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Climate of the Past Discussions

Interactive comment on "Glacier equilibrium line altitude variations during the "Little Ice Age" in the Mediterranean Andes (30°–37° S)" *by* Álvaro González-Reyes et al.

Anonymous Referee #2

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1 GENERAL COMMENTS

The manuscript tackles the very interesting question of the influence (if any) of the LIA experienced in the Northern Hemisphere between 1500-1850 CE on the Mediterranean Andes. Little research has been published about this subject, making this line of research of great interest. The authors use a novel and promising method based on the derivation of glaciological parameters from GCM reconstructions. This approach could provide relevant insight into the climatic anomalies during the Little Ice Age in the Mediterranean Andes, and the effect of such anomalies on glacier mass balance.

Although the title suggests that the authors will set the focus on the climatic variations during the LIA, the abstract and introduction explain that serious doubts exist about the very existence of an LIA climatic anomaly on the MA. Therefore, the main question becomes whether there is or not an LIA climatic anomaly on the MA. However, the rest of the paper focuses on the climatic analysis of the LIA itself, not allowing the comparison of the LIA with the centuries immediately before and after it. It would make sense to perform the ELA calculation over the whole millennia covered by the GCMs used, or at least starting a century or two before the LIA and extending it to the present. In such a way, it would be possible to establish whether the ELA during the LIA was anomalously low or not.

Using such a novel method, the authors should put more effort into the validation of the outputs. The comparison of the ELA time series derived from CR2 data with Universidad Glacier studies is an interesting exercise, but there are only four data points, and the fit is poor and with non-systematic differences. Arguably, the aim of the method is not to reproduce the ELA of a specific glacier, but to use the ELA as a proxy for regional glacier Mass Balance. Therefore, the misfit with the Universidad Glacier ELA would not be a problem, but it would also be of little help to assess the performance of the method (especially because the authors made this comparison using CR2 data instead of the past1000 runs of the CMIP5). In general, a more significant effort needs to be made to validate the methodology. For this, it would be advisable to extend the computation of the ELA to the present using the past1000 runs and compare it with a compilation of the mass balance data available in the MA. It would be reasonable to also apply the method over the Alps using the same CMIP5 runs and using the wealth of validation data available in that area. This Northern Hemisphere test would also allow seeing how well this method can identify the LIA anomaly in an area where its existence is well established.

Also, the authors should acknowledge the significant errors that might arise from the simplified model used to compute the ELA, but emphasizing that they aim to capture

the relative variations of the ELA in time, not to compute exact past absolute values.

In some instances, it appears as the authors are picking features of one or another model to support an assortment of ideas. However, they avoid an honest discussion of the substantial differences between GCMs, and the implications of such differences in terms of reliability.

Finally, much clarity could be gain with less liberal use of the concept of “Equilibrium Line Altitude”. Often the manuscript refers to “daily” and “seasonal” ELA, while this concept is valid, by definition, only at an annual timescale. The method to compute the ELA is explained in a very superficial way. However, the reader can guess that they determine the ELA as the elevation at which mass balance is zero over any given time interval. However, when the authors compute a daily “ELA”, it is unclear which value should it take in days where mass balance is above/below zero at all elevations (for example days with positive ablation at all elevations and no accumulation). The manuscript will be easier to understand if the term ELA is reserved only for annual timescales. The references to “daily” and “seasonal” ELAs does not seem to be necessary at all and can be avoided without any loss of clarity. Moreover, even using the concept of ELA at an annual timescale, the authors should explain the significant differences between the ELA they compute, and the common glaciological meaning of it, which is associated to a specific glacier, and influenced by mass advection, wind redistribution of snow, local topography, local winds, etc.

Summarizing, this manuscript has the potential to be an excellent piece of research, providing a very interesting contribution. While it could be published with minor revisions, I strongly suggest doing some major ones to address concerns regarding validation and the time interval over which the ELA was calculated.

2 SPECIFIC COMMENTS

Line 12

“during the period 1500 – 1848 CE” At this point, I would just say “during the LIA”, because the fact that you do not use the period just used to define the LIA (1500 – 1850 CE) is distracting, and the reason for this becomes clear much later, and it is not relevant in the abstract.

Line 17

The acronym for empirical orthogonal functions should be spelled out here as it is the first mention of it. Also, for “EOF1” one would understand that you are referring to the first EOF. Therefore, “first EOF1” would be redundant.

Line 43

Include a short sentence saying what the large-scale estimate by Neukom et al. (2014) suggests. Does it show a temperature anomaly in the MA during the LIA period?

Line 95

"portion" suggests that you are doing a sub-diurnal analysis and that you compute the fraction of the total daily precipitation falling as snow. However, Eq. 2 suggests that you either consider all or none of the daily precipitation as snow.

Line 106

The authors should say here that G(t) is not a variable available directly as a GCM model output, and it is instead estimated using the parametrization by Annandale et al. (2002), that is based on the minimum and maximum daily temperatures, as well as the relative position to the Sun. Refer to Appendix A1 for more details.

Line 107

Although it is clear what you mean by snowpack here, the terminology is vague and not consistent throughout the manuscript. I would suggest referring to this consistently as firn.

Line 107

You should add here that the actual albedo is calculated following Oerlemans and Knap (1998), a method that takes into account snow age and snow depth, and refer to Appendix A2 for details.

Line 110

Who obtained those factors? Pellicciotti? (be explicit). What do you mean by "based". Is that the average? How different are the values at Juncal Norte and San Francisco? Give a sense of the variability of those factors along the MA.

Line 112

How similar?

Line 115

Eq. 3 say $T_{mean} > T_{crit}$, and here you say $T_{mean} > 0$. If the T_{crit} of Eq.2 and Eq.3 refer to different parameters, please use a different symbol. Or just write $T_{mean} > 0$ in Eq.3

Line 125

"GCMs based on past1000 experiment simulations (runs r1i1p1) of the CMIP5 initiative". Explain what the "past1000" refers to, and give some detail of those CMIP5 runs. Otherwise, the wording in this sentence is confusing.

Line 134

You refer to “this period” before actually defining it. Then define it in the following sentence. Please rephrase.

Line 135

You said in line 131 that “specific grid point information [of the GCM I guess] was used to compute temperature and precipitation lapse rates”. However, here you say that you use a standard and constant lapse rate for temperature. And in line 139 you say that you also use a constant lapse rate for precipitation. Therefore, the statement in this line is misleading.

Line 136

You have not said why you need maximum and minimum temperature; they do not show up on any equations. This ambiguity will be solved if you introduce the suggestion made for line 106.

Line 140

In general, there is no such thing as a “daily ELA” or “winter ELA” or “summer ELA”, see general comments. Nevertheless, this daily and seasonal values seem irrelevant as they are not used anywhere in the results or discussion.

Line 149

Is it well established that the first EOF of the SST captures the ENSO signal? If so, give a reference at least.

Line 150

Be consistent. PC1 or EOF1. Use either PC or EOF nomenclature.

Line 176

Again, it has not been explained why you need min/max temperatures to compute the ELA. See comments on line 136 and 106.

Line 177

It says “period 1979 – 2015” but Figure A1 says 1979-2016.

Line 186

How is the range of modeled ELA calculated? How do you estimate the uncertainty?

Line 192

This section is confusing, especially at this sentence. It should be more explicit that the authors are testing the ELA calculation method using a completely different dataset than the one used during the LIA period. Then, it is unclear the relevance of this comparison between the Carrasco ELA and the authors mean LIA ELA.

Line 194

General comment to section 3.2: Is this the most appropriate way to compare/validate? Arguably, it would be more interesting to see how these models reproduce the inter-annual and decadal variability of YESO station.

Line 196

“quite well” is too succinct and not substantiated. NCAR seems, and MRI seems to overestimate summer temperatures significantly. Also, Figure 2 excessively aggregates the data. If we are looking for anomalies in a time series, it would be better to see the time series of El Yeso and the GCMs between 1979 and 2015, not just monthly means.

Line 207

General comment to section 3.3: The discussion of the difference between model’s mean ELAs does not contribute much. The periods of high/low ELA are interesting, and it would be helpful if figure 3 is modified to show these periods better. Maybe a smoothed version of the data can be presented in all panels (not just panel d), and vertical lines can be incorporated to highlight the periods you talk about here.

Line 223

These “significant and common periodicities” were not obvious to me in figure 4. It would be interesting to highlight those period intervals in figure 4. Only the two year periods seem to be common to all models and maybe something around five years. A log-scale in the X-axis of figure 4 might help the visualization.

Line 226

They look quite different to me.

Line 236

“EOF1 retained”. Maybe “explained” is better terminology.

Line 236

“of the total variance” of what?

Line 237

The authors again talk about a sub-annual ELA, which is confusing. See general comments.

Line 240

What about the mismatch in timing of the periods with joint periodicities? The authors also talk about periods in-phase and anti-phase, which is quite confusing, and they do not address this later.

Line 261

Where was this comparison on the results? It does not seem to be there. Or are the authors talking about the 1979-2010 comparison of figure 2? The latter is what line 263 seem to suggest.

Line 271

This comparison would be much more useful if the authors present the calculation of

the present ELA using the past1000 runs.

Line 279

The authors can not know if there was a lower/higher ELA during the LIA compared with the second half of the past millennium because they only computed the ELA during the LIA. To know if there was a climate anomaly that could generate a glacier advance in the MA during the LIA, they need to compute the ELA over the whole past1000 data range, or at least in a range that extends beyond the LIA. With that information, they could see how the ELA during the LIA compares with the ELA before and after it, and with the prediction for the present (using past1000 runs data).

Line 292

Again, it would be nice to have those periods clearly highlighted in figure A4. Otherwise, it is difficult to see what the authors mentioned here.

Line 299

Shouldn't this be in the results?

Line 313

It seems advisable to replace "values" with "anomalies" or "departures". Otherwise, the authors would be suggesting that there is such a thing as "negative precipitation".

Line 316

Where did the authors observed that? There is no such data in the results.

Line 327

Statement a bit too strong for the data support. For MRI-CGCM3 there does not seem to be a significant relationship.

Line 329

Perhaps it would be better to say "might mask". If the authors say "seems to mask", they should explain better why it seems that way.

Line 331

Consistent, but one would not expect to see such a quick response to climate. So better than saying "low ELA values around 1840" they could say it in the same way they do in the following paragraph: "low ELA between 1800-1848". The very low ELA they found around 1820 might have more to do with that maximum advance than the low ELA around 1840. Therefore, to associate the advance right away with the whole 1800-1848 period makes more sense.

Line 334

However, the authors can not say if it was colder than the centuries before or after that interval (1500-1848 CE), which is also very relevant for the discussion and the comparison with the northern hemisphere. A paragraph like this is missing in the conclusions.

Line 341

Again referring to a "daily ELA", see general comments.

Line 360

It would be very illustrative and helpful to have a figure with the mean time series of SST over the El Niño 3.4 region (and some visualization of the EOF1 of it) alongside the ELA derived from each model.

Line 385

"R_a for T_t". Perhaps "times" is a better term than "for" here.

Line 387

T_{max} should be T_{min}

Line 391

Why not use the elevation in the range 100-6000 m used for MB calculation? Please justify this choice.

Line 398 (Eq. A8)

It would be better to use α_{ice} instead of α_{hielo}

Line 399

What does "global" mean in this context? Is it just the actual surface albedo?

Figure 5

Explain better how to interpret the arrows. The explanation in the caption is binary (in phase or anti-phase). However, the arrows can be seen in all directions. Do they display the angular phase difference?

Figure 6

It would be nice to have a box showing the El Niño 3.4 region as well as the MA.

Figure A1

What the do the bar sizes, error bars and green circles mean?

3 TECHNICAL CORRECTIONS

Line 57: No brackets around SST

Line 66: First sentence of the line is redundant.

Line 111: I would recommend writing "90×10^{-4} (mm h^{-1} C)" instead of "90 (mm h^{-1} C) $\times 10^{-4}$"

Line 114. Change "Appendix section" by "Appendix A"

Line 122: studies CARRIED out by Kinnard...

Line 147: Hyphen on long-term seem to have extra spaces or be a long hyphen.

Line 250: "MRI-ESM-P", I think it should be "MPI-ESM-P"

Line 260: "in terms to" might better be "in terms of"

Line 399: Word "global" is repeated

Line 402: "firm" should be "firn"

Figure 3: "$\pm$standarddeviationcalculatedfortheperiod". Better put it in words.

Figure 4: The areas beyond ARE shown as a lighter shade.

Figure 5: I guess that SSTA should be just SST

Figure A4: variability containED in the total regional precipitation

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