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Interactive comment on "Little Ice Age climate reconstruction from ensemble reanalysis of Alpine glacier fluctuations" *by* M. P. Lüthi

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Response to reviewer comments

I appreciate the valuable comments of all three reviewers. Three topics raised questions from all reviewers, and are discussed in the following. The remainder of the comments is answered below.

1 Reconstruction of recent ELA changes

page 5154, lines 7-10: "Since present-day glacier geometries have not yet fully adjusted to the climate of the last decades (depending on glacier response time) the reconstructed ELA during the last decades might be considerably higher"

Recent ELA variation cannot be fully reconstructed from glacier length changes because not all information of the signal (i.e. the ELA change) has yet been transported through the system. ELA variations cause mainly a volume change which only eventually leads to a length change at the terminus that can be used to reconstruct the full ELA change.

Numerical experiments for a step change of the ELA show that this ELA change can only be recovered after 5-10 years of GLC history. These synthetic GLCs were created for 5 LV-models with parameters that are similar to the ones from the reconstruction.

2 Role of precipitation

To evaluate the role of precipitation, the gridded precipitation reconstruction form Pauling et al. (2006) was used, with average values of precipitation from 12 grid cells over the Alpine area. The comparison with the reconstructed ELA record (Fig. 2 in the paper) is shown below in Figure 1 (with 3-year smoothing to alleviate the signal-to-noise problem). Three low-ELA phases in 1712-17, 1747-52 and 1767-77 seem to coincide with high-precipitation years. For the other periods there seems to be no correlation. Also summer precipitation is mostly uncorrelated with the reconstructed ELA and therefore not shown.

It seems worth adding the precipitation panel to Figure 2 in a next version of the manuscript.

3 Volcanic cooling

To investigate the question whether volcanic cooling might have caused the reconstructed very cold ELA periods, I listed these periods since 1590 in Table A below (they are given in the table as the 5 year intervals used in the optimization procedure). Data on volcanic eruptions is taken from Arfeuille et al. (2013) and also shown in Figure 1. For all these cold phases there is a large tropical volcanic eruption within 4 years of the cold period, with exception of 1783 Laki eruption (6 years later). On the other hand, not all eruptions are associated with a reconstructed low-ELA phase, although the biggest ones are. Seasonality of the eruption and stratospheric transport patterns play a crucial role for this.

The reconstructed cold phases that appear *before* an eruption are all within the time span 1710-1790, and are therefore due to the reconstruction of the Bossons and Mer de Glace glaciers. While the dates of volcanic eruptions are correct (by layer counting in many ice cores), not all dates of the GLC record are certain. After about 1770 the pictorial record for Bossons glacier is pretty good (Nussbaumer and Zumbühl, 2011), but before there are issues with dating the glacier depictions (e.g. error bars in their Figure 3).

While the above discussion does not away with the differences in timing of reconstructed ELA variations and volcanic events, the Table still supports the hypothesis of a possible causal relation between volcanic cooling and low-ELA periods. It seems useful to include such a table in the manuscript.

t_0	t_1	Δt	year	volcano	NH (Tg)
1587	1592	+0	1587	Kelut	
1602	1612	+2	1600	Huaynaputina	46.0
1637	1642	+0	1641	Parker	33.8
1677	1687	+4	1673	Gamkonora	6.3
1712	1717	-2	1719	Unknown NH	31.5
			1729	Unknown NH	12.0
1747	1752	-3	1755	Katla	8.0
			1761	Makian	8.4
1767	1777	-6	1783	Laki (mostly troposheric)	93.0
			1796	Unknown NH	6.7
			1809	Unknown	27.6
1812	1817	+0	1815	Tambora	58.7
			1831	Babuyan Claro	17.0
1837	1847	+2	1835	Cosiguina	26.4
1882	1892	+0	1883	Krakatau	11.2
1912	1922	+0	1912	Katmai	11.0

Table A: Phases of very low reconstructed ELA between 1590 and 1920, and explosive tropical volcano eruptions causing significant aerosol loading in the northern hemisphere (NH). The Δt row indicates the number of years of delay (+) or advance with respect to the eruptions. The estimated NH stratospheric aerosol loading (based on ice cores, Gao et al. (2008)) is given in the last column (data from Table 1 in Arfeuille et al., 2013).

Reviewer 1 (Tómas Johannesson)

page 5148, line 14: The last sentence in the abstract is perhaps a bit too strong. It, furthermore, combines temperature and volcanic radiative cooling in one as if volcanic radiative cooling affects glaciers independent of the temperature, whereas meaning should be (see comment below) that volcanic radiative cooling mainly causes temperature variations that then lead to glacier fluctuations. How about something like "... are thus mainly explained by temperature variations that before the end of the 19th century appear to be driven by variations in volcanic forcing to a large extent" or something like that.

Glacier mass balance is clearly dominated by summer temperature. Radiative forcing is another important aspect of summer mass balance, indirectly through air temperature, and directly through radiative melt of the ice Ohmura et al. (2007a); Huss et al. (2009). Stratospheric aerosols from explosive eruptions of tropical volcanoes can lead to a reduction of direct radiation of tens of W m^{-2} for several years while diffuse radiation is enhanced, but overwhelmed by the effect on direct radiation (Robock, 2000).

Based on these arguments, and Table A, I still think the last sentence of the abstract is appropriate, but I have weakened its statement. It now reads
"The glacier advances during the LIA, and the retreat after 1860 can thus be mainly attributed to temperature and volcanic radiative cooling."

page 5154, lines 7-10: This sentence seems to imply that the methodology presented in the paper would lead to higher reconstructed temperatures for the present-time climate if glacier variation records were available for several more (future) decades. Since the glacier model represents the lag of the glacier response with respect to the climate forcing, this effect should be taken care off by the model to a first approximation at least. Clarify.

This statement triggered comments from all reviewers. This point is answered on Page

1 above.

page 5157, line 18: Specify whether the 7 W/m2 refers to radiation impinging on the glacier surface that is partly reflected because of the albedo of the glacier surface or energy available for melting after albedo effects are taken into account.

Ohmura et al. (1992) state

> *Refinement of the relationship is possible by introducing global and long-wave net radiation. The equivalent values for temperature, precipitation and radiation at the glacier equilibrium lines are approximately 1°C, 350 mm w.e. and 7 W m^{-2}, respectively.*

Their analysis uses global radiation plus long-wave net radiation instead of net radiation, on which barely any data is available. They explicitly state that albedo effects (p. 400) that using their parametrization has the advantage to avoid albedo effects, which should be considered as internal properties of the glacier.

So the answer is that this is the "climatological budget" before albedo has been taken into account.

page 5158, line 7-9: State more clearly that the ELA sensitivity given in table 2 is inconsistent with Ohmura's value by more than an order of magnitude (1 W/m2 <-> 200 m; rather than 7 W/m2 <-> 100 m). The radiation variations on the right y-axis scale of figure 5b are ca. -0.5 to +0.5 W/m2 whereas the ELA variations are -100 to 100 âŮęm, corresponding to temperature variations on the order of a degree C. This seem to indicate that the TS variations cannot directly be important cause of temperature or glacier length variations? Discuss more clearly.

As reviewer Gerard Roe points out, this discussion is probably not meaningful, and I intend to discard it (i.e. page 5158, lines 7-12). It is not meaningful to directly compare TSI (top of atmosphere) radiation variations to values at the surface. As for the importance of TSI: I do not think that we can assess its influence from this comparison, except for a qualitative comparison of the curves.

page 5159, line 17-19: I am unsure what is meant by this sentence.

Indeed, this is not very clear. I deleted this whole sentence.

Page 5151, line 10: Delete "and per square meter" (inconsistent units)

Deleted.

page 5152, line 4: Should 283 values be 282 values?

1200/10 + (2010-1200)/5 +1 = 283 (including the starting value in the year 0)

page 5153, line 10: "data are"/"data are"?

Oh yes (sigh). Changed.

page 5155, line 24: "closely follow" is a bit too strong, might perhaps be "bear some resemblance to"

Adapted this suggestion.

page 5126, line 26: "differ from reality by the dynamical effects ..." might be "differ from reality because of the simplistic model dynamics, e.g. by the dynamical effects ..." (I don't think the small scale terminus topography is the only or main effect here)

Changed to "Obviously, modeled GLCs still differ from reality by the simplistic model dynamics and the assumed geometry which cannot represent the dynamical effects of small scale terminus topography."

page 5157, line 25: "only limited"/"limited"?

Adapted.

page 5159, line 15: "volcanic event"/"volcanic events"?

Singular seems appropriate here (not changed).

page 5160, line 27: "found ... in"/"inferred ... from"?

Adapted.

Reviewer 2 (Paul Leclerq)

Apart from some specific comments below, I have one general question. There are several papers on the natural variability of glacier fluctuations (e.g. Oerlemans 2000, Roe 2011). Significant length fluctuations can result from the random noise in the climatic forcing of glacier systems - regardless of climate change. Therefore, we should be careful to explain every glacier fluctuation in terms of climatic change. It might be useful to include some comments on this in the paper. Can the methods of this study be used to quantify the likelyhood that the observed glacier fluctuations are the result of natural variability instead of the reconstructed climatic changes?

I absolutely agree. But then, I don't see where in the paper I invoke "climate change" to explain the GLC variations. Frankly, I am not enough of a climatologist to appreciate the difference between "natural variability" and "climate change". Maybe I don't understand the comment, but of course every change in mass balance is due to short-term phenomena in the climate system, i.e. variations of temperature, precipitation or radiation. The aim of this paper is simply to extract the ELA-variation causing the observed glacier changes.

p 5148 line 22: Here and several times in the paper it is stated that glacier mass balance depends on summer temperature and radiation and on winter precipitation. This is not strictly true, although the mass balance is most sensitive to changes in temperature in summer. For example, Oerlemans and Reichert (2000) "Relating glacier mass balance to meteorological data by using a seasonal sensitivity characteristic" show *for Hintereisferner that glaciers in the Alps are sensitive to temperature changes from March to October and sensitive to changes in precipitation in the entire year (without specific seasonality which can be explained by the high impact of summer snow events, e.g. Oerlemans and Klok 2004 "Effect of summer snowfall on glacier mass balance"). I think this should better specified in the paper. It might also have an impact on the comparison with the other climate reconstructions. As far as I know, tree-ring data are mostly correlated with, and calibrated on, June-July-August temperature anomalies, such that glaciers and tree rings do not represent the exact same temperature signal. This should be mentioned.*

Mostly agreed with this comment, and changed accordingly in several places. As for the importance of summertime precipitation: this seems only episodically to be of importance for mass balance (i.e. cold front precipitation). In a comparison of the Pauling (2006) summer precipitation reconstruction with the reconstructed ELA history in Figure 2 above, there is no close correlation visible.

p 5152 line 5: Do the optimized values of Z0 and s of the model correspond to the actual values of the slope and altitude range for these 7 glaciers? Probably not, as the model has a constant width such that adjustments of the optimal Z and s will compensate for variations in the width of the real glaciers, but how large are the deviations?

These parameters are quite, but not entirely, unlike those of real glaciers. Table 2 in Lüthi et al. (2010) shows that mainly the bed slope s is often similar to terminus slope. Since this quantity controls mass transport, it might also include the effect of basal motion which is not accounted for by the model, and would lead to larger modeled slopes. How similar these values can be read from the following Table B, which also might be considered for the next version of the manuscript.

Glacier	Z	β_{LV}	β_{tot}	β_{low}	τ_v
Bossons	4235	15.80	24	24	8
Gorner	630	3.90	8.0	1.2	67
Grosser Aletsch	1090	3.50	3.5	3.	53
Mer de Glace	2190	6.70	9	4.5	22
Rhone	1940	5.90	6.6	5.	26
Rosenlaui	685	12.20	14.5	16.	27
Unt. Grindelwald	1090	6.80	12.	9.	32

Table B: LV-model parameters of the best-matching DESM: Vertical elevation difference of the accumulation area Z, bed slopes of the LV-model β_{LV}, average slopes of the whole glacier and the ablation area β_{tot} and β_{low}, and the Harrison volume time scale τ_v.

p 5154 lines 6-10: To me, this seems a rather strange argument. It hints that the ELA altitudes are underestimated in the last decades, which would correspond to the results in Figure 2b. We have had the same problem in our temperature reconstruction from glacier length changes: the reconstructed temperature rise over the last decades in the Alps was smaller than the temperature rise in the instrumental record (Leclercq and Oerlemans 2012, Fig 5b). So, although I hoped that the two degrees of freedom LV-model would do a better job than our one degree of freedom L-model, this result not surprising. However, the response time is included in the LV-model, which models the dynamic response of glaciers. Because of this dynamic response, the glacier geometry is in general not in balance with the climate, but this is exactly what the glacier models try to capture. Therefore, the argument that the ELA might be even higher because the glacier geometries have not fully adjusted to the present-day climate seems silly. Moreover, if recent ELA changes are underestimated, why would this be different in earlier centuries?

Probably silly is not the appropriate term here, but it might appear surprising at first sight. The issue is discussed in Section 1.

p 5155 l 10: Fig 3c

Corrected.

p 5155 l 13-14: Why does a spatial average of temperature result in an increase in the sensitivity of ELA to temperature?

It doesn't. But hemispheric spatial averaging reduces variability compared to the local temperature variations, therefore we compare to a different signal with lower amplitude.

p 5155 line 7 - p 5156 line 1-2: I am not really convinced by the relation between volcanic eruptions and ELA. The high sulphate concentrations seem to coincide with, or be followed by, high and low ELA episodes more or less equally often.

Agreed that the coincidence is not one-to-one. However it seems striking that most negative ELA spikes appear in close to a volcanic eruption. Assuming that a casual effect exists between volcanic eruptions and low ELA, one should discuss the time lags (both positive and negative) between the two. See also discussion in Section 3 and Table A.

Most large tropical volcanic eruptions are precisely dated (from polar ice core stratigraphy), but the strength of their effect is a result from GCMs used to understand the spreading of the aerosols around the globe, which might take a year (e.g. Robock, 2000), so the potential lag is of minor influence here. On the other hand, GLCs are reconstructed from old paintings which sometimes are not accurately dated, and radiocarbon ages of trees with sometimes important error bars. In addition, the dated tree trunks are recovered from a variety of positions with respect the glacier shout, which itself is not of constant shape, so that another source of dating error results. Furthermore, the ELA reconstruction uses bins of 5 years with constant ELA, such that the dates of reconstructed ELAs are of the same order at best.

Whether the above, often not quantifiable, dating offset can explain the time lags between volcanoes and cold ELAs could be assessed by forward modeling of ELAs with

a combination of climate time series as forcing functions. Such work is currently in progress, and hints to an important impact of volcanic forcing.

p 5156 line 19: I have the feeling that "unaltered" refers to the smoothing done in the method used in Oerlemans 2005 and Leclercq&Oerlemans 2012 etc. The smoothing of the GLC in the inverse method of Oerlemans corresponds with accepting a mismatch between modelled and measured GLC in the forward approach, both using the same argument of lines 25-27.

Yes, "unaltered" refers to the method in the cited papers where smoothed GLCs are used. I do not think that smoothing the data, and matching data in a least square sense with a dynamical system is the same, or leads to similar results. Removing the variability of data by smoothing does not allow for high frequency signals to be transferred to the forcing function found by inverse modeling. So my feeling is that the linear response model would yield different results if it were used on unsmoothed data in an optimization procedure similar to the one employed here.

p 5161 line 12-14: What do you mean with "the relative importance of different proxies in resulting GLCs"? Sentence is not clear to me. It seems to express that proxies, instead of climate variations themselves, cause glacier fluctuations, but that would be nonsense.

This is on page 5162. The sentence has been reformulated as
"Work to identify the relative importance of different climate fields for the resulting GLCs with help of macroscopic glacier models is currently under way."

Table 2: Are the units of TSI correct? I would expect m3/W

Steinhilber et al. (2009) gives TSI in W/m^2, as it should be for a radiative flux.

Reviewer 3 (Gerard Roe)

I do have some reservations about the climate interpretations made by the author. - Glacier dynamics act as a low-pass filter, meaning a significant fraction of the climate variance can never be recovered. For glaciers with response times that may be a few decades, there is considerable suppression of variance even in the decadal to multidecadal frequency range. This should be mentioned. It seems like it should show up in the analyses here as uncertainties in ELA reconstructions (The model will be less sensitive to the details of ELA fluctuations at higher frequencies which are damped). The suppression of variance and the uncertainty as a function of timescale should be noted. A table showing the optimized timescales/parameters of the LV model would be helpful to understand the timescales on which this occurs. I would like to have seen uncertainties on the ELA reconstruction presented.

These are very good points. Most of these are addressed now in the paper with the additional Table B on LV-model glacier parameters and time scales.

Concerning the possibility to recover short term variability it is important to keep in mind that the presented method uses several glaciers with different reaction time scales, and therefore allows to obtain higher frequency forcing functions than by using individual glaciers only. Nevertheless, the choice of 5-year intervals to represent ELA history already limits the climate variability that can be recovered by the method. and is slightly shorter than den shortest volume time scale of 7.5 years for Bossons glacier.

The LV model in Luthi (2009) had some phase issues at frequencies higher than 1/response time. It seems like that might be an issue here given the long response time of some of these glaciers.

The phase issue was in comparison with a full model on the same geometry with the same boundary conditions, i.e. zero basal velocity. The slower reaction of the FS-model with respect to the LV-model (Fig. 17 in L09) is partly due to the no-slip

boundary condition, as the terminus first has to become thick enough to advance. With basal sliding boundary conditions the response of the FS-model response would likely start earlier. This case is also more realistic, however, there is a gap in knowledge on the dynamics of a glacier terminus during glacier advance. Therefore it is difficult to quantitatively assess how important basal motion is for terminus advance, and how realistic the LV-model is in that respect.

The association with volcanoes does not seem to be as strong as the text implies. Prior to 1600 there is no connection, even visually. After 1600 it seems that only around half of the ELA lowerings can be clearly associated with a concurrent or preceding eruption. The analysis is precise enough for these to be quantified rather than loosely characterized.

This is now addressed with Table A in the beginning of this document. I will add a similar table to a new version of the manuscript.

Mass balance is discussed in terms of winter accumulation, summer temperature and summer radiation. For reasons detailed in the comments below this seems like an awkward grouping. It neglects the important role of wintertime temperatures, and treats temperature and radiation as independent. The important differences in the nature of the radiation (e.g., downwelling shortwave vs net longwave) are not made clear. Elsewhere the distinction between summertime and annual mean temperature is not made clear.

This is answered in the points later on.

I would have liked to see this presented as an ELA reconstruction rather than a temperature reconstruction, and I think the paper would have been stronger. I think that the dismissal of the importance of precipitation changes is not justified.

I don't understand this comment. The optimization is on ELA variations, and results are presented as ΔELA in Figures 2, 3, and 4.

There are ordered by the page of the pdf document that I download, and quotes a phrase near the point in the text to which the comments pertained.

Apparently the reviewer used the "print version" from the website, which, he claims, contains no page numbers. However, downloading the print version from TC provides a version with page and line numbers. Gerard Roe kindly sent me his marked-up PDF version, so that I am confident to accurately locate the questions. Also that version has line numbers, which somewhat adds to my confusion on the version of the manuscript which was reviewed.

P1:"climate parameters" These would normally be called "climate fields"

Replaced as suggested by the term "climate fields"

P1: "by their rate" does not scan well

Replaced by "by accumulating or losing ice volume"

P1: "equilibrium line altitude" I appreciate the focus on ELA not temperature,

Thank you, that's the whole point.

P1: Atlantic Multidecadal Oscillation

Good catch! Corrected.

P1 "Most" There are a lot of qualitative statements like this that could be quantified.

Table A and the precipitation figures help somewhat to alleviate this shortcoming. I don't think that calculation of cross-correlations is very meaningful for this study.

P1 "explained by temperature and volcanic cooling alone." The explanation is not total, is it even a majority of the variance?

Additional work is underway to characterize this quantitatively. The strong concluding sentence is formulated in a weaker sense now as
"The glacier advances during the LIA, and the retreat after 1860 can thus be mainly

attributed to temperature and volcanic radiative cooling."

P1 "same climate variables" I think this is a bit carelessly phrased. Winter temperature affects the rain-snow line, trees don't care much about winter precipitation.

Shortened to
"Records of glacier length changes (GLCs) provide supplementary independent information but are caused by fundamentally different processes."

P1 "proxies" GLCs are also proxies.

changed to "other proxies"

P2: "valid on long time scales." I am not sure I buy this. It is well known that treerings do not give very trustworthy information on multidecadal timescales (it is hard to remove growth issues). Perhaps there is a sweet spot around decadal time-scales?

Those studies mostly compare GLCs to solar forcing, so I don't think changing this statement is needed.

P2: "bedrock geometry and ice thickness distribution." Also on climatic parameters, like lapse rate.

Added "and local mass balance distribution."

P2: "slightly over-damped harmonic oscillator" Didn't Harrison use a critically damped system rather than an over-damped one? I am not a big fan of this interpretation of the equation (and there are others) as there is nothing intrinsically oscillatory about nonlinear diffusion of ice flow.

This statement describes the behavior of the LV-model linearized around a steady state is the harmonic oscillator equation, as described in Section 3.6 of L09, and Figure 2 shows that it is slightly over-damped. In my opinion it helps the reader understand the essential dynamics of the LV-model, which is a quite boring dynamical system (no chaotic trajectories).

P2: "advantage" I suppose it cuts both ways because it means there is tuning involved, which reduced the degrees of freedom for comparison.

Obviously, yes, this is the topic of the paper.

P2: "on" should be of.

Fixed.

P2: "dynamically equivalent simple model" Seems like an unwieldy phrase. The author hardly uses it again.

This term was already used in Lüthi and Bauder (2010) and Lüthi et al. (2010). For consistency I'll stick to this term.

P2: "summer temperature, winter precipitation and radiation" This triplet has been used perhaps a little carelessly. Winter temperature matters for accumulation (snow vs rain); really ablation only depends on radiation and sensible/latent heat fluxes since it is energy, not temperature per se that causes melting. In various papers Oerlemans shows lots of good figures of the seasonality of the sensitivity to different climate variables.

Winter temperature rarely plays a role for the high accumulation areas, but might for the ablation area. Summer precipitation seems to have little correlation with the reconstructed ELA, as Figure 2 above shows. But I agree that these statements might be over-simplified.

P2: "from their last maximum extent of the Little Ice Age" This statement applies only to the Alps. For Scandinavia, it was 100 years earlier, and elsewhere on the planet the picture is unclear.

The paper explicitly deals with glaciers from the Alps (even in the title). Nevertheless, I have added a qualifying statement at the beginning of the sentence "In the Alps, ..."

P2: "The direct response is caused by the difference between the rates of ice melt and mass transport to the terminus." I don't think it is true to imply that the terminus position

has a direct and instant response to climate. Ablation rates rates are typically many m/yr and so are far larger than any local climate fluctuations (typically 10s of cm/yr). Transport fluctuations dominate the terminus position.

Terminus position certainly has a direct and instant response to climate. The terminus of a steady state glacier moves with speed $u_t = \dot{b}_t \sin\alpha$, with $\dot{b}_t$ the melt rate perpendicular to the surface. Terminus slope angles of steady glaciers are of the order $30°$, such the forward motion is about 50 % of the terminus melt rate. If melt is strongly reduced, e.g. by a short ablation season and/or cold summer temperature, the glacier *immediately* advances by 50 % of this reduction.

P2: "Ice melt." Ice melt occurs over much of the glacier. The melt-line is far up the glacier so melt anomalies must also be transported.

Yes, I agree. And I don't understand the point being made.

P3: "The climate history" This doesn't give enough information to know what was done. Were the length data interpolated onto a regular timescale? Was the numerical integration done on annual time-steps? All glaciers were weighted equally I presume? I am not familiar with the optimization algorithms, and a few thoughts about whether these were used just for convenience, or efficiency, or if there were other reasons would be helpful.

The integration of the LV-model was done with a Runge-Kutta-like ODE solver (LSODA from the ODEPACK library, using Adams and BDF methods.) So the time step varies according to the needed accuracy. (In this case, fixed time steps of a year give almost the same result.)

For clarity, this explanation was added to the section:
"The LV-model (Eq. 1) was solved forward in time with an ODE solver (LSODA from ODEPACK), and evaluated on a yearly time step. To drive the LV-model and..."

No data interpolation was done, and the yearly modeled GLC values are compared to the measurements.

P3: "These 283 ELA values" The ELA history of these glaciers is assumed to be identical. It is possible to evaluate that assumption from the interannual variability of the observational record. More importantly, if that assumption is made, it means these glaciers do not provide independent information about climate. So the main advantage of having multiple glaciers is that they have nonoverlapping length histories. And the hope is also that the climate reconstruction averages out the vagaries of individual glaciers. This should be clearly stated.

Not quite. The main advantage of using several glaciers is that the forcing signal can be recovered by their different reaction to the same forcing. This would not be possible using glaciers with the same response times and characteristics (or using just one glacier with a complete record).

The advantages of the method are stated in the results (starting on line (5152/26), the discussion (starting on line 5156/15), and in the conclusions. I don't think it is appropriate to put further qualifying statements in a "methods" section.

P3: A large penalty" What is the constraint on the ELA in intervals when there are no length data available?

There are no constraints during these periods. No results are shown because of the large differences between modeled length an ELA variations. To clarify this I added the statement:
"A large penalty was added to the cost in case of violation of one-sided constraints on glacier length (e.g. living trees or terminus moraines), and no cost was added if no constraints are available. "

P3: Fig. 1. It is very hard to see the black dots unless blown up large on my screen. In print this is going to be a problem. There are vertical orange bars in Figure 1 that are not described.

I agree that the dots are small. However, bigger dots would be blurred together, and a bigger figure seems not possible in this screen-sized journal.

There are no vertical orange bars in Figure 1. The horizontal bars are described in the figure caption (they represent life times of trees).

It is not clear what the point of showing 20 red lines of GLCs is (and they are very hard to distinguish). 20 is an arbitrary choice (out of how many attempts?), and I don't see why the author does not just show the best. On the other hand it would be very informative to see some idea of the spread in the ELA reconstructions, as those are going to be more poorly constrained, and that is your real product in this paper. You could do this with box and whisker plots on top of the ELA reconstruction bars.

I completely agree. The idea was to indicate the spread of possible solutions (20 out of ten-thousands of model runs). Maybe it would be better to show standard deviations as a shaded area, but that would not help with the readability of the figure.

As a remedy, I'll use a single line as proposed, and try to indicate the spread of solutions, probably with shaded areas or additional lines.

P3: "for all glaciers" It is not really an independent test - it is just that the glacier variations have been very coherent since 1800 or so. The variations among the different glaciers 1600 to 1800 are not captured well. And they can't really be, by construction, since the length history is just a smoothed and filtered version of the same ELA time series.

I'm not sure I understand this comment. Why would it be impossible to obtain the correct variations between the different glaciers driven by the same ELA history, assuming that a better model with more realistic geometry were used?

P4: "robust" Robust is not a quantitative word (how robust?) so it does not make sense to give quantitative percentages for the parameters.

Of course, "robust" is not quantitative. The meaning is that the result does not look significantly different for 10% variations in parameters. I will quantify this for the final version of the paper.

P4: "low summer temperature, reduced incoming radiation, high solid precipitation)" I've already made a comment that I think this is an awkward triplet of variables for talking about climate controls.

I removed the explanation in parentheses.

P4: "high-amplitude ELA oscillations" I would not describe these as oscillations. High amplitude depends on context. What is the interannual variability in ELA in observations of these glaciers?

Replaced "high amplitude" with "rapid". The ELA variability during the last century is given for some glaciers by Figure 10 in (e.g. Huss et al., 2008): Interannual variations exceed 300 m.

P4: "the reconstructed ELA during the last decades might be considerably higher." I don't understand this comment. The LV model is already accounting for the glacier adjustment time.

This is discussed in the introductory section.

P4: "To obtain similar variability of the records" In doing this the author is going to overestimate the coefficient somewhat. He is asking temperature alone to drive the glacier record, when we know that at least a component of it is due to accumulation variations. A rough estimate of their relative importance can be seen by looking at the standard deviations of interannual variability in winter and summer mass balance in the world glacier inventory. It is between 1to1 and 2to1 (with summer winning) in the Alpine glaciers I've looked at.

Agree on the comment. But for a meaningful comparison between two proxy records a scale has to be chosen. This exercise does not attempt attribution (which is the subject of an ongoing work), but compares other proxy records to the ELA reconstruction.

P5: "controlled by a combination of processes" Not to mention all the errors in the proxies!

Agreed, but impossible to quantify in any meaningful way.

P5: "low elevations from where most proxy records are recovered." We know this is not the case from the modern climate. A wet year in the valleys is also a wet year on the peaks, similarly for summertime temperature. It is the horizontal separation of the records that is more important.

To my knowledge there are only very few studies showing this for longer term climate, notably by Gilbert and Vincent (unpublished PhD thesis), who find similar temperature variations (thus supporting your point). However, the high accumulation areas might see different precipitation systems. But I agree that lateral variations in precipitation might be important, too. (Nothing changed).

P5: "Figures 3 and 4" The captions must state very clearly what exactly is being reconstructed. There is a mixing of annual mean and summertime temperatures

These are just the records from the respective papers. The request information will has been added.

P5: Atlantic Meridional Oscillation" It is the Multidecadal Oscillation. Mann's (2009) time series is arguably not a proper reconstruction of the AMO, it is just the SSTs is a box in the North Atlantic, and departs from the original definition.

Agreed. Since the original also references this series as AMO, I don't see a need to change this.

P5: "at least partially, close similarities". Can't be partial and close at the same time, to my ear.

Changed to "... that show, at least during limited time spans, close similarities."

P5: temperature history" This is annual-mean temperature not summertime recon-

structions, and for the entire NH, not Europe. Throughout it is important to be very clear about what kind of temperatures have been reconstructed.

Added “annual-mean”. Northern Hemispheric is already given here.

P5 "presumably due to hemispheric averaging of the records." And because the author has neglected precipitation. Also, and maybe most important, the author has averaged the ELA in 5 or 10 year chunks, which squashes the variance by a factor of sqrt(5) or sqrt(10), so he needs to be very careful about the ratios of ELA to temperature, which has been averaged differently by both the trees and your analysis).

Yes, agreed. But then these records are also smoothed by the somewhat arbitrary 7-year running mean. In my opinion, the only way to assess the influence of these climate fields is to run a forward model forced with these data sets (or linear combinations thereof), work which is currently in progress.

P5: "temperature history" What kind of temperature?

PAGES 2k consortium (2013) shows annual-mean temperature. Again added “annual-mean”.

P5: "summer (JJAS) temperature" This depends on the target variable of the particular proxy reconstruction.

I don’t understand this comment. Trees don’t care about the target variable.

P5: "closely follow" It doesn’t to my eye. There are a lot of qualitative comparisons being described in the text. But why not a calculate correlation coefficient where the time series overlap and so calculate the shared variance? It would be an objective measure of these statements.

I don’t think that calculating correlation coefficients is useful for these different data sets, and would provide any more insight than the inspection by eye of the similarity of the curves (nothing changed).

P5: "phases coincide with, or are preceded by," I am unconvinced by this qualitative comparison. Volcanoes have an effect on climate for only 3 to 4 years, and obviously for it to be a cause of cooling, it must precede low ELAs. Later you talk only about the LIA, but you do not qualify the statement here.

This is discussed in more detail above.

P5: "explains the long term GLCs". I think this is an overstatement – the overall 20th century decline of all glaciers is captured, but there are plenty of errors where the glacier records overlap in the 17th to 19th centuries.

Considering that the model geometry is quite different from reality, and that ice dynamics and mass balance are very inaccurately parametrized in the LV-model, I still think that this is remarkable. The fast-reacting glaciers, for example, seem to have had an extent around 1550 that is comparable to 1870, while the slowly-reacting glaciers don’t.

Nevertheless, I made this statement less bold, claiming now
“Figure 1 shows the remarkable result that one single ELA history (Fig. 3) causes long term GLCs of seven Alpine glaciers with very different geometries that mostly agree with the documented record.“

P5: "seven Alpine glaciers" The fact it is 7 is not that remarkable I think. We already know that the climate is highly coherent on these space scales. The different geometries filter the climate signal a bit differently, but it does not strongly affect the coherence of the glacier response (Huybers and Roe, J Climate, 2009). Moreover the author has optimized a scaling factor for each glacier that would subsume any difference in the magnitude of climate variability from place to place.

Response time and climate susceptibility are quite different for all of these glaciers (at least in their LV-model representation), so I think it is remarkable that it is possible to find a ELA history that mimics all glaciers at once (nothing changed).

P5: “the important assumption that all GLCs are caused by the same ELA history.”

Again, we already know that spatial coherence of Alpine climate is high.

OK, so we agree that this assumption is valid (nothing changed).

P5: "precipitation, vary on short spatial scales". The magnitude can vary on short spatial scales, but the spatial coherence of the temporal variability is very high. (i.e., a wet year on one glacier is a wet year in another glacier, etc.).

Added " while their variability is similar".

P5: "Our assumption of similar ELA variation is supported" I don't think so. It would be true for precipitation too because of the high spatial coherence.

This discussion has now been streamlined to dwell less on the individual components. It now reads
"This result justifies a posteriori the important assumption that all GLCs are caused by the same ELA history. This assumption is also supported by studies of glacier mass balance variability (Vincent et al., 2004; Huss et al., 2009). Even if all glaciers are in the same mountain range and within 130 km of each other, local climate, and especially precipitation, vary on short spatial scales, while their variability is similar (e.g. Frei and Schär, 1998; Casty et al., 2005). Long-term instrumental records show that variations in temperature are closely linked over the Alps (e.g. Casty et al., 2005; Auer et al., 2007)."

P5: "dominated by summer temperature and radiation". I think dominated is too strong. The standard deviation in summertime and wintertime mass balance are available for a selection of Alpine glaciers from the World Glacier Monitoring Service and for the Alps varies between 1:1 and 2:1 (with summer winning), but 'dominating' is too strong for these glaciers.

Changed, see last comment above.

P5: "infer missing data of the remaining glaciers." Well, but only when we already known that the climate is highly coherent, and in that case there is no independent in-

formation about climate from different glaciers in the same region. They all experience essentially the same climate and just filter it to slightly different degrees.

True. The point discussed here is that we can infer timing, and to a lesser degree also extent, of the GLCs from other glaciers. This in turn could help interpreting the disjoint GLC records as a whole data set, and might help with dating problems.

P6: "mainly (75%) on temperature and to a lesser degree (25%) on radiation (Ohmura et al., 2007)" I don't like this generalization. Different studies arrive at different values and at minimum an uncertainty should be given. The Ohmura study was a regression relationship and for a handful of glaciers. Energy is the real driver of ablation, and that is radiation, and sensible/latent heat fluxes. Near surface air temperatures (which I think you mean) influence the heat fluxes but they are also a consequence of it. It is confusing to mix temperature and radiation.

The numbers 72% resp. 28% given by Ohmura et al. (2007a) stem from interpretation of four long mass-balance series, and using radiation observatory data. Even if this is only a "handful of glaciers" there are no other seasonal mass balance series of long-enough duration. The only papers I am aware of investigating the role of radiation on decadal time scales are Ohmura et al. (2007b) and Huss et al. (2009). Of course I am grateful for pointers to further literature. From both publications I gather that variations in air temperature (as measured by remote meteorological stations) and incoming short wave radiation (as measured by radiation observatories) control summer mass balance independently. In that sense I don't think it is confusing to mix temperature and radiation, but confusion might arise if "temperature" is considered right at the glacier surface which is strongly controlled by radiative fluxes.

P6: "high interannual variability," Wintertime accumulation variability is not negligible here.

I agree that precipitation and wintertime accumulation is important, but to a lesser degree than summer melt. According to a reanalysis of long Alpine mass balance records (Huss et al., 2008, Fig. 7) winter balance variability is 2 to 3 times smaller than summer balance variability.

P6: "The influence of precipitation on the ELA is relatively small" First, this is not a precise statement (what does "relatively" mean?). Second, the numbers cited do not support the point. The standard deviation in summertime temperature in typically somewhat under 1oC. At high elevations in mountainous regions annual accumulations are often several meters. So 350 mm/yr represents only 10to20

It is not only the temperature changes that are important for summer balance, but also the duration of the melt period which varies substantially from year to year, and also on decadal time scales.

P6: "TSI change of 1W m−2" This is a sloppy comparison (sorry for the directness). TSI is the radiation at the top of the atmosphere in a plane perpendicular to the sun's rays. The Ohmura calculation is for total energy over summertime season. One needs to divide the TSI by four to get daily-mean insolation, multiply by something like the cosine of latitude, and multiply by the albedo to account for the fact that most of the solar radiation is reflected off the bright ice surface.

Thanks for the directness. I think the most reasonable thing to do is to entirely leave away this section (i.e. page 5158, lines 7-12).

P6: "This sensitivity is considerably higher" I don't understand the logic here. The author has already excluded TSI on the basis that it does not match the record.

Left away now (see above).

P6: "this range" what range? The meaning is unclear.

The meaning was "order of magnitude", left away now (see above).

P6: episodic and rapid glacier advances" I don't think the author has made a strong case for this. What does "episodic" or "rapid" mean?

Here I'm lost. Episodic and rapid describes the data, for example the GLCs of Bossons, Mer de Glace and Grindelwald during 1600-1850. I don't see how one could argue about this (nothing changed).

P6: "radiative summer cooling big volcano eruptions." missing word?

Added "by".

P6: "decadal time scale" This is a little long.

Agreed. Left away "and decay on decadal time scale".

P6: "0.2K" This presumably refers to a specific event, but the preceding is talking in general terms. Also do note that this number is smaller that the interannual variability in summertime temperature (often around 0.8 to 1C). To assert the role of volcanoes in this way is to also accept that interannual variability is comparably (or more) important: for a four year average the summertime-temperature standard deviation would be 0.8C/sqrt(4) = 0.4C

This part has been streamlined to
"They cause incoming solar radiation reduction by several $\mathrm{W\,m^{-2}}$ for two years (e.g. Robock, 2000; Weber, 2005; Fischer et al., 2007) and an effect of 0.2 K cooling lasting about four years in Central Europe was inferred (Esper et al., 2013)."

P6 "four years" This is more like the right time scale.

Changed, see above.

P7 "Longer term persistence" There really isn't much long-term persistence in summertime temperatures. Calculate the autocorrelation of the any long-term station data or mass balance data in Europe and you generally find negligible persistence (Burke and Roe, Climate Dynamics, 2013). Nor do you need it to generate persistent glacier excursions. A white noise climate forcing time series has no persistence, but equal power at all frequencies. A glacier acts as a low-pass filter producing persistent glacier *excursions (e.g., Roe, J.Glac. 2011, and plenty of others).*

In this section I was citing some literature about this controversial topic. Apparently the reviewer disagrees with the conclusions of Miller et al. (2012) or Schleussner and Feulner (2013) which propose persistence through sea ice formation and consequent ocean circulation changes in the North Atlantic.

P7 "or are closely preceded by" Can you not quantify this? It looks like no more than maybe half can this be said to be true. It is very unconvincing prior to 1600 and not very convincing afterwards. Taking the strict criterion that the volcano must precede the ELA lowering (and blowing up the figure to see the alignments clearly); for the period beginning around 1600, I count 9 events where there is an ELA lowering exceeding 100m. Of these, there are 4 or 5 that have volcanoes preceding or concurrent with the start (depending on the size of the magnifying glass), and 5 or 4 that do not.

These concerns are treated in the section at the beginning.

P7: short-lived, rapid" Can the author refer to a figure and point out examples of what you are thinking. I am not convinced of any rapid advances.

Figure 1 shows rapid advances for Bossons, Mer de Glace and Grindelwald, i.e. those glaciers with relatively good GLC records. I consider a 500 m advance in 7 years as “rapid” (see e.g. Nussbaumer et al., 2005; Nussbaumer and Zumbühl, 2011).

P7: "advances during the LIA", Why not just interannual noise (e.g., Oerlemans, Ann. Glac., 2000; Roe, 2011)?

Isn’t it interesting that no large and rapid advances are observed after ca 1870? So there seems to be some fundamental differences between the LIA period and after. I have a hard time believing that the observed rapid glacier advances could be triggered with white noise forcing, but should do some numerical experiments with the LV-model to exclude that possibility.

P7: "Atlantic Meridional Oscillation" Atlantic Multidecadal Oscillation. Please get this

right!”

Corrected.

P8:” "multiproxy" but still largely trees and a lot of overlap with Buentgen, so not independent

Agree (nothing changed).

P8: "Another possible explanation" Also possible is that the proxies are wrong! They are more complicated and nuanced than the instruments!

Sure, proxies can be wrong, too. This is why I write “another possible explanation” (nothing changed).

P8: "temperature reconstruction" I would have preferred to see this claimed as an ELA reconstruction.

Agreed, I’ll changed that back to ELA. The sentence now reads
“The presented method to reconstruct ELA variations from a set of GLC records yielded a new 1600 yr history which is independent of other instrumental or proxy data.”

P8: "most of which closely correspond to explosive tropical volcanic eruptions." No. The claim is only true (and still not that convincing) for the LIA period.

With the data given in Table A this might be more convincing. I have added the qualifying statement “during the LIA”.

P8: energy balance on the glacier surface." This is an example of where it is confusing to talk of both radiation and temperature. The author cannot neglect the fact the temperature was lower because the radiation was lower. You also have to be careful because the temperature data has been smoothed (both in the analysis and by the proxies), meaning spikes in the temperature time series have been blurred into dips.

I agree that temperature was lower because radiation was lower. These lower temperatures reduce the sensible heat flux to the glacier surface. But then there is also the *direct* influence of radiation on ice melt due to penetration of radiation into the ice, or absorption at the surface. So, I do not see a problem in the above statement on the energy balance at the glacier surface.

P8: "radiative forcing and the cooling due to changing stratospheric volcanic aerosols are not yet well understood." But you earlier cite the Ohmura paper with confident numbers saying 75 "Table 2." I don't really like these coefficients. They are not comparable since different temperatures (summertime vs. annual mean) are being used. The units on TSI look wrong. As mentioned earlier, they are dangerous to compare with other studies because both the ELA reconstruction and the proxy reconstructions have been smoothed and smoothed differently, which will affect the regression relationship.

Qualified this as "summer mass balance":
The dependence of summer mass balance on radiative forcing and the cooling due to changing stratospheric volcanic aerosols are not yet well understood.

The units on TSI are W/m^2 in Steinhilber et al. (2009). I do not understand the comment about smoothing, as the ELA reconstructions shown here has not been smoothed, although the proxies are.

"Fig. 1. Comparison" What are the solid vertical black lines in Figure 1?

The only black lines in this figure are dotted, and indicate the centuries.

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Please also note the supplement to this comment:

http://www.the-cryosphere-discuss.net/7/C3051/2014/tcd-7-C3051-2014-supplement.pdf

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Interactive comment on The Cryosphere Discuss., 7, 5147, 2013.

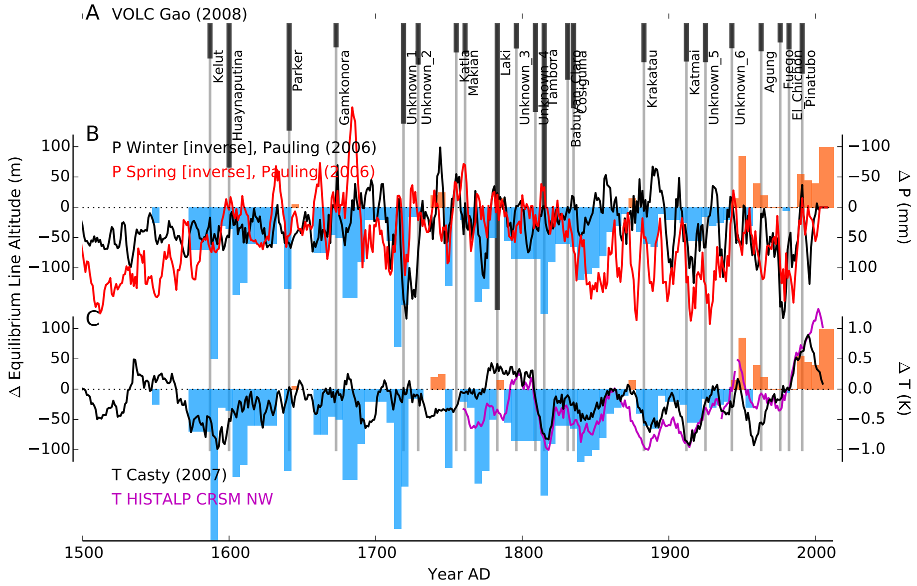

Fig. 1. Modeled ELAs are shown with red and blue colored areas. Instrumental records are shown for comparison in panels \textbf{(B)} and \textbf{(C)}. Precipitation from \cite{Pauling&al2006} is smoo

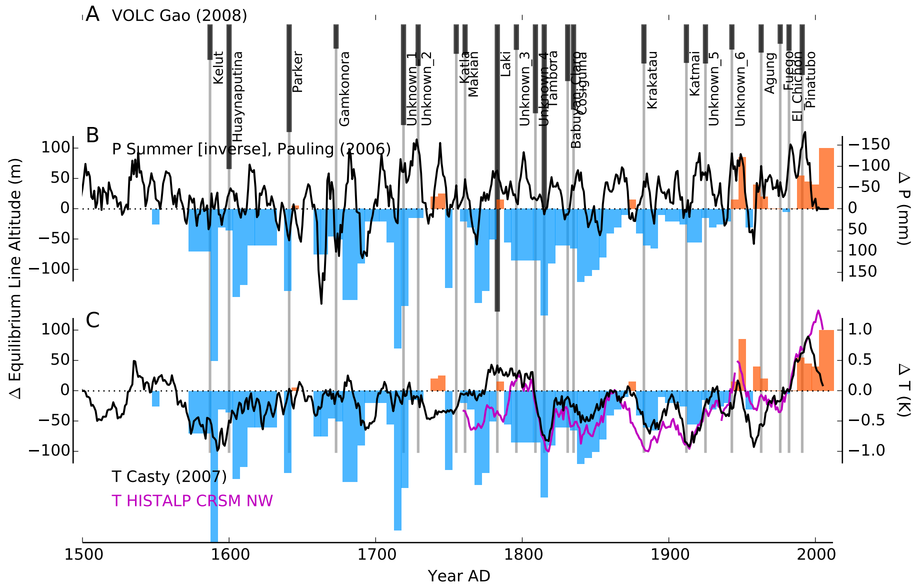

Fig. 2. Same as Figure 1, but with summer precipitation from Pauling (2006).