Response to Anonymous Referee #1

In this letter we try to briefly comment all the observations from Rev#1. The major changes can be read in the reviewed version of the manuscript and supplementary information (if a review is requested by the Editor).

Rev#1: In this paper, the authors use a novel proxy, based on long chain diols (LCDs), to reconstruct temperatures for the last 1500 years in an alpine lake from the southern Iberian Peninsula. Based on the reconstructed temperatures, the authors discuss the effect of greenhouse gasses and other climate affecting factors on the temperatures in alpine areas in SW Europe, and make predictions on what the temperatures and effects will be like at the end of the 21st century. The authors present an interesting dataset from an exciting area. To the best of my knowledge, this is the first time LCDs have been used for temperature reconstruction in a freshwater environment, and the results are promising.

Thank you very much for your comments.

Rev#1: However, since this is the first application of LCDs as a freshwater temperature proxy, I would expect a more thorough discussion on this application, also because Rampen et al. (2014a) were critical in their study on the application of LCDs, and in particular on the Long chain Diol Index (LDI), as a freshwater temperature proxy. In their marine LCD study, Rampen et al. (2012) observed a positive correlation between temperatures and the fractional abundances of C30 1,15-diol, a negative correlation between temperatures and the fractional abundances of C28 and C30 1,13-diol, and no correlation between temperatures and the fractional abundances of C32 1,15- diol. Moreover, they observed that the fractional abundance of C32 1,15-diol remained below 0.3 for most of the marine sediments. Based on those results, Rampen et al. (2012) introduced the LDI, with a stronger SST correlation compared to the fractional abundances of the individual LCDs. Assuming the LDI is correlated with temperature in the studied lake Lago de Rio Seco (LdRS), the fractional abundances of the 1,13-diols do show negative correlations with temperature, but both the variation and the temperature correlation for C30 1,15-diol is extremely low, whereas the C32 1,15-diol does show a significant correlation over a wide range of fractional abundances. This means that the rationale behind the LDI in marine environments may not apply for LdRS. As a result, I would recommend to (also) test other LCD indices that include the C32 1,15-diol and/or multilinear regression analysis.

We agree, and in the new version of the manuscript we have included a discussion about the LDC distribution in both LdRS sediment cores and we have tried different approaches for the LCDs-temperature calibration (based on multiple regressions, C32 1,15-diols, etc.: new supplementary information). We have also included samples of the long core (from 1908 to 2008) in the calibration to make it stronger (see a full explanation in Rev#1 comment 10). Rev#1 will see in the new supplementary information of the paper that the different equations do show very good correlations with temperature, which support our interpretation about the relationship of LCDs and temperatures in this alpine lake. We believe that we have discussed this matter deeply in this new version of the manuscript.

Regarding the potential use of the C30 1,15-diol in the calibration equation, we would like to mention that the potential relationship between LCDs and lake temperatures is discussed in the paper form Rampen et al., (2014a), who also described the scarce correlation between C30 1,15-diol and both mean annual temperatures and GDGT-derived temperatures. However, this isomer was eventually used in a multilinear regression equation along with C28 and C30 1,13-diols, and C30 1,15-diol, and even in the LDI calibration equation, showing good correlation in both cases (r^2>0.64 once one outlier was removed).

Rev#1: In addition, it seems like the correlations between the LDI and the fractional abundances of the individual LCDs are statistically different for the two sediment cores. With the exception for C30 1,13-diol, the slopes for the fractional-abundance-of-individual diols from the 2 different cores differ significantly, when plotted vs the LDI. The calibration of the LDI seems to be based on samples from 1908 and younger, and only using those samples obtained from the short core. Almost all of the fractional abundances of the 1,13-diols in the older sediments are higher, and almost all of the fractional abundances of the C32 1,15-diol in the older sediments are lower. As a result, the reconstructed temperatures before 1908 are a result of extrapolation of the dataset, and one might even argue that the LCD distribution was significantly different in the samples before 1908. In particular the C28 1,13-diol and C32 1,15-diol values show very different trends between the short and the long core for the overlapping time-period - the long core shows much larger ranges of values, something not mentioned in the manuscript. The different LCD distributions in the two cores for the overlapping period raises questions if a calibration, only based on samples from the short core, is applicable for the long core. What could possibly explain the (significant) differences between the two cores? Why were samples from the long core not included in the calibration, even though a number of samples fall in the time-period for which temperatures are available? For these reasons, I consider it questionable if the authors provide sufficient support for the use of the LDI (or LCDs in general) for the temperature reconstruction in this study. To me, a better and more critical discussion seems crucial for this paper, also because the authors never seem to question their results and don't refrain from making some very strong statements, based on these results.

The fractional abundances of the different isomers vary throughout time because they are supposed to be related to changing environmental variables such as temperature. This may be one explanation for the comment "Almost all of the fractional abundances of the 1,13-diols in the older sediments are higher, and almost all of the fractional abundances of the C32 1,15-diol in the older sediments are lower".

Regarding the potential LCD differences between both cores, samples from both cores have different time averaging, and therefore fractional abundances for the same specific

age (with different time averaging depending on the core) might be slightly different in each core. The location of the cores in different areas of the lake could also affect the registered LCD abundances. The use of ratios (i.e. LDI) in the calibrations may partially solve this issue. Anyway, it is worth mentioning that individual LCDs for the overlapping period in both cores show the same general trends. The comparison of the slopes is not the best way to compare both data sets, especially when the slopes are calculated from scarce samples, as it is the case of the overlapping period for the long core. Data from both cores can be compared using a Pearson correlation, but these data have to be converted to the same time-averaged age in order to perform a correlation between individual LCDs. In this case, the Pearson correlation (r) between both cores for the calibrated time period (1908-2008) is: 0.93 (C28 1,13-diol), 0.91 (C30 1,13-diol), 0.81 (C30 1,15-diol), and 0.91 (C32 1,15-diol), no p value is provided due to the low number of samples (n=7). If we consider the whole overlapping period, the Pearson correlation (r) between both cores is: 0.86 (C28 1,13-diol), 0.81 (C30 1,13-diol), 0.90 (C30 1,15-diol), and 0.89 (C32 1,15-diol) (n=10).
Another concern of Rev#1 was: "*The different LCD distributions in the two cores for the overlapping period raises questions if a calibration, only based on samples from the short core, is applicable for the long core.*", and to fix this issue we have included the long core data in a new calibration, which is base in a total of 26 samples (short + long core samples). Both, the slope and the r^2 are pretty similar to those of the previous calibration (only using the 19 short core samples). Although the calibration is performed in down-core samples (from 2008 to 1908), the application of this new calibration is not an extrapolation of the dataset since the correlations are performed between diol indices and temperature data, without a time or depth constrain. Actually, temperatures just before 1900 slightly increased, even though the down-core trend from ~1950 to 1908 shows a decreasing temperature trend.

We thank Rev#1 for these comments that have helped us to improve the diol discussion in the manuscript.

Rev#1: Nit-picking and other comments.

1. **Rev#1:** The title says "Extreme warming rates affecting alpine areas. . ." However, is it the extreme warming rates, or is it the extreme warming itself, that affect the alpine areas?

Changes in the local flora and fauna in these ecosystems (Menéndez et al., 2014) are actually affected by both the extreme warming that is causing an enhanced melting of the permafrost and seasonal snow / ice, and the extreme rates of warming (i.e. almost 0.2 ºC/decade, which is higher than the global one according to the IPCC, 2013 of ~0.06 ºC/decade).

2. **Rev#1:** Lines 81-82. Rodrigo-Gámiz et al. (2015) tested the use of the LDI, but did not apply it as a temperature proxy - they did not perform climate reconstructions. Rampen et al. (2014b) also tested the applicability of long chain

diols as temperature proxies without applying it for climate reconstruction. Rampen et al. (2014b) tested different indices than the LDI - indices based on 1,14-diols. AND Lines 82-83. Rampen et al. (2014a) tested the applicability of the LDI in freshwater environments, but did not use this proxy for climate reconstruction - to the best of my knowledge, no-one has published the use of LCDs for temperature reconstruction in freshwater environments so far.

We totally agree with Rev#1, and we have re-phrased the sentence: *"Other promising algal-lipid biomarkers, the long-chain diols (LCDs), have also been assessed as temperature proxy in marine environments (Rampen et al., 2014b; Rampen et al., 2012; Rodrigo-Gámiz et al., 2014; Rodrigo-Gámiz et al., 2015); however, these biomarkers have only been tentatively tested in freshwater records as temperature proxies (Rampen et al., 2014a)."*

3. **Rev#1:** Line 86. In my opinion, Rampen et al. (EPSL 276, p. 207-213, 2008) and/or Willmott et al. (Antarctic Science 22. P. 3-10, 2010) would be better references than De Bar et al. (2016), as they introduced and first applied the indices also used by De Bar et al. (2016) and others.

Thanks for the suggestion - these references have been changed.

4. **Rev#1:** Lines 88-91. Are the authors specifically referring to paleoenvironmental reconstructions in freshwater environments here? Otherwise, I think the text and selected references do not do justice to the number of LCD studies that appeared recently. Rampen et al. (2014a) did not apply LCDs for palaeoenvironmental reconstruction - they only tested the applicability of LCDs in freshwater environments.

That is right. We referred to paleoenvironmental reconstruction in lakes. We have rephrased the sentence: "*Nevertheless, only a few studies have tested them as lacustrine archives of paleoproductivity (Shimokawara et al., 2010), past rainfall anomalies (Romero-Viana et al., 2012), or temperatures (Rampen et al., 2014a), among others.*"

5. **Rev#1:** Line 101. It is incorrect to state that the LDI has only been calibrated with other indirect temperature proxies - Rampen et al. (2014a) also correlated the LDI with annual mean air temperatures obtained from climate observation stations nearby the various lakes that were studied (e.g. see Fig. 5 in their paper).

Thank you for the remark. We have rephrased the sentence, including not only the LDI: *"The application of LCDs as a temperature proxy is novel in freshwater environments and only two preliminary calibrations have been obtained from recent surface sediments using both mean annual temperatures from weather stations and another indirect summer temperature proxies (based on GDGT distributions) (Rampen et al., 2014a)."*

6. **Rev#1:** Lines 102-104. Unless they provide reasons to believe otherwise, the authors should emphasize that for now, their calibration is only applicable for LdRS.

We agree. However, this calibration although "local" could be applied to other alpine lakes in the Sierra Nevada, since alpine lakes in this area are pretty close and have similar algae communities (Barea-Arco et al., 2001; Sánchez-Castillo, 1988; Sánchez-Castillo et al., 1989). We have rephrased the paragraph: "*Although this calibration can be only applied to LdRS record, and probably to some of the other alpine lakes in the Sierra Nevada area, these new data support and reinforce the use LCDs as a paleotemperature proxy in freshwater environments.*"

7. **Rev#1:** Lines 170-173. The difference in the sedimentation rates above 16cm (0.13-0.9 cm/yr) and below (0.008 cm/yr) seems large and relevant to me. I think the reason for this change in sediment rates, and the possible effects for this study, should be discussed.

Thank you for this comment, which made us notice that the 0.9cm/yr sedimentation rate was a typo. Actually, the sedimentary rate is between 0.09 and 0.13 cm/yr. We have corrected the typo. In any case, the main reason of a ~10 times increase in the sedimentation rate (from 0.008 to more than 0.09) is mainly due to meltings during the last stages of the LIA and post-LIA, as well as human activities (i.e. construction of pathways, refuges) in the alpines areas of Sierra Nevada during the 19th century (García Montoro et al., 2016; Titos Martínez, 2019; Titos Martínez and Ramos Lafuente, 2016) that intensified after the 40s of the 20th century (Jiménez et al., 2015). Since the catchment basin of the lake is bare, with only few patches of vegetation surrounding the main water body (no nutrient supply), erosion mostly provided inorganic material (mica-schist and clays). The main effect of the high sedimentation rate on the algal community was the dilution of algal compounds such as chlorophylls and labile carotenoids, not affecting the relationship of these pigments with temperatures (Jiménez et al., 2015). We have explained so in the last part of the Introduction (new lines 141-148).

8. **Rev#1:** Lines 264-267. Isn't this an indication that the LDI calibration from this study cannot be directly applied for other LCD studies?

We agree and dealt with this comment in Rev#1 comment 6, but included a further new sentence in this paragraph remarking so: "*Therefore, the outcomes of this paper (i.e. temperature calibration) should not be generally applied to other LCD records unless they show a similar isomer distribution.* "

9. **Rev#1:** Lines 274-276. It is unclear why a correlation between the LDI and the abundance of Chrysophyceae cysts would be an indication that these algae could be the source of the LCDs; to me, this only seems to indicate that Chrysophyceae are more abundant during warmer periods. The LDI is a ratio between various LCDs and should be independent from the abundance of their source organisms, unless LCDs have multiple sources, and specific LCDs are produced by specific organisms. It would be more relevant if a correlation between the absolute abundances of LCDs and algal numbers was observed.

We mentioned this hypothesis as an alternative to the potential biological sources since Eustigmatophyceae algae have not been identified in the alpine lakes of Sierra Nevada.

However, both reviewers were concerned about the cyst and LDI relationship and therefore we opted to remove this sentence (former lines 274-276) and figure (Figure S3) from the manuscript. Further molecular and sediment traps studies (currently in progress) would be also required for this statement.

10. **Rev#1:** Lines 297-299. Why is the calibration only based on LCD data from the short core? How was dealt with the fact that samples may contain a signal collected over several years - how was the instrumental temperature selected?

We thought that samples from the same core would be better for a downcore calibration. However, Rev1# is right and including the samples from long core would mean more control points. Therefore, this time we have included 7 samples (from 2006 to 1908) from the long core in the calibration, and our new calibration is based on a total of 26 samples (see second comment for details).

Samples used in the calibration have a time averaging between 5 and 7 years; thus a mean of the historical temperatures covering the time averaging of each sample was done in order to have a mean temperature of the specific time-averaging of each sample. We had explained the issue in the captions of Figs., 4 and 5 i.e.: "*Solid dots represent the same time averaging as the LDI data in LdRS lgc*". However, we have also clarified this in the material and methods section.

The selection of the temperature time-series is explained in detail in the material and methods section *2.3 Reference temperature time-series for LDI temperature calibration*, taking into account the longest and more reliable temperature time-series in the area.

11. **Rev#1:** I'm not convinced it is correct that only one regression analysis is performed in which, for each sediment sample, four different temperatures are included; every sample appears four times in this regression analysis. I think it would be better to perform four different regression analyses; one for each of the four reference temperature series.

Yes, we agree and we performed all the possible combinations: a) one regression including the 4-temperature time-series vs. b) individual regressions for each obtained temperature time series. Eventually the result (in ºC) when the mean value of the four-LDI temperature was obtained was the same in a) and b). We chose one regression with the 4-temperature data for each point since we can assess the global residuals of the equation; and, therefore the global errors in one graphic; otherwise we would get four individual records of residuals and errors that make more complex the assessment of the model as a whole. In addition, with option b) we would have four calibration equations, whereas with option a) we have one calibration equation summarizing all the potential temperature reconstructions. We think that this is the best summary temperature data for a single calibration equation.

12. **Rev#1:** It would be useful if the reference temperature data was also provided, for example in table S7. I would like to see the instrumental temperatures in a figure, for example in figure 5.

Yes, we have included the mean instrumental temperature obtained from our four simulations in Fig. 4 (former Fig. 5). As well as in Supplementary table S7.

13. **Rev#1:** Line 308. There are too many decimals indicated in this equation. Also, there are 19 samples (n=19) used for the calibration, not 76.

We have reduced the decimals and clarified the "n issue" in the text: There are now 26 samples (including those from the long core) and four temperature time-series: *"(n=26x4; R^2= 0.81)**

** n=26 sediment samples plotted against the four temperature simulations providing a total of 104 combinations."*

14. **Rev#1:** Line 319. As indicated above, I would start the discussion with a critical look at the LCD data.

We have included a general discussion about the distribution of the different LCDs obtained and their potential temperature relationships (first section in the discussion).

15. **Rev#1:** Line 346. What about the prominent warming observed in the LDI around 1830, which is not registered in other records?

This warming follows the trend observed during the 18th century, and is related to the latest stages of the LIA. The study record comes from an alpine area, which is highly sensitive and may record this warming stronger than in other areas. In any case, the comparison with other records depends on the scale and time averaging. For example, the time averaging is high when comparing other records with the long core, which could prevent us from identifying specific events. However, looking carefully at this period (from ~1820 to 1840) in the high-resolution record of the short core (current Fig. 4), one can observe that the tree ring CPS MSTA record also registered this warming in the 1830s decade. In this paragraph we were describing the major trends for the last 1500 years and we did not pay attention in describing that in detail. However, this warming in the latest stages of the LIA was mentioned in section 4.3. In any case, we have tried to clarify this, and we have added a sentence in the paragraph that Rev#1 mentioned: "*However, the warming in the latest stages of the LIA is more pronounced in the LdRS record."*

16. **Rev#1:** Line 441. I really don't think the resolution of the LCD record for the LIA is high enough to identify 'events'.

Rew#1 is partially right, we do not have enough sample resolution in the long core to perfectly track these events; however, the LdRS long core data during the LIA do not only represent specific temporal data, but time averaged samples. There is a mean time-averaging of ~87 years in each sample and thus we are not recording a specific moment, but a time-averaged period, where we see these temperature drops agreeing with periods of solar minima. Anyway, perhaps the word "correspond" in the text has too many connotations and we have rephrased the sentence, specifying that they are coeval only:

"Most of the above-mentioned cooling events recorded in LdRS, such as those during the LIA, are coeval with low solar activity periods…"

17. **Rev#1:** Lines 477-479. I think that three sample points in the period between 1690 to 1850 are insufficient to determine the warming rate for that time-period. ". . . a low sample density for the LIA, which might slightly increase the uncertainty for this period. . ." in lines 482-483 seems like a strong understatement to me; I would refrain from making statements based on this warming rate.

That is also partially right. Samples in the long core for the LIA have a time-averaging of ~87 years. Therefore, samples do not provide information about a specific moment, but a time-averaged period, what is more representative than a snapshot of a specific moment. Therefore, in this new version we have also included samples from the short core for the first half of the 19th century (5 more samples). We have explained these new data in the text.

18. **Rev#1:** Line 488. To me, it is not clear why slower warming rates in other European alpine areas are indicated as "An even more alarming result". How do the slower warming rates in other areas affect the Sierra Nevada?

We wanted to explain that it is alarming that Sierra Nevada had higher warming rates than the Alps during that period. We agree that the phrasing did not transmit this message. We thus have removed "An even more alarming result" in the revised version.

19. **Rev#1:** Lines 497-498. I don't see how the 'limited' LCD data in this study can be directly applied for an extrapolation to predict temperatures for 100 years in the future. I don't think this is the correct way to make such statements; I believe climate prediction is a very complex study area, and the simplification demonstrated here is almost offensive to that particular field of research. In contrast to this simple extrapolation of a trend observed over the last century, one can also claim that in 100 years the temperatures will be more than 3 ºC cooler than now, as demonstrated by the trend that started at the beginning of the 21st century (like the warming rate for the last stages of the LIA, the cooling rate for the 21st century of 0.32 ºC/decade is based on 3 data points). Climate predictions should not be that simple. The lack of restraint to make such extrapolations, the lack of research to test these predictions, the lack of additional information (other studies) about climate predictions, and the lack of restraint to predict the effects of the possible future warming in a very vague and yet very alarmist way, without providing any other type of support, is not correct. In my opinion, the text between lines 497-519 should be removed. In a way, it also affects the credibility for the rest of this paper.

We have removed this issue from the discussion, and added some sentences and references about published temperature projections in Sierra Nevada at ~1000 masl. We have also explained the lack of these kind of projections in the alpine areas of the region, and the potential use of the new data in the assessment of future scenarios.

20. **Rev#1:** Fig. 7c does not show that temperatures may rise at least 1.4 ºC (strange annotation, 'at least' combined with _) by the end of the 21st century.

We have removed this paragraph. See also comment Rev#1 19.

21. **Rev#1:** Lines 568-571. As indicated before, this is not the first study in which LCD temperatures were calibrated with instrumental data. This has already been done by Rampen et al. (2014a).

Rev#1 is right. We have changed the sentence clarifying that this is the first study using instrumental temperature time-series.

22. **Rev#1:** Line 592. According to figure 5c, there is no such thing as an abrupt temperature increase in 1950s; if anything, the warming trend briefly flattened during the 1950s. The warming started around _1900 and continued until _2000?

Rev#1 is right. We only meant that alpine temperatures of southern Iberia exceeded the highest temperature scores reached during pre-industrial times in the 1950s. We have changed the sentence accordingly.

23. **Rev#1:** Figure 2b and c. It is unclear to me why these two figures cannot be combined in one. The lines and data points are exactly the same, the only difference is the scale on the y-axis.

We agree and we have combined both figures.

24. **Rev#1:** Figure 5. I don't understand how some of the linearly interpolated data points can deviate that much from the original dataset. This is most clearly visible in 5a.

Dotted lines are only the lines connecting the original data points in Figs 4 and 5. The different environmental records have been lineally interpolated and time-averaged to the same intervals as the studied cores (dots/points) in order to properly compare them with LdRS data. For example, data of different records within the time interval 1942-1948 (as defined in the short core), have been time averaged (to the period 1942-1948) and assigned to the same age (i.e. 1948) to facilitate the correlation among them. Although this might create a slight offset in age with the original data, it is the best way to correlate all the records with the same time-averaged-intervals as the studied cores of LdRS.

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