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Interactive comment on "Extreme warming rates affecting alpine areas in SW Europe deduced from algal lipids" *by* Antonio García-Alix et al.

Anonymous Referee #1

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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.

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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. However, since this is the first application of LCDs as a freshwater temperature proxy, I would expect a more

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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.

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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.

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.

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Nit-picking and other comments. .

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?

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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.

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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.

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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.

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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.

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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)

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Lines 102-104. Unless they provide reasons to believe otherwise, the authors should

emphasize that for now, their calibration is only applicable for LdRS.

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Lines 170-173. The difference in the sedimentation rates above 16cm (0.13-0.9 cm/yr) and below ($\sim$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.

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

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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.

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

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.

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.

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

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Line 319. As indicated above, I would start the discussion with a critical look at the LCD data.

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Line 346. What about the prominent warming observed in the LDI around 1830, which is not registered in other records?

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Line 441. I really don't think the resolution of the LCD record for the LIA is high enough to identify 'events'.

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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.

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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?

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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 °C 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 °C/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.

Fig. 7c does not show that temperatures may rise at least ∼1.4 °C (strange annotation, 'at least' combined with ∼) by the end of the 21st century.

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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).

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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?

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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.

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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.

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