Dr. Antoine Rabatel
LGGE
54 rue Molière, Domaine Universitaire
38400 Saint Martin d'Hères, FRANCE
Tel: +33 4 76 82 42 71
Fax: +33 4 76 82 42 01
@: rabatel@lgge.obs.ujf-grenoble.fr

02 August 2013

Authors response to reviewers.

Paper title: *Changes in glacier equilibrium-line altitude (ELA) in the western Alps over the 1984-2010 period: evaluation by remote sensing and modeling of the morpho-topographic and climate controls*
Authors: A. Rabatel, A. Letréguilly, J.-P. Dedieu and N. Eckert
MS No.: doi:10.5194/tcd-7-2247-2013

Dear Handling Editor: Dr. Gudmundsson, dear reviewers: Dr Zemp and anonymous,

We deeply thank you for your careful reading and the encouraging general comments provided on the paper.

The remarks and technical comments have been considered. Thanks to these comments, we think that the manuscript has been improved and we hope that it now satisfies the standards for a publication in *The Cryosphere*.

Hereafter, you will find a detailed description of the authors' replies to each comment of the reviewers (the text of the paper appears in blue and the changes are underlined).

Enclosed, in a pdf file, you will find an updated draft of the manuscript showing the changes that have been made (changes are highlighted in yellow).

Best regards,

Dr. Antoine Rabatel, for all the authors.

Response to reviewer #1

Reviewer #1, comment #1:

The paper employs an analytical perspective that will probably be unfamiliar to most glaciologists.

Authors reply:

By "unfamiliar analytical perspective", you probably refer to the Lliboutry's "multivariate statistical analysis" approach published in *Journal of Glaciology* in 1974. This method, developped to extract the spatial and temporal terms in mass balance measurements time series, is however very interesting as it allows to understand how much of the mass balance of a glacier is related to the characteristics of the glacier itself (its morpho-topographic variables), and how much is related to a common signal depending of the climate conditions.

Even if this method has been then applied successively in the whole Alps by Reynaud (JoG, 1980) and at the northern hemisphere scale by Létreguilly and Reynaud (AAR, 1990), and more recently in an expanded way on Sarennes Glacier by Eckert *et al.* (JoG, 2011) and Thibert *et al.* (TC, 2013), it has remained relatively unkown. All these references are given in the paper for helping the reader to go further in the knowledge of this method.

The method is used here for the first time on ELA time series, and we hope that many readers will be interested by this kind of approach and the presented results.

Reviewer #1, comment #2:

The paper uses factor analysis, which occurs only seldom in the glaciological literature, for instance, but devotes not a word to describing it.

Authors reply:

A description of this statistical method has been added at the beginning of the section 3.1.2 "Spatial variability of the ELA", when we introduce the Fig. 6 showing the results of such an analysis. You can read: "The factor analysis being a statistical method allowing to describe the variability, differences or similitudes among observed and correlated variables according to a lower number of factors, i.e. unobserved variables, for instance the space and time. For the ELA time series, ...".

Reviewer #1, comment #3:

The paper does not reflect a broad awareness of the glaciological literature dealing with analysis of ELA variation, such as Braithwaite, Raper (2009, Ann. Glac. 50(53) 127).

Authors reply:

This paper is indeed interesting and brings arguments that corroborate our findings about the relationship between the ELA and the average altitude of the glaciers.

Consequently, we refer to this paper in the discussion (section 4.1 The role of morpho-topographic variables) in the following way: "[...] the average surface area of a glacier as well as its average altitude can be deemed to be representative of its average ELA. This is in good agreement with the results presented by Braithwaite and Raper (2009) showing on the basis of 94 glaciers that the glacier area-median and mid-range altitudes are accurate proxys of the ELA for a period of time, e.g. several decades."

Other references have also been added to answer one comment of reviewer #2.

Reviewer #1, comment #4:

2248,5 A quantity that varies in space or time is a variable, not a parameter. This indicates a change that is needed in numerous other places. Especially objectionable are climate parameter and meteorological parameter.

Authors reply:

The term "parameter" has been changed by the term "variable" everywhere it is needed.

Reviewer #1, comment #5:

2251,6 glacierized is needed here and in many other places instead of glaciated.

Authors reply:

The term "glaciated" has been changed by the term "glacierized" everywhere it is needed.

Reviewer #1, comment #6:

2252,24 Presumably the point here is that the 20-m difference is small compared with variation of the ELA.

Authors reply:

Indeed, this is a supplementary argument. A sentence has been added in agreement with your comment: "This matches the vertical accuracy of the DEMs, and is small in comparison to the interannual variability of the SLA (the standard deviation of the measured SLA over the whole study period ranges between 75 and 255 m depending on the glacier)".

Reviewer #1, comment #7:

2253,24 αi would be better called the long-term mean over the period of record. It seems that αi and βt should be defined right after (1), and that εit = αi + βt - SLAit, where αi and βt are formed as stated and SLAit is the observed SLA.

Authors reply:

Your remark has been considered, following equation 1, you can now read: "Where SLA_{it} is the snow line altitude of the glacier i for the the year t, α_i is long-term mean for each glacier over the period of record, β_t is a term depending on the year only which is common to all the glaciers analyzed, and $\varepsilon_{it} = SLA_{it} - \alpha_i - \beta_t$, with $\varepsilon_{it} \sim N(0,\sigma^2_\varepsilon)$ a table of centered residuals assumed to be independent and Gaussian (N denotes the normal distribution)."

Reviewer #1, comment #8:

2254,19 Presumably standard deviation of the measured SLAs is meant, not their RMSE.

Authors reply:

Indeed, this has been corrected.

Reviewer #1, comment #9:

2254,23 The remarkable fact that records from each of 21 weather stations are used for exactly two glaciers (Table 1) and from one other station for one glacier is worthy of spelling out in the text or in the caption of the table.

Authors reply:

We are sorry, but we do not understand this comment. Indeed, the 22 weather stations have been used all together in the analysis and interpretation of all the 43 ELA time-series.

Reviewer #1, comment #10:

2255,6 Presumably two stations in Switzerland were used in addition to the 20 mentioned to get the 22 listed in Table 1. If so, the paper should be more specific by saying two stations in Switzerland instead of additional stations in Switzerland.

Authors reply:

This has been changed according to your comment.

Reviewer #1, comment #11:

2256,3 That mass balance correlates well with the SLA does not mean the SLA is the ELA. If glacier-wide mass balance correlates well with mass balance at a point near the middle of the altitude range of the glacier, which is the case for most mid-latitude glaciers, the mass balance at that point is not the ELA and that altitude is not necessarily the ELA. Moreover, the SLA and ELA coincide only in the absence of superimposed ice.

Authors reply:

The header of the section "3.1 Changes in ELA" has been modified to answer your comment. We now rely on our previous works (Rabatel et al., JoG, 2005 and 2008) where we have shown that the end-of-summer snowline measured on satellite images is highly correlated with the ELA measured from ground measurements, and can consequently be considered as an accurate proxy of the ELA. The Figure 3 of the draft you have read has been removed in the new version as it became useless. You can now read: "As mentioned above, it has been demonstrated that on mid-latitude glaciers where superimposed ice is negligible, the end-of-summer SLA is an accurate indicator of the ELA (see Figure 6 in Rabatel et al., 2005, and Figure 3B in Rabatel et al., 2008). Because of this strong correlation and for the sake of simplicity, we hereafter only use the term ELA."

Reviewer #1, comment #12:

2258,1 The two effects of higher temperature in the south should be distinguished in the text. How much is due to enhanced ablation and how much might be due to shifting of precipitation from snow to rain?

Authors reply:

We only consider in this study the effect on higher temperature in the southern sector of the French Alps in increasing the quantity of CPDD during the summer period, which is a good proxy of ablation. The effect on precipitation change from snow to rain is not considered because in our analysis we do not consider the precipitation during the ablation season. This has been specified in the text as follows: "which could be associated with differences in winter accumulation between the southern and northern Alps (see below) and warmer summer temperature in the southern sector which would increase the amount of CPDD and thus the ablation (note that the effect of higher summer temperature on a shift of precipitation from snow to rain is not considered in this study because we do not use summer precipitation in our analysis)."

Reviewer #1, comment #13:

2258,7 It is an increasing trend but it is not a linear trend.

Authors reply:

You are right, the increasing trend in not linear. However, the rate of increase mentioned in the text is given assuming a linear trend. The sentence has been rewritten as follows: "Summer CPDD present an increasing trend, averaging 5.3±1.9 CPDD/yr at 3,000 m a.s.l., assuming a linear trend over the period 1984-2010."

Reviewer #1, comment #14:

2258,12 Sublimation should be mentioned, only if to say that all ablation is assumed to be due to melting.

Authors reply:

This has been done. You can now read: "assuming that all of this energy is used to melt the snow, and so that sublimation is negligible."

Reviewer #1, comment #15:

2258,16 It would be appropriate here to cite Beniston (2004, GRL 31) or Beniston, Diaz (2004, Glob. Planet. Change 44)

Authors reply:

The reference to Beniston and Diaz paper published in *Global and Planetary Change* has been added.

Reviewer #1, comment #16:

2258,24 Standard deviation after 2001 appears to be larger than half of before 2001.

Authors reply:

No it is not. Standard deviation for the 1984-2001 period is 140 mm and after 2001, it falls to 62 mm. These values have been added in the text: "However, one can note that, on average, interannual variability was lower after 2001 (standard deviation divided by 2 after 2001, falling from 140 mm to 62 mm)..."

Reviewer #1, comment #17:

2260,6 How the average altitude is calculated should be explained. Is it the width-weighted average or is it just half the sum of the highest and lowest altitudes?

Authors reply:

You are right. The way we computed the average altitude of each glacier has been added in the section "2.2.2. DEM, computation of the altitude of the snowline and of the glacier morpho-topographic variables" as follows: "The mean elevation of each glacier has been computed as the arithmetic mean of the elevation of each pixel of the DEM included within the glaciers outline. This mean elevation is rather close to the median elevation of the glaciers. Indeed, the difference between the tow variables is 10 m in average for the 43 studied glaciers. This shows that the studied glaciers have in average, an almost symmetrical area-altitude distribution (Braithwaite and Raper, 2010)."

Reviewer #1, comment #18:

2260,14 The sentence needs to be recast for it implies that the ELA is the independent variable upon which the geometric properties depend.

Authors reply:

The sentence has been rewritten according to your remark as follows: "Indeed, the ELA constitutes the lower limit of the accumulation zone, which represents ~2/3 of the total glacier surface area in a steady state glacier. Accordingly, the wider the accumulation zone, the bigger the glacier, the lower its snout, the lower the mean altitude and consequently the lower the ELA of the glacier."

Reviewer #1, comment #19:

2260,24 What is meant by meridional effect should be explained.

Authors reply:

The "meridional effect" is characterized few lines earlier: P. 2260, L. 2 to 4 in the version you have read as follows: "This meridional effect is consistent with the drier and warmer conditions associated with the Mediterranean climate that prevails in the southern part of the study area". Consequently, from our point of view, there is no need to explain it again.

Reviewer #1, comment #20:

2262,10 Fig. 4c shows winter P.

Authors reply:

You are right, this has been corrected, this figure is now numbered 3C.

Reviewer #1, comment #21:

2262,24 How the seasons are defined should be stated, particularly if it is meant that the entire year is partitioned into just winter and summer. That is, there are no transitional seasons during which both accumulation and ablation might occur (as they might also occur, in vastly unequal amounts, in both winter and summer.)

Authors reply:

The two climate variables we used are defined in the section "2.3. Meteorological data" as follows: "The climate variables used in the analysis were (1) cumulative positive degree days (CPDD) from May 15 to September 15 for each year t, extrapolated to the altitude of 3,000 m a.s.l. using a standard gradient of 6 °C/km (3,000 m a.s.l. being the approximate mean elevation of the SLA of the 43 glaciers studied over the whole study period); and (2) cumulated winter precipitation from September 15 of the year t^{-1} to May 15 of the year t."

About winter precipitation we also mention that: "During this period [September 15 of the year t^{-1} to May 15 of the year t], liquid precipitation is negligible at 3,000 m a.s.l."

Furthermore, we do not consider the seasons from a climate point of view, but the two important periods for glacier mass balance: the accumulation period and the ablation period, which are fortunately quite well temporally defined for mid-latitude alpine glaciers. Obviously, ablation may still occur on the lower reaches of the glaciers after September 15, but we consider in the current study, the CPDD at 3000 m a.s.l. (at the level of the ELA) where Thibert et al. (2013) have shown that in our study area, after September 15, CPDD are negligible. This point is also mentioned in the 2.3. section.

Reviewer #1, comment #22:

2263,4 Units of CPDD are °Cd, not °C. Here 115m°C^{-1} is said to be sensitivity to CPDD, whereas at 2263,11 it is correctly called sensitivity to summer temperature.

The Fig. 9 caption also ascribes the wrong units to CPDD.

Authors reply:

You are right. The text has been modified according to your remark: "These two graphs show that the sensitivity of the ELA to summer temperature was 115 m/°C, ..."

The caption of Fig. 9 has also been modified according to your remark, with the CPDD units writing as °Cd.

Reviewer #1, comment #23:

2264,6 Why spatial and temporal variables are called covariates should be explained.

Authors reply:

We changed the term "covariates" by the synonymous expression "explanatory variables", because the latter one is more understandable.

Reviewer #1, comment #24:

2264,16 How standardized variables are reduced should be explained.

Authors reply:

This has been done in the following way: " [...] the model is fed with reduced standardized variables (by dividing by the standard deviation the difference between each value and the average of the series)."

Reviewer #1, comment #25:

2265,5 poorly informative priors is an example of statistical jargon in the paper that few glaciologists will comprehend.

Authors reply:

Bayes's theorem (Bayes, 1763) allows combining the observations and a *prior* distribution which encodes extra data about the unknowns (parameters and latent variables), leading the joint *posterior* distribution of all parameters and latent variables. Poorly informative *priors* for all parameters is a standard choice to obtain posterior estimates only driven by information conveyed by the data. The term priors is a simplification of prior probability distributions.

We changed the text by using the complete expression because it may be more understandable and added an explanation about this choice which is common in this kind of studies. You can now read: "Poorly informative *prior* probability distributions were used for all parameters, this standard choice allows to obtain *posterior* estimates only driven by information conveyed by the data."

Bayes T. (1763). Essay Towards Solving a Problem in the Doctrine of Chances. Philosophical Transactions of the Royal Society of London. 53, 370-418 and 54, 296-325.

Reviewer #1, comment #26:

2266,6 It is not clear why Eqn (15) is denoted $R^2_{time/space}$ when its numerator contains only the temporal term.

Authors reply:

This equation is the ratio between the temporal variability and the sum of the spatial and temporal variabilities (i.e. everything except the random fluctuations). It allows to compare the respective weight of the separable temporal and spatial effects. Maybe another notation could be preferred but is just a notation.

Reviewer #1, comment #27:

2266,7 The notation would be more compact were the denominators of Eqns (15,16) written as VAR(ELAit) - σ^2.

Authors reply:

It has been changed according to your remark.

Reviewer #1, comment #28:

2266,15 lower level should be defined.

Authors reply:

By lower level, we mean at the observation level of each year/glacier. The sentence has been reformulated in a sake of clarity: "At the observation level of each year/glacier of the model, local and annual adjustment statistics can also be computed as ...".

Reviewer #1, comment #29:

2267,2 If $\varepsilon_{it} \sim N(0; \sigma^2_\varepsilon)$ (as stated at 2253,25) would not $\Sigma\varepsilon_{it}$ be very small? The disparate expressions for σ_t in Eqns (17 and 19) are worthy of comment, as also are the two for σ_i in Eqns (18 and 20).

Authors reply:

Even if all residuals are taken as Gaussian with zero mean and if the model works well (unbiased model), this does not mean that it is true also at the "lower" level of each year/glacier. And it is indeed the role of Eqns. 17-20 to check this point in terms of mean difference and mean square error. The idea is to determine if the model is similarly good everywhere/all the time, and, if not, for which year/glacier it is the most/less efficient, and possibly try to investigate why.

Reviewer #1, comment #30:

2267,11 What is the MCMC sequence?

Authors reply:

MCMC is defined earlier in the text (P. 2265, L. 1-2 in the version you have read). The term "sequence" has been replaced by the expression "iterative simulation run" in a sake of clarity.

Reviewer #1, comment #31:

2267,16 The first five mean R^2 values in Table 4 do add to 1.00, so the statement about being very close but not fully equal to 1 should be made quantitative. Maybe to more decimal places they add to something such as 0.997.

Authors reply:

Five decimal are needed to find 0.99995. As a consequence, this sentence was removed.

Reviewer #1, comment #32:

2270,23 How the 170-m increase was determined is unclear. Was it the difference between the 43-glacier mean ELA in 2010 and the 43-glacier mean ELA in 1984? If so, it is irrelevant, although interesting, that the 26-year variation of the mean is nearly linear

Authors reply:

You refer here to the "Conclusions" section. The way the 170-m increase is determined is specified in the section "3.1.1. Temporal variability of the ELA" (P. 2256, L. 11 to 15 in the version you have read), where we mention: "over the study period, the ELA time series showed an average increasing trend of 6.4 m/yr, assuming a linear trend which results to be statistically significant considering a risk of error of 5%. This is the equivalent of an average increase of 170 m over the 1984-2010 period, i.e. higher than the interannual variability of the average ELA."

As a consequence, this increase is not the difference between the 43-glacier mean ELA in 2010 and in 1984, but it results from a linear regression over the whole period which results to be statistically significant considering a risk of error of 5%.

Reviewer #1, comment #33:

2271,23 The full variability might be well approximated but the full variability is not reproduced.

Authors reply:

The term "reproduce" has been changed by "well approximate".

Reviewer #1, comment #34:

2277 Fig. 1 would be better were it to show locations of all 22 weather stations, not just 14 of them. An alternative would be to give latitude and longitude in Table 1 for all stations and all glaciers.

Authors reply:

According to your comment we added the coordinates (latitude, longitude, altitude) of all weather stations in Table 1. Because all the glaciers are shown on the map, we think that it is not necessary to give their coordinates in Table 1.

Reviewer #1, comment #35:

2284 The meaning of spline regression should be explained. Apparently, smoothing splines are fit to the 27 values in each panel, but nothing is said about the criteria of the fits, such as weights or boundary conditions or the polynomial degree or the order of continuity. A reference should be supplied.

Authors reply:

In the context of climate reconstruction from proxies, splines have been widely used to model a smooth temporal signal (e.g., Hilasvuori et al., 2009). For coherence with the spatio-temporal framework used later in the paper, we used here the Bayesian approach of Wahba (1978) and Speckman and Sun (2003). They proposed an *a priori* distribution for the vector of latent temporal variables such that its Bayesian estimate is a cubic smoothing spline and showed that such a *prior* can be written as an intrinsic autoregressive model, with improper probability density function, making inference easy. Details of the computations and application in a close field can be found in Lavigne et al. (2012).

We had the necessary explanation and reference in the caption according to your remark. You can now read: "On each graph, the dashed line represents the smooth underlying trend captured by a cubic smoothing spline regression (Lavigne et al., 2012)."

Hilasvuori, E., F. Berninger, E. Sonninen, H. Tuomenvirta, and H. Jungner. Stability of climate signal in carbon and oxygen isotope records and ring width from Scots pine (Pinus sylvestris l.) in Finland. *Journal of Quaternary Science*, 24(5), 469-480, 2009.

Lavigne, A., L. Bel, E. Parent, and N. Eckert. A model for spatio-temporal clustering using multinomial probit regression: application to avalanche counts in the French Alps. *Envirometrics*, 23, 522–534, 2012.

Speckman, P.L., and D. Sun. Fully Bayesian spline smoothing and intrinsic autoregressive priors. Biometrika, 90(2),289-302, 2003.

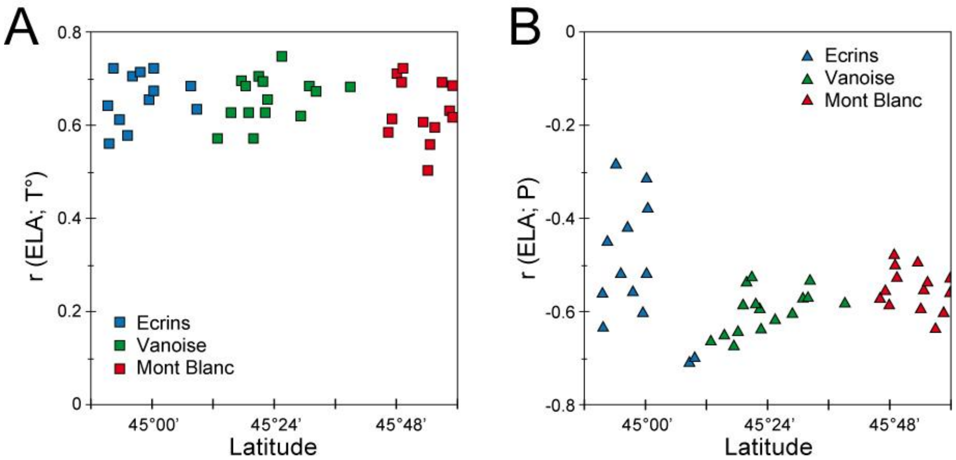
Wahba, G. Improper priors, spline smoothing and the problem of guarding against model errors in regression. *Journal of the Royal Statistical Society*, Series B (Methodological), 364-372, 1978.

Reviewer #1, comment #36:

2288 Writing r(ELA; T) and r(ELA; P) follows conventional notation for correlation between two variables, whereas ELA-T and ELA-P denote differences.

Authors reply:

This has been changed as recomended. Here is the new version of the figure.



Response to reviewer #2: Dr. M. Zemp

Reviewer #2, comment #1:

Title: shorten, e.g. to: "Changes in glacier equilibrium-line altitude in the western Alps from 1984-2010: evaluation by remote sensing and modelling of morpho-topographic and climatic controls."

Authors reply:

The title has been shortened according to your remark.

Reviewer #2, comment #2:

Page 2249, Line 8, update: "..on around 260 glaciers worldwide…"

Authors reply:

The number of monitored glaciers worldwide has been updated according to your remark and the last WGMS report you metion in the next comment.

Reviewer #2, comment #3:

Page 2249, Line 11, update: WGMS (2012)

Authors reply:

This reference has been updated.

Reviewer #2, comment #4:

Page 2250, Lines 1-12: The paragraph following "Here, we…" shall provide the aims of the study. Further detail your aims (Lines 1-3) and move the other text bits (i.e. Lines 4-12) to the introductory paragraphs above.

Authors reply:

The last part of the "Introduction" section has been re-organized and rewritten to answer you comment. You can now read: "*[...]. On the other hand, for mid-latitude mountain glaciers, the end-of-summer snowline altitude (SLA) is a good indicator of the ELA and thus of the annual mass balance (Lliboutry, 1965; Braithwaite, 1984; Rabatel et al., 2005). This enables ELA changes to be reconstructed for long time periods from remote-sensing data (Demuth and Pietroniro, 1999; Rabatel et al., 2002, 2005, 2008; Barcaza et al., 2009; Mathieu et al., 2009), because the snowline is generally easy to identify using aerial photographs and satellite images (Meier, 1980; Rees, 2005). Consequently, it is possible to study the climate-glacier relationship at a massif or regional scale (Clare et al., 2002; Chinn et al., 2005), which is particularly useful in remote areas where no direct measurements are available.*

In the current study, we rely on previous studies conducted in the French Alps (Dedieu and Reynaud, 1990; Rabatel et al., 2002, 2005, 2008), to reconstruct ELA time series for more than 40 glaciers over the 1984-2010 period, using the end-of-summer snowline detected on satellite images. Our aim are: (1) to quantify at a regional scale the temporal and spatial changes of the ELA; (2) to characterize the relationships between ELA and both morpho-topographic and climate variables; and (3) to reconstruct the spatio-temporal variability of annual ELA time series by incorporating the above mentioned relationships in an expansion of Lliboutry's variance decomposition model (1974).

Reviewer #2, comment #5:

Page 2250, Lines 13-22: delete. There is no need for a description of each section in a short paper.

Authors reply:

The last part of the "Introduction" section has been rewritten in agreement with the previous comment (#4), see our response to comment #4. As a consequence, the paragraph describing the content of each section at the end of the "Introduction" has been deleted, as recommended by the reviewer.

Reviewer #2, comment #6:

Page 2251, Lines 4-5; relates also to Page 2256, Lines 10-11 and Fig. 4: The first selection criterion (i.e. only glaciers with high enough maximum elevation) might lead to a bias in your sample. Many glaciers experienced ELAs above their maximum elevation in the past two decades, with extreme values in 2003 (Zemp et al. 2005, DGS; Schär et al. 2004, Nature). Add a corresponding section to the discussion: Is this a limitation of your approach? Is there a workaround?

Authors reply:

Our first criterion makes that small glaciers of low altitude and without permanent ELA are indeed excluded from our sample. From our point of view, this is not a limitation of the approach. Such small glaciers of low altitude are completely imbalanced under current climate conditions and are supposed to disappear more or less rapidly if current climate conditions maintain, and even faster if the current trend toward higher temperatures follows. At the end of the ablation season these glaciers are only an ablation zone, the ELA is more or less far above the upper reaches of these glaciers and is consequently only theoretical. Furthermore, ablation on these small glaciers of low altitude is generally increased by local conditions (border effects) which can predominate on the regional climate signal driving most of the mass balance variability under "normal" conditions.

Finally, one of the purpose of the paper is to quantity ELA changes from remote sensing using the snowline altitude as a proxy of the ELA. If the snowline cannot be seen, the method is just not applicable. Another point is to quantify the respective controls of the morpho-topographical and climate variables on the variability of the ELA. If a small glacier does not have any ELA anymore due to its low altitude, this quantification is just useless

Reviewer #2, comment #7:

Page 2251, Line 10: delete the URL here but keep it in the Acknowledgements.

Authors reply:

It has been done according to your remark.

Reviewer #2, comment #8:

Page 2251, Line 18: avoid acronyms in title.

Authors reply:

It has been done according to your remark: “SLA” has be replaced by “Snowline altitude”.

Reviewer #2, comment #9:

Page 2252, Line 7: avoid acronyms in title.

Authors reply:

It has been done according to your remark: “DEM” has be replaced by “Digital elevation model”.
We also replaced all the acronyms in the other titles.

Reviewer #2, comment #10:

Page 2252, Lines 14-16: You investigate the classical selection of simple morphotopographic parameters which is OK. What about testing some ‘new’ ones which allow considering the sensitivity to hypsometry (e.g., above/below median or mean elevation area ratio)?!

Authors reply:

One of our goal is to identify how much of the variance of the ELA time series is explained by the morpho-topographic variables. Consequently, we indeed choose “classical” morpho-topographic variables, commonly used in several glaciological studies based on inventory data. We tested a wide range of variables (slope, lenght, surface area, compacity index, minimum elevation, mean elevation, modal elevation, median elevation, latitude, aspect) and kept the ones with the higher significant correlations. A new index like an area ratio related to the mean or median elevation could be interesting to test, however, such an index is correlated to the mean elevation and the surface-area of the glacier (two variables we used). As a consequence, it should not explain a bigger part of the ELA variance than the variables we used.

Reviewer #2, comment #11:

Page 2252, Lines 17-24: The study by Paul and Haeberli (2008; GRL) showed that elevation differences can be >100m in the ablation zones of Swiss glaciers between 1980s and 2000s. Your approach might, hence, introduce a bias in years of low SLAs. Check and add a corresponding remark here or add a section in the Discussion.

Authors reply:

You are completely right evocating that point. We forgot to mention in the draft you have read that to avoid this problem, we used the French IGN DEM to compute the altitude of the snowline for the first part of the period, and the ASTERGDEM for the second part of the period. This has been corrected by adding the following paragraph to the section "2.2.2. Digital elevation model, computation of the altitude of the snowline and of the glacier morpho-topographic variables": "*However, because the glacier surface lowering can reach several tens of meters at lower elevation (up to 60 m at 1,500 m a.s.l.) due to the important glacier shrinkage over the last decades (Paul and Haeberli, 2008), when the SLA is at lower elevation as it was the case in 2001 (2,839±129 m a.s.l.) it is better to use a DEM as close as possible as the date of the used images. Accordingly, the DEM from the French IGN was used to the first half of the period (till the late 1990s) and the ASTERGDEM was used for the second part of the period. Changing from one DEM to the other in the late 1990s has no impact on the results because at that time, the SLA was located between 3,000 and 3,200 m a.s.l., an altitudinal range for which the difference in elevation between the DEMs is lower than their vertical accuracies*."

Paul, F., and Haeberli, W.: Spatial variability of glacier elevation changes in the Swiss Alps obtained from two digital elevation models, Geophys. Res. Lett., 35, L21502, doi:10.1029/ 2008GL034718, 2008.

Reviewer #2, comment #12:

Page 2255, Lines 22-26, Page 2256 Lines 1-3: Setting SLA equal to ELA is a major assumption and certainly not of general validity. Hence, I recommend (i) adding ELA values to Fig. 3 or adding a figure showing SLA versus ELA (maybe as an inset) for the tree glaciers with mass balance measurements and (ii) not using the term ELA for SLA.

Authors reply:

We are not talking about any SLA (i.e. the transient snowline during the summer period), but about the end-of-summer snowline altitude on mid-latitude temperate mountain glaciers for which several studies have pointed (Lliboutry, 1965; Braithwaite, 1984) and then demonstrated (Rabatel et al., 2005; 2008) that it is highly correlated to the ELA, and consequently is a very good indicator of the ELA. The main reason is that the superimposed ice is absent or negligible on these glaciers. The above refered studies are quoted in the "Introduction" section.

Furthermore, we also mention that the present study relies on the previous ones we have performed in the French Alps (Dedieu and Reynaud, 1990; Rabatel et al., 2002; 2005 and 2008), where the relationship between the end-of-summer SLA and the ELA has been demonstrated on the basis of snowlines measured on satellite images and ELA computed from field measurements on monitored glaciers.

Anyway, because your comment about the Figure 3 of the draft you have read, is close to the comment #11 of reviewer 1, the Figure 3 has been removed in the new version as it became useless. The first paragraph of the section "3.1. Changes in ELA" has also been rewritten in a sake of clarity. You can now read: "As mentioned above, it has been demonstrated that on mid-latitude glaciers where superimposed ice is negligible, the end-of-summer SLA is an accurate indicator of the ELA (see Figure 6 in Rabatel et al., 2005, and Figure 3B in Rabatel et al., 2008). Because of this strong correlation and for the sake of simplicity, we hereafter only use the term ELA."

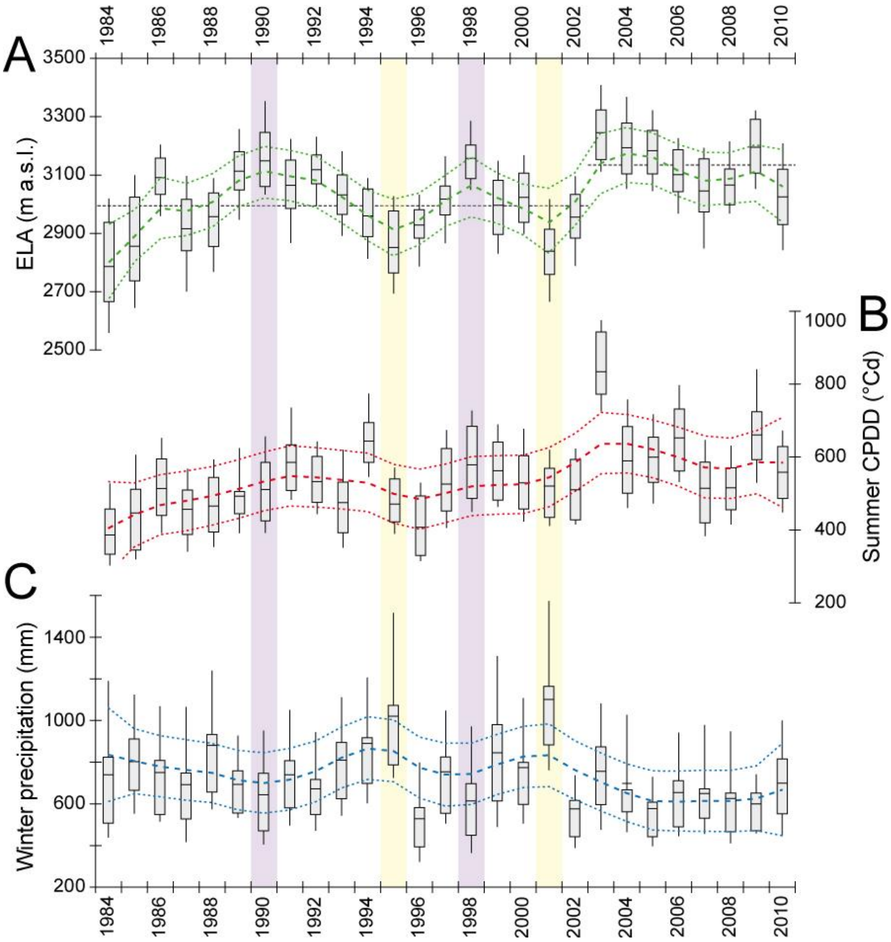
Reviewer #2, comment #13:

Page 2256, Lines 20-27, Fig. 5: I agree that 2003 might mark a breakpoint in the time series. The extreme heat wave resulted in a reduction, or even complete loss, of the firn area and, hence, introduced a positive feedback on glacier mass balances after 2003. This issue might be further detailed in the discussion section. Optionally, you might show average SLAs before and after 2003 in Figure 5.

Authors reply:

In agreement with the reviewer comment, we added a few sentences to present this issue. However, we did so in the same 3.1.1 section "Temporal variability of the ELA", as it appears more consistent with the rest of the text. You can now read : "*It should be noted that the 2003 extreme heat wave resulted in a reduction, or even complete loss on some glaciers, of the firn area, hence introduced a positive feedback on glacier mass balances after 2003, consistent with higher ELAs*".

We assume that the reviewer is talking about Figure 4 (not Figure 5) when he asked to show the average SLAs before and after 2003. In the new version of the draft this figure is now #3. As recomended by the reviewer, we added a dashed horizontal line showing the average SLAs after and before 2003. See below in the upper graph (A).



Reviewer #2, comment #14:

Page 2258, Lines 14-15: "…about 300 m higher than the average for the whole period (in fact above many glacierized summits), …"

Authors reply:

The precision mentioned by the reviewer has been added to the text

Reviewer #2, comment #15:

Page 2259, Lines 14-19: delete.

Authors reply:

This paragraph is a kind of header paragraph of the "Discussion" section. We did the same for the "Result" section.

From our point of view, it is interesting to help the reader understanding how the section to follow is organized and structured. We did so in several papers, and we recognize that it is not a necessity, but we think that the paper becomes easier to read.

Reviewer #2, comment #16:

Page 2260, Lines 15-18: I do not agree that glacier size of 2010 represents the average ELA of the period 1984-2010 – most Alpine glaciers are too large for current climate (cf. Mernild et al. 2013, TCD).

Authors reply:

We do not say that glacier size of 2010 represents the average ELA of the period 1984-2010, but that "*over a long period of time (almost 30 years in our case), assuming pseudo-stationary conditions over this period, the average surface area of a glacier as well as its average altitude can be deemed to be representative of its average ELA.*"

Furthermore, this point is in good agreement with the study of Braithwaite and Raper published in Annals of Glaciology in 2010. This point was raised by the reviewer 1 (see his comment #3) and that is why we added the following sentence: "*This is in good agreement with the results presented by Braithwaite and Raper (2010) showing, on the basis of 94 glaciers, that the glacier area-median and mid-range altitudes are accurate proxies of the ELA for a period of time, e.g. several decades*."

Braithwaite, R.J., and Raper, S.C.B.: Estimating equilibrium-line altitude (ELA) from glacier inventory data, A. Glaciol., 50(53), 127-132, doi: 10.3189/172756410790595930, 2010.

Reviewer #2, comment #17:

Page 2264, Lines1-19: It might be interesting to compare your results to those from more other studies, e.g. by Kuhn (1981, IAHS), Braithwaite and Zhang (2000, JG), Oerlemans (2001, Balkema Publishers), Zemp et al. (2007, GPC). Maybe add an additional table.

Authors reply:

The text was indeed a bit confusing as it gave the impression that we compared our results only with French references: Vincent, 2002 and Gerbaux et al., 2005, when we actually looked at other references which were not mentioned in the text because they are within the range of the presented "extreme" sensitivity values given by Vincent and Gerbaux et al.

We added an additionnal sentence to mention other sensitivity values published in the literature. You can now read: "*Our estimate of ELA sensitivity to summer temperature, 115 m°C^{-1}, is in the middle range of values reported in the literature which range from 60–70 m°C^{-1} (Vincent, 2002) to 160 m°C^{-1} (Gerbaux et al., 2005). Other values from the European Alps and other mid-latitude regions that can be found in the literature are: 90-115 m°C^{-1} (Braithwaite and Zhang, 2000), 100 m°C^{-1} (Zemp et al., 2007), 125 m°C^{-1} (Kuhn, 1981) or 130 m°C^{-1} (Oerlemans and Hoogendoorn 1989)*."

Braithwaite, R.J., and Zhang, Y.: Sensitivity of mass balances of five Swiss glaciers to temperature changes assessed by tuning a degreeday model, J. Glaciol., 46(152), 7-14, 2000.
Kuhn, M.: Climate and glaciers, IAHS, 131, 3–20, 1981.
Oerlemans, J. and Hoogendoorn, N. C.: Mass-balance gradients and climate change, J. Glaciol., 35, 399-405, 1989.
Zemp, M., Hoelzle, M., and Haeberli, W.: Distributed modelling of the regional climatic equilibrium line altitude of glaciers in the European Alps, Glob. Planet. Change, 56, 83-100, doi: 10.1016/j.gloplacha.2006.07.002, 2007.

Reviewer #2, comment #18:

Pages 2263-2270, Section 4.3: this section is way too long and combines methods, results and discussion. Split and distribute the text to the corresponding sections. Also, better motivate/emphasize the benefit of this additional statistical exercise.

Authors reply:

You are right. The first part of this section has been moved to the "Data and methods" section in a new subsection "*2.4. Modeling of the control of the morpho-topographic and climate variables*", where the motivation of this approach has been better highlighted (see the new version of the paper). The rest of the original section has been conserved in the "Discussion" section (in a subsection called "*4.3. About efficiency of our modeling approach to reconstruct equilibrium-line altitude time series from morpho-topographic and climate variables*") because the results of this statistical exercise directly feed the discussion about the respective control of the different parameters and the capacity of the model to be used to reconstruct SLA time series from morpho-topographical and climate variables.

Reviewer #2, comment #19:

Pages 2272-2276: The reference list is slightly francophone. It might be worth supporting and discussion your findings in view of other Alpine studies.

Authors reply:

We added several references thanks to your comments and the ones of reviewer 1. References added appear highlighted in yellow in the text and the "References" section of the new draft.

Reviewer #2, comment #20:

Page 2275, Line 30, replace WGMS (2011) by WGMS (2012): WGMS: Fluctuations of Glaciers 2005–2010, Volume X, edited by: Zemp, M., Frey, H., Gärtner-Roer, I., Nussbaumer, S.U., Hoelzle, M., Paul, F., and Haeberli, W., ICSU(WDS)/IUGG(IACS)/UNEP/UNESCO/WMO, World Glacier Monitoring Service, Zurich, Switzerland, 336 pp., publication based on database version: doi:10.5904/wgms-fog-2012-11, 2012.

Authors reply:

The reference has been updated.

Reviewer #2, comment #21:

Page 2279, Table 2: You may delete this Table and add the key information to the text.

Authors reply:

You probaly refer to the page 2278 in the draft you have read if you are talking about Table 2, which present the characteristics of the wavelength of the spectral bands used on the satellite images of the three sensors (Landsat, SPOT and Aster). We prefer to keep this small table as it is because the information shown here is better presented in a Table than written in the text. Furthermore, we think that adding this information to the text would unnecessarily overload the reading.

Reviewer #2, comment #22:

Page 2282, Fig. 2: This figure nicely shows some important issues of the SLA selection which might be worth to be discussion in more detail: somewhat arbitrary definition of the glacier system/catchment and selection of the snow line for SLA calculation. How different are the manually derived SLA (including expert knowledge) from those derived from automatic approaches (e.g., Huss et al. 2013, AG)?

Authors reply:

You are right, our description of the methodology used to delineate the snowline was very short and precisions were missing. We added a new paragraph to answer your comment and provide more information to the reader: "*The snowline was delineated on the central part of the glaciers to avoid border effects on the glacier banks (shadows from surrounding slopes, additional snow input by avalanches, over-accumulation due to wind) which could generate equilibrium-line position dependence on local conditions (Rabatel et al., 2005). The delineation has been performed manually because automatic method hardly succeed in distinguishing the snowline from the firn-line when both are observed on the glacier. A distinction which results to be also difficult visually when the pixel size of the satellite images is too coarse (see the discussion section)*.

Reviewer #2, comment #23:

Page 2283, Fig. 3: Add ELA values. See comment above relating to Page 2255.

Authors reply:

This figure has been removed from the new version of the paper. See also our answer to your comment #13 and to the comment #11 of reviewer 1

Reviewer #2, comment #24:

Page 2284, Fig. 4: The caption is hard to understand without reading the text section. It might help to indicate where to find the corresponding information.

Authors reply:

The caption of Figure 4 (Figure 3 in the new version of the draft) has been modified according to your comment and comment #35 of reviewer 1. You can now read: "***Figure 3:** Changes over the 1984-2010 period in **A)** the ELA for the 43 glaciers studied (see Fig. 1 and Table 1); **B)** summer CPDD; and **C)** winter precipitation recorded by the weather stations used (see Fig. 1 and Table 1). In each graph, the horizontal black bar represents the annual average of the sample, the gray box represents the median interval (Q3-Q1), and the vertical black lines show the interval between the first and the last decile (D1 and D9). On each graph (described in sections 3.1.1. and 3.2.), the dashed line represents the smooth underlying trend captured by a cubic smoothing spline regression (Lavigne et al., 2012). The dotted line is the corresponding 95% credible interval. The yellow and purple boxes highlight the years in which the* *average ELA at the scale of the study region is linked with positive (yellow) or negative (purple) winter precipitation anomalies (see section 4.2)*."