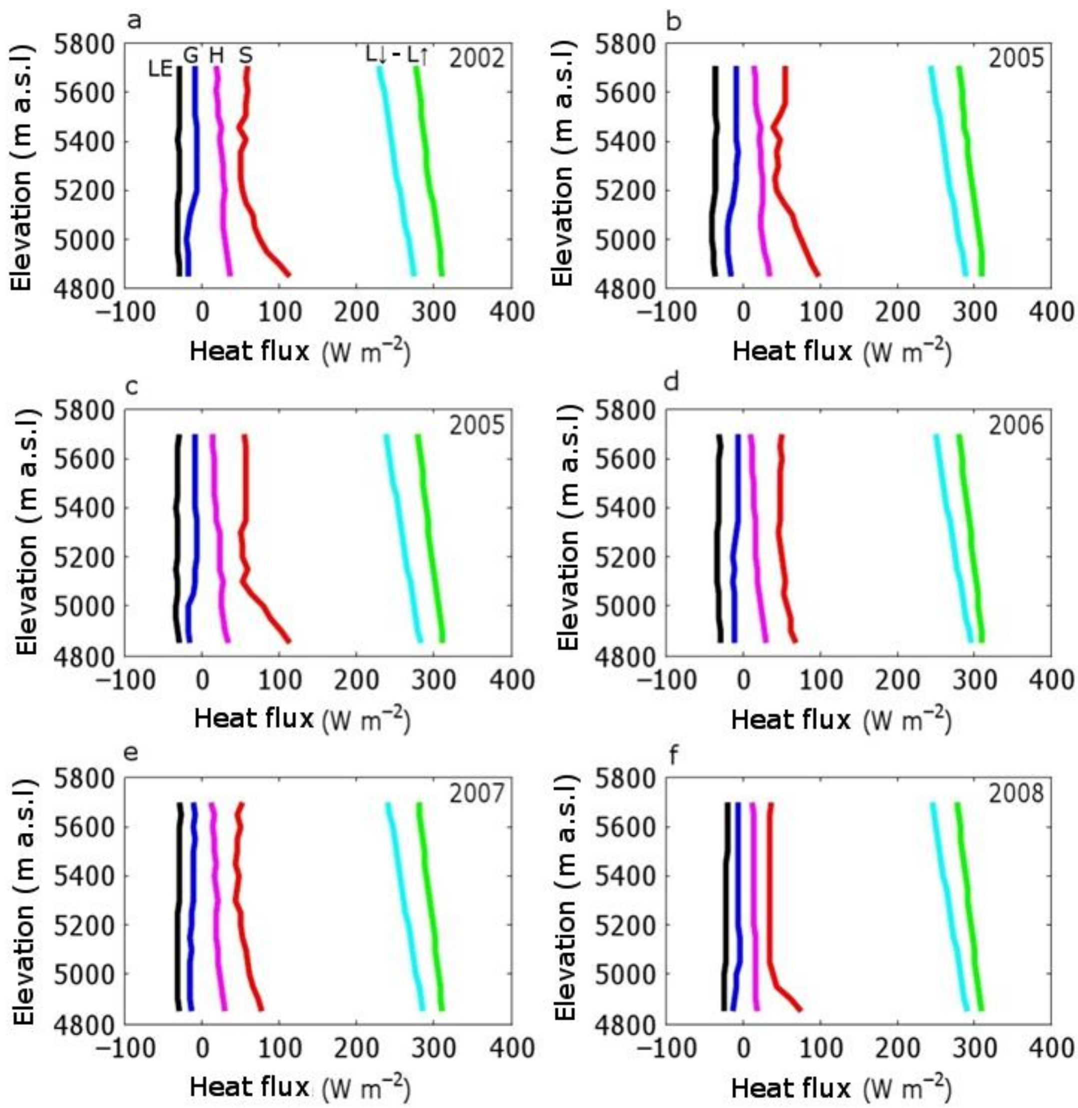

Figure R1. Evolution of the heat fluxes with elevation between 2002 and 2008, using data from the AWS_{G1} in figures a&b and from AWS_{M1} in figures c,d&f. Colored lines are for LE (in black), G (dark blue), H (pink), S (red), incoming longwave radiation (light blue), and minus the outgoing longwave radiation (green).

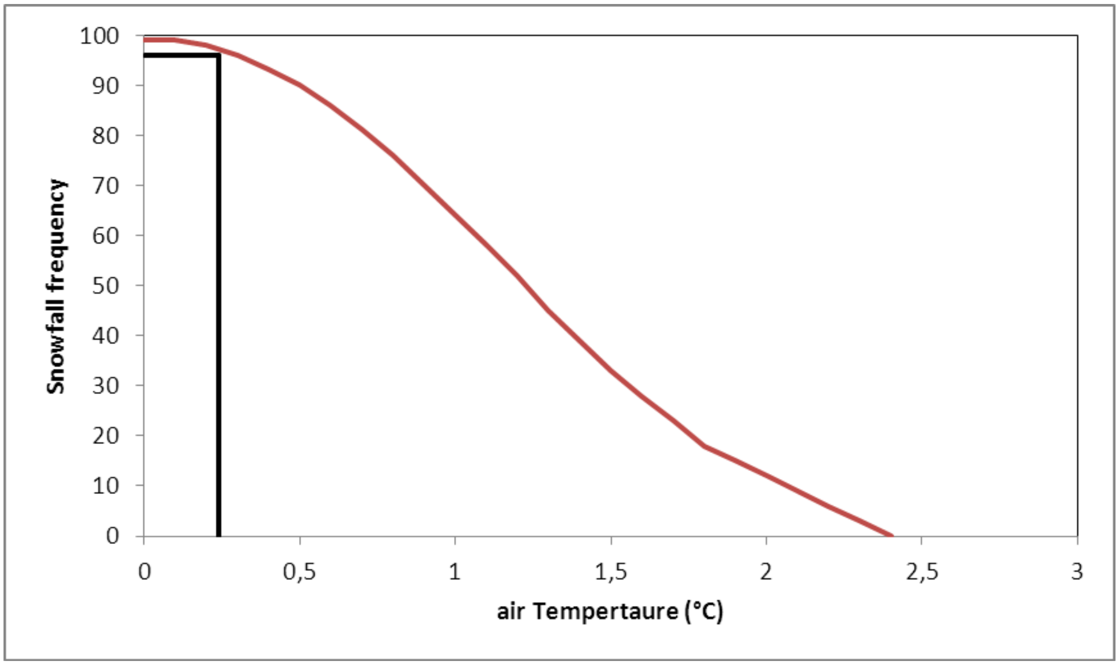

Figure R2: evolution of precipitation phase with temperature (red line). The black line shows the frequency (95%) of solid precipitation events for mean temperature (0.24°C) computed only for days with precipitation occurrence between 2000 and 2008.

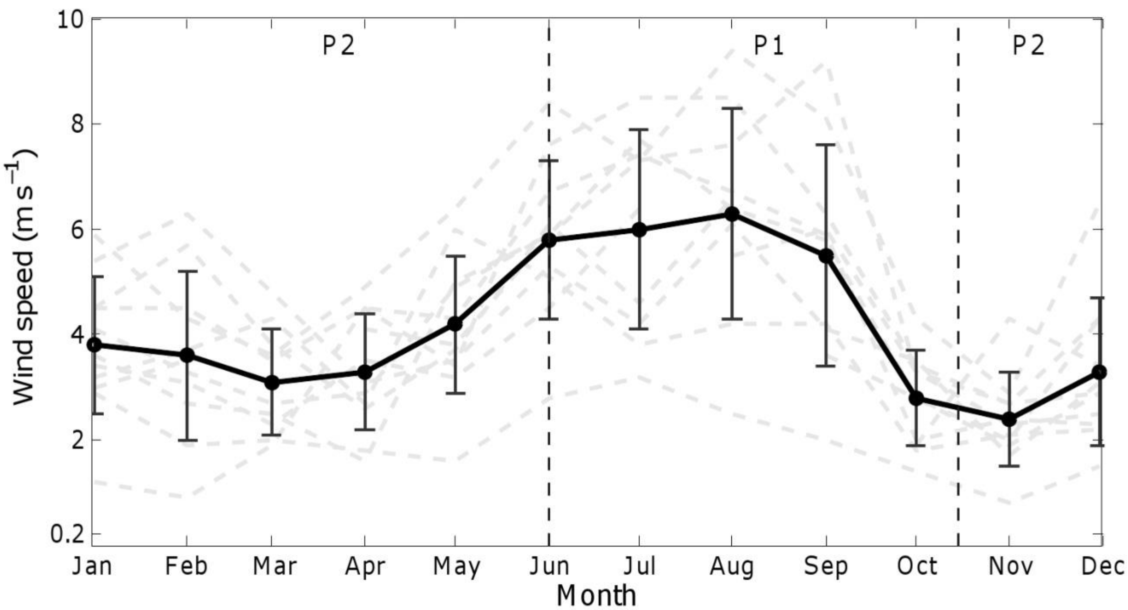

Figure R3: Separation between period 1 and Period 2 according to wind speed. The Continuous black line is the mean monthly wind speed over 2000-2008. The discontinuous grey lines are the mean monthly wind speed values for each year. Finally the vertical bars indicate the standard deviation of monthly wind speed over 2000-2008.

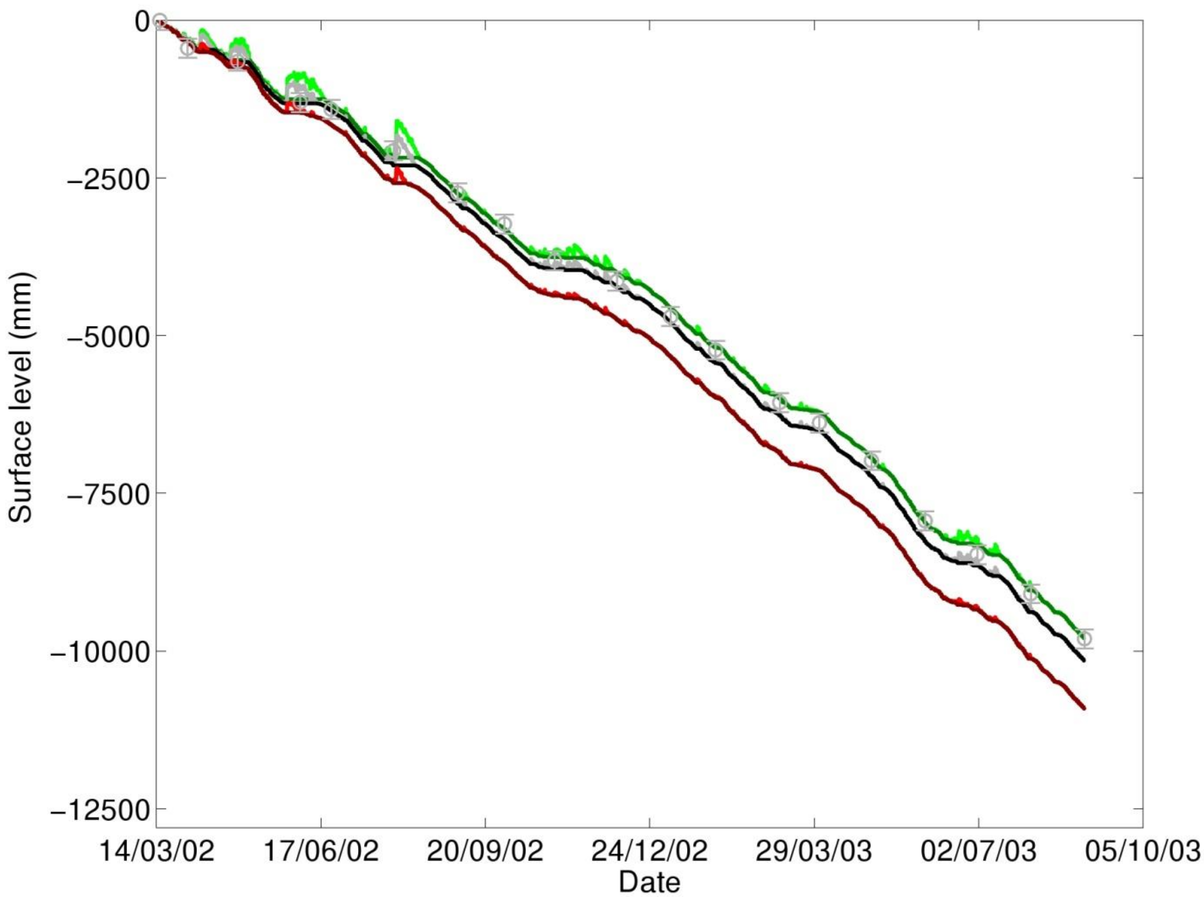

Figure R4: Comparison between measured (circles) and modeled surface mass balance and using the full surface energy balance approach in Favier et al. (2011) using a 120% increase in precipitation (green curves), the 76% correction propose by Wagnon et al. (2009) (black curves) and using raw precipitation data (red curve) (values expressed in m of ice). Results are for 4,900 m a.s.l from March 14, 2002 to August 31, 2003. The gray (light pink and light green) lines represent the level of snow for each situation assuming a density of 200 kg m^{-3}.