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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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Overall comments:-

This is an interesting, new approach to recovering climate history from glacier length variations. It is good to know that the combination of simple models and optimization algorithms have reached the stage where direct inversion methods are no longer required, and irregularly sampled data can be used.

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.

-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 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.

-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.

-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.

Specific comments

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.

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

P1: "by their rate" does not scan well

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

P1: Atlantic *Multidecadal* Oscillation

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

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

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.

P1 "proxies" GLCs are also proxies.

P2: "valid on long time scales." I am not sure I buy this. It is well known that tree-rings 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?

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

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.

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

P2: "on" should be of.

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

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.

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.

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.

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.

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.

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.

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

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.

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.

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

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.

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?

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.

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.

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

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.

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

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.

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

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.

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).

P5: "temperature history" What kind of temperature?

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

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.

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.

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.

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.

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.

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.).

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.

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.

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

P6: "mainly (75

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

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

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.

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.

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

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?

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

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

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

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

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).

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.

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.

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

P7: "Atlantic Meridional Oscillation" Atlantic *Multidecadal* Oscillation. Please get this right!"

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

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

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

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.

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.

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.

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

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