Response to reviews

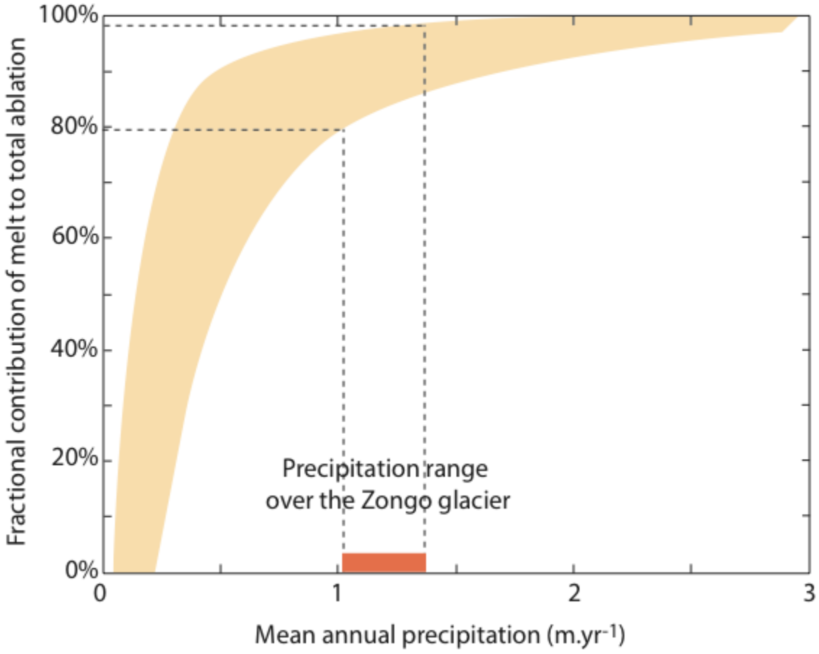
We are grateful to the 2 reviewers for their careful evaluation of the manuscript. We agree with the majority of their comments and we believe that the concerns they raised helped us a lot in improving the manuscript. We describe below how we propose to answer and address their comments during the revision of the manuscript.

Response to reviewer 1

Specific comments

1) Validity of the calibrated parameters for different climatic conditions. We agree with the reviewer's criticism that the accuracy of our PDD model may be altered under different climatic conditions than those of the Zongo. Our section 5.3 entitled "**Physical limitations of the temperature-index models**" was initially written to warn the reader about this caveat. However, to take into account the reviewer's comment, we decided to go a step further in revising our manuscript. For this we added a new figure showing the fraction of melt in total ablation vs the mean annual precipitation. This graphic is based on an energy balance approach applied to a distributed model for the present-day glaciers of Central Asia (Rupper and Roe, 2008). This graph is very useful because it shows that the amount of precipitation is a key 1st order parameter controlling the respective proportion of melt and sublimation. Annual precipitation of the Zongo glacier is between 1000 and 1350 mm.yr^{-1} and, in this case, sublimation represent less than 20% of total ablation (Favier et al., GRL., 2004). Consequently, under these climatic conditions, neglecting sublimation is not a major source of error while the annual glacier-wide mass balance is considered. We reworked this section and added new sentences to state that a new sentence in the text to explain that *"melting remains the dominant ablation process (>75% of total ablation) while precipitation is above 0.5 mm.yr-1 (Fig. 6) (Rupper and Roe, 2008). Sublimation however becomes the major ablation process when precipitation is below 0.25 m.yr-1 (Fig. 6)."*

We also revised the last sentence of the paragraph to state that *"If precipitation are lower than 0.25 mm.yr-1, these models (1, 2, 3) may significantly underestimate the total ablation. This could lead to inaccurate paleoprecipitation and paleotemperature reconstructions. In such configuration, the structure of model 4 clearly represents a more realistic way to calculate the mass balance of tropical glaciers developed in dry areas. Nonetheless, the calibration of the 'a' parameter of model 4 should ideally be done using mass balance data from a glacier developed in a drier tropical zone than the Zongo. Unfortunately, there is a lack of mass balances records in such areas."*



NEW FIGURE: Figure 6. Mean annual precipitation vs fractional contribution of melt to total ablation. Modified from (Rupper and Roe, 2008). This curve was obtained from a distributed energy balance model applied to the present-day glaciers of Central Asia.

2) ***In my opinion, the authors should provide an example of a paleoclimate reconstruction (with the presented models) in order to be in line with the scope of "Climate of the Past" and in order to validate their claim, that PDD-models are able to reconstruct paleoclimate conditions based on paleoglaciers in today ice free areas of the tropics.***

We do not really agree with this claim that providing an example of paleoclimatic reconstruction would be a test of the model accuracy. Indeed, for a really robust test, one would require a paleo-englaciated site, where precipitation and temperature would be known accurately and precisely from another quantitative and independent proxy. We are not aware of the existence of such ideal configuration. However, we think that another more realistic way for testing the modeled output (precipitation-temperature curves) is to compare these curves with the precipitation-temperature curves observed today at the ELA of glaciers. We initially followed this approach during the writing of section 5.4, as an attempt to evaluate whether the model output is realistic or not. We however decided to modify completely this section, in order to be more specific. We also modified Fig. 6 (which is now Fig. 7) to show the climatic conditions observed today at the ELA (grey envelop in Fig. 7) from a worldwide dataset of glaciers (Ohmura et al., 1992). This allows a comparison between these data and the model results. This comparison is carefully discussed in section 5.4 to specify the precipitation range for which the model results seem to be more robust. We also added there a reference to the study of Condom et al., GPC, 2007 dedicated to the tropical glaciers of Andes:

"The shape and slope of these modeled precipitation temperature curves can be compared with the dataset obtained by previous studies by compiling the climatic parameters at the ELA e.g. (Condom et al., 2007; Ohmura et al., 1992; Shi, 2002). Grey envelops in Fig. 7 show the range of temperature and annual precipitation at ELA reported by (Ohmura et al., 1992) from a large worldwide dataset of glaciers. Although the comparison should be considered with caution given that (Ohmura et al., 1992) reported summer temperatures rather than annual temperature, the 4 models yield precipitation-temperature curve that are largely compatible with the observed climatic conditions over a wide rang of precipitation. Notably, the dP/dT slopes of both estimates agree quite closely over the precipitation range between 500 and 1500 mm.yr-1 (Fig. 7). However, it must be noted that the dP/dT slopes of the modeled curves become larger than the observation curve for precipitation higher than 1500 mm.yr-1. This probably indicates that our PDD models underestimate the influence of snowfall on ablation. Indeed, models 2 and 3 having lower ablation parameters for snow than for ice better match the observations of (Ohmura et al., 1992) (Fig. 7). Although the model-data discrepancy remains difficult to understand, a possible explanation might be that the (Ohmura et al., 1992) curve is built by considering only summer temperatures, while the present work considers variations in mean annual temperature, without including any change in seasonality. Another possible bias of our reconstruction is that our model does not consider any correlation between the lapse rate and precipitation. Several observation indeed indicate that the lapse rate may be correlated with precipitation (Pigati et al., 2008). Including such a relation in PDD models yield P-T curves characterized by a much slower and more realistic dP/dT slopes, for precipitation higher than 1500 mm.yr-1 (Fig. 4 in (Pigati et al., 2008)). Indeed, higher precipitation increases the lapse rate, which induce a negative feedback on the amount of ablation at high elevation.

Further work remains necessary to test and improve the ability of our models to mimic the climatic conditions at the ELA, particularly for dry (< 500 mm.yr-1) and wet conditions (> 1500 mm.yr-1). Nevertheless, for precipitation ranging between 500 and 1500 mm.yr-1, the P-T curves yielded by our models are consistent with the precipitation-temperature sensitivity constrained by present observations at the ELA (Fig. 7). This quite good agreement suggests that these models do not yield unrealistic results when they are used to reconstruct climatic conditions from past extents of tropical glaciers."

Based on the comments above, the title as well as the content of the paper could (should) be: "Degree-day melt model calibration for tropical glaciers: An example from Zongo glacier (Bolivia, 16°S)". Having the data given in the paper as well as the PDD-model comparison available is scientifically a step forward. If this content, as it stands now, is in line with the scope of "Climate of the Past" needs, however, to be assessed by the editors.

We understand that it could be more attractive for the readers of CoP if we could provide examples of paleoclimatic reconstructions relying on these calibrated models. We thus propose to include a new section (section 5.5) with a short summary of the different sites and time periods where we applied this model for paleoclimate reconstruction:

* **Hawaiian islands - LGM (19 ka) and Heinrich 1 (17-15 ka)** (Blard et al., Nature, 2007)
* **Central Altiplano, Bolivia - Heinrich 1 (17-15 ka)** (Blard et al., QSR, 2009)

*** Cordillera Real, Bolivia - Late and Mid Holocene and Little Ice Age** (Jomelli et al., Nature, 2011).

In each case we discuss the uncertainties associated with these model-based reconstructions.
As a consequence of this modification, we would like to keep unchanged the original title of the manuscript. Here is below the text included in this new section 5.5:

5.5 Examples of paleoclimatic reconstructions using these PDD models

A model whose structure is very close to the one of model 3 has been used in previous studies to reconstruct Late Pleistocene and Holocene paleoclimatic conditions in different tropical areas (Blard et al., 2009; Blard et al., 2007; Jomelli et al., 2011). In these works the chronologies of past glacial extents were constrained by cosmogenic ^{3}He or ^{10}Be dating of moraines. One of this study, (Blard et al., 2007), explored the paleoclimatic significance of the last glaciation in the Hawaiian Islands. The model indicated that high elevation paleotemperatures were 6 to 7°C colder than today at 4000 m in the Central Pacific, from the last glacial maximum until about 15 ka BP. In this study, precipitation was independently constrained by pollen records between 250 and 500 mm.yr-1 (Hotchkiss and Juvik, 1999). Given that sublimation is probably the major ablation process under such dry conditions, it is clear that our temperature reconstruction could potentially be affected by a bias. The conclusions of this article should thus be thoroughly tested using a distributed energy balance ablation model.
A derivate of model 3 was also used to infer the paleoclimatic conditions prevailing during the Lake Tauca episode in the central part of the Altiplano (Blard et al., 2009). Cosmogenic 3He dating of the moraines deposited over the Tunupa volcano (Blard et al., 2009) established that the local glacial maximum in the center of the Altiplano was broadly synchronous with the Lake Tauca highstand, between 17 and 15 ka BP (Blard et al., in press; Placzek et al., 2006). This synchronism allows a combined modelling of the lake hydrological budget with the calculation of the Tunupa glacier mass balance. Since lake and glaciers have contrasted sensitivities to precipitation and temperature, this combined approach permits a significant reduction of the uncertainties results: our calculations indicate that from 17 to 15 ka BP, the centre of the Altiplano was characterized by temperature ~6.5°C cooler and average precipitation ranging between 400 and 620 mm.yr-1. Although sublimation is not dominant for this precipitation conditions, it is clear that the use of an energy balance model would be useful to explore the limits of these precipitation-temperature reconstructions.
Finally, model 3 was also used to reconstruct the climatic condition prevailing during the Holocene epoch over the Telata massif (Jomelli et al., 2011). Our modeling indicates that temperatures were depressed by 3.5°C during the Early Holocene and were still 2.5°C cooler during the Little Ice Age (for unchanged precipitation). Because the Telata location is not far from the Zongo glacier (less than 7 km), atmospheric conditions are very close to those of the calibration site, which suggests that these reconstructed paleoclimatic conditions are probably not affected by a significant inaccuracy.

Technical notes:

1. (p.2121, lines 13-14): for what range in precipitation does this relation hold, was

this relation tested?

We modified this sentence:
"In particular, it is important to keep in mind that classical temperature-index models are not suited for high elevation (>6000 m) tropical glaciers fed by low precipitation (< 500 mm.yr^{-1}), where wind erosion and sublimation are the dominant ablation mechanisms (Wagnon et al., 2003)."

2. (p. 2123, lines 15-20): this has been elaborated in detail in the central Andes e.g. by Kull and Grosjean 2000, or Kull et al. 2008

These articles rely on energy balance models, we now quote them in the Introduction.

3. (p. 2124, lines 14-18): see specific comment (1). PDD-models should be used only under climate conditions favoring melting as the dominating ablation process.
Sublimation plays an important role on tropical glaciers even under today's climate conditions (Winkler et al. 2009). Besides temperature, the atmospheric moisture content plays a crucial role determining the dominant ablation process (sublimation versus melting) (e.g. Juen et al., 2007).

We modified these lines in the Introduction section to make the reader aware of this limitation: PDD models should be used only when melting is dominant over sublimation. We also modified the section 5.3 to quote Winkler et al., 2009 and to indicate that the amount of sublimation is controlled by the atmospheric moisture: *"The atmospheric moisture content plays a crucial role in determining the dominant ablation process e.g. (Juen et al., 2007). However, a recent modeling study has shown that the amount of precipitation may be used as a first order proxy to estimate the sublimation/melting ratio (Rupper and Roe, 2008)."*

4.) (p.2141, lines 16-18): comment: This is the case when modeling climate conditions from paleoglaciers in today ice free areas…

We don't think it is necessary to change this sentence.

5.) (p.2141, lines 18-23): and…. e.g. Ginot et al., 2006; Kull et al., 2008, Winkler et al. 2009

OK, we added these 3 references.

6.) (p. 2144, lines 1-9): this has been done in detail for the central Andes by Kull and Grosjean 2000, as well as Kull et al. 2008

OK, so we modified the penultimate sentence of the Conclusion: *" Eventually, it could be* *appropriate to move toward energy balance models under certain favorable conditions, as proposed by (Kull et al., 2008) and (Kull and Grosjean, 2000) in their pioneer studies."*

7.) I suggest considering additional literature and results as e.g. mentioned above in order to put the study in a broader context.

OK, we added this references and, as explained above, we added more words about the implications of these previous studies.

Response to reviewer 2

Review of Climate of the Past manuscript number: 2011-64 entitled "Degree-day melt models for paleoclimate reconstruction from tropical glaciers: calibration from mass balance and meteorological data of the Zongo glacier (Bolivia, 16° S)"
Overall impression:
This paper gives a thorough comparison of the application of various kinds of temperature index models on a single tropical glacier in the Bolivian Andes. It utilizes a very complete data set of mass balance and meteorological observations to calculate mass balance profiles for average conditions as well as individual years. This alone makes this paper unique enough for publication. The motivation for the research is clearly stated as being for the purpose of using such models for reconstructing temperature and/or precipitation conditions at locales where paleoglaciers existed. However, much of the material to that end seems added on as an afterthought or is less well developed than the rest of the paper. A more thorough study would apply this work to a couple of test glaciers for the purpose of reconstructing climate and for revealing the potential uncertainties in doing so.

In order to address a similar criticism of Reviewer 1, we included a new section (5.5) providing examples of 3 different paleoclimatic reconstructions that were already published (see above).

1. Does the paper address relevant scientific questions within the scope of CP?
The paper addresses relevant questions which are somewhat outside the scope of CP

2. Does the paper present novel concepts, ideas, tools, or data?
The paper combines existing techniques and explores their application in a novel way

3. Are substantial conclusions reached?
The conclusions are strong but not necessarily substantial. I would like to see a more thorough application to paleo glaciers.

We addressed this question by added a new section 5.5.

4. Are the scientific methods and assumptions valid and clearly outlined?
Yes.

5. Are the results sufficient to support the interpretations and conclusions?
Yes.

6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?
Yes

7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution?
Yes

8. Does the title clearly reflect the contents of the paper?
Yes

9. Does the abstract provide a concise and complete summary?
Yes

10.Is the overall presentation well structured and clear?
Yes

11.Is the language fluent and precise?
There are many grammatical errors in this paper. I've suggested fixes for many of those, but I'm sure there are a lot more. This paper needs a very careful reading by a native English speaker.

We tried to perform all the English corrections suggested by the reviewers. However, if the paper is accepted for publication, we will be happy to make the manuscript carefully corrected by an English speaker (among our colleagues)

12.Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?
Yes

13.Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?
No

14.Are the number and quality of references appropriate?
Yes

15.Is the amount and quality of supplementary material appropriate?
NA
Overall, I think this paper is acceptable with major revisions. If a re-review is needed, I would be happy to provide one.

Major Issues
--It appears that the comparisons are made against what look like smoother mass balance profiles rather than the much more variable stake data distributed across the glacier surface. It's not clear whether the models are run in a fully distributed fashion. but, if they are, then it might make sense to analyze the mass balance at every stake rather than against the smoother, average balance profile for a given year.
Judging the model comparison based on the mass balance profiles may not allow the more complex models incorporating solar radiation to perform as well as they otherwise would.

As the spatial density of the mass balance data is quite poor, we do not use a fully distributed model. Instead, we use the local slope characteristics (slope, orientation) at each elevation step. We added a sentence in section 3.3.4 to make this clear: *"θ was calculated for each altitude step (between 4950 and 6050 m) by using a 30 m digital elevation model of the Zongo surface. This calculation is performed taking into account the local slope of the glacier at each elevation step."*

--In the abstract, the author refers to the technique of obtaining a temperature and precipitation change from a glacier area as an "inversion". While inversion techniques could be applied, the methods used here and for most other similar studies are not formal inversions. Thus this terminology needs to be changed.

OK, we removed this word and modified the sentence into: *"Calculations indicate that the model provides a better reconstruction of the mass balance if the ablation is modeled with different melting factors for snow and ice."*

--There are very many typographical and grammar errors in this paper that need to be fixed before publication. I outline some of them below, but there are surely more.

Thanks for your careful corrections. We applied all the suggested modifications.

Minor Issues:

Page 2122, lines 7-10. It is stated here that "computation of accumulation is generally quite straight forward". This idea is reflected in the treatment of accumulation in this paper wherein a uniform value is applied everywhere in the accumulation area. I think the assumption and the treatment of precipitation is far more complex and this needs to be addressed in the paper. When the authors finally get around to looking at T vs Pre curves, the temperature variability for a given precipitation amount is analyzed, but the precipitation variability is not discussed.

The PhD thesis by Lejeune established quite well the link between temperature and the nature of precipitation in the Zongo area. However we modified this section in order to be more prudent: *"As the link between temperature and the nature of precipitation is quite well constrained by observations e.g (Hock, 2005; Lejeune et al., 2007; Vincent, 2002), the computation of accumulation generally does not represent a major source of uncertainty."*

Page 2125, line 4: What exactly does "characteristics of a temperate glacier" mean here? Are you discussing the climate? The ice temperature? The accumulation/ablation seasonality?

This is related to the ice temperature. We added few words to avoid confusion: *"Given the temperature of its ice, the Zongo has the characteristics of a temperate glacier (Francou et al., 1995)."*

Page 2125, line around 22: I think you are referring to temperatures measured within the glacier's boundary layer versus those measured away from the boundary layer. Please change the wording to reflect this.

We added few words: *"...it is relevant to calibrate the model using local temperatures measured out of the Zongo glacier, i.e. away from the boundary layer."*

Page 2126, line 15: Aren't the lapse rates also governed by precipitation amount and rate? Aren't latent heating effects important here?

We added the word "precipitation" in the list of the factors controlling the lapse rate.

Page 2132, Model 3: Although it's discussed later, I'm not sure why you use a different formulation than Hock (1999)? For comparison, wouldn't it be more useful to a wider audience to maintain her original formulation?

We initially tested the following snow ablation law (similar to Hock, 1999):
$A = (MF + a_{snow} * I) * T_p$). However, the calibrations we performed in this study always yielded $a_{snow} = 0$ as best results, for all hydrological years. That is the reason why we decided to slightly modify the initial formulation of Hock and keep the I term for ice ablation only.

Page 2137, lines 9-10: What does "such a smooth definition of the best R2 mean?

We replaced these words by "*such a low sensitivity*".

Page 2138, line 9 or so: You are comparing the performance of the various models within the accumulation area but your model for snow accumulation seems blatantly wrong to me. I understand your choice of a fixed accumulation rate up there based on limited measurements, but maybe a better approach would be to compare the actual measured accumulation with the modeled rather than the curve based on smoothed data.

This criticism is hard to understand: the modeled accumulation curve is not modified by any smoothing or interpolation. As described in the article, accumulation and ablation are calculated as discrete elements at each elevation step.

Page 2143 line 24: The statement that temperature can be constrained within a degree may only be true if the precipitation is known exactly! This will rarely be the case, so the actual constraint will be much looser and will involve uncertainty in temperature and precipitation. This needs to be clarified in the text.

We added this condition in the sentence: *"... if precipitation is known exactly, these models are able to yield temperature reconstructions with ~1°C uncertainty."*

Editorial:

The constructs many sentences by beginning with Indeed,... These can mostly be simplified by concisely stating the sentence without the lead-in.

2121-4: the ablation models aren't “laws” they are parameterizations or relationships
2121-10: change topographic to topography
2121-12: depluralize timescales and strike “The performed”
2121-22: change relative to relatively
2121-24: insert “the” between in and main
2122-3: use nonexistent rather than inexistent
2122-8: remove the mass balance equation
2122-14: depluralize days
2123-2: replace several with “the” and insert “that govern melt” after processes.
2123-12: change “in” to “to”, change “reproducing” to reproduce, and change “day-night” to diurnal.
2123-23: change play to plays.
2124-5: depluralize “radiations” (here and throughout the text) and change “are” to is.
2124-12: depluralize “folds”
2126-12: strike “this parameter indeed” and replace with which.
2126-14: depluralize “covers”
2126-25: strike “the gauge to properly collect” and replace with measuring
2127-16: strike “Indeed”.
2127-26: depluralize “balances”.
2128-23: change “particularity” to capability.
2130-5: change “is” to as
2132-1: change “tan” to than
2133-9 to 10: Make read “These types of models (i.e. models 3 and 4) take into account...”
2133-11: Change “beams” to radiation.
2133-17: depluralize “temperatures”
2133-17: Make read “This model has the characteristic of allowing ablatiojn even when temperature
is...”
2134-9: make sentence read “Parameter optimization was realized...”
2134-10 depluralize “years”
2134-14: strike “permitted to obtain” and replace with “results in selection of”
2137-12: Switch the order of “improve significantly” to “significantly improve”
2137-14: strike “a”
2137-15: Change “dispersion” to scatter and “belong to” to “are of”
2137-25: Change “a specificity” to “specific to”
2137-29: change “in” to “to”
2138-1: change “reproducing” to reproduce.
2138-6: strike “are” and change “efficient” to “efficiently”
2138-27: change “possibility to take into account” to “ability to account for.”
2139-1: change “next” to future

2139-2: pluralize model
2139-2: Which models are you referring to here?
2139-9: make read "... permits the incorporation of geometrical..."
2139-16: pluralize "glacier"
2139-16: change "inexistent" to "nonexistent"
2139-17: switch order of wording make read "directly compare"
2140-11: change "larger than" to "as large as"
2140-13: strike "by"
2140-20: change "in" to "of"
2140-21: change "than" to "as"
2140-24: using word "factors" after MF is redundant. Strike second instance.
2141-7: change "than" to "as"
2141-11 to 12: change "have not a good ability in catching" to "are not capable of capturing"
2141-15: change "valid" to validate.
2141-16: move "as low as possible" after "input data"
2141-28: depluralize "precipitations"
2142-10 insert "required to maintain the present ELA" after "input data"
2142-11: word "paramaters" is misspelled.
2142-13: change "contrasted" to "the contrasting"
2142-15: strike "or"
2142-19: depluralize "parameters"
2142-22: strike "obtained"
Sentence starting on 2142-28: Insert "However" at the beginning and strike however later in the sentence.
2143-2: remove double negative to read "our approach yielded realistic results."
2143-11: change "precise and accurate" to in-depth.
2143-23: change "able to fit" to "capable of fitting"
In table 4, remove the equations describing each model. These are already presented in the text and in table 3.

We performed all of these corrections.