Clim. Past Discuss.,
https://doi.org/10.5194/cp-2019-147-RC2, 2020

Climate of the Past Discussions

Interactive comment on "Volcanism and climate change as drivers in Holocene depositional dynamic of Laguna del Maule (Andes of central Chile – 36° S)" *by* Matías Frugone-Álvarez et al.

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Received and published: 18 February 2020

General comments

Frugone-Álvarez et al. present a thorough, multidisciplinary study on multiple sediment cores from the Laguna del Maule (LdM) lake in the Chilean Andes. The paper is rich in new multi-proxy data (Chronology, Stratigraphy, Bathymetry, Seismic, Sediment description, Tephra and Sediment micro-XRF, pollen) and extensive supplementary material building on previous investigation of a shorter record. The integration of these datasets and regional comparisons are used to derive large scale atmospheric and hydroclimatic changes in the Holocene of South America. The figures are detailed

and support the manuscript well. This paper contributes to closing the gap of our understanding of environmental and climatic changes in the Southern Hemisphere and is very suitable for Climate of the Past. I recommend publication after some minor revisions.

I have outlined some comments below. Technical corrections/suggestions on the main text and the supplementary material are attached as pdfs with comments.

Specific comments

I complement the authors on citing R packages but version numbers (on packages and R itself) should be included too.

I would really like to see the data presented in this paper shared in an online repository. "The data from proxies and facies are available from the authors upon request." is unsatisfactory and does not ensure data availability for the long-term future.

Chronology

I want to complement the authors on incorporating age uncertainty in the plots of proxy data (Figs. 7,8) and the discussion as well as considering the short-comings of the current age model.

It is unfortunate that no tephrochronology could be established, especially in such a volcanically active region. Tephrochronology has a high potential to strengthen the chronology but also to modify it with possible changes in the interpretation, especially considering the differences between the Singer et al 2008 36Cl ages and the presented 14C age model. I would suggest to highlight that the main conclusions of the paper (as the focus lies on the Mid to Late Holocene) are independent of this discrepancy to avoid discrediting the interpretation.

The age model plot in Fig. 5 needs to show the panels with iterations, accumulation rate and memory, which are included in the default output from Bacon and hold important information for the chronology development.

Why was a prior of 80 a/cm chosen as a prior for the accumulation rate? An approximate estimate of the ca. 260 cm in the LdM sequence without layers of instantaneous deposition and almost 14 ka would suggest something closer to 50 a/cm. How does the posterior distribution look (see panels in age model plot by Bacon, comment above)?

And why was a segment length of 4 cm ("thick") chosen? I understand this value is rather arbitrary but can have quite some influence on the resulting chronology. This should be discussed/acknowledged, or at least the information of those extra panels provided by Bacon should be included in Fig. 5 to be able to judge the performance of the age model.

Micro-XRF data

It is not always clear if log ratio transformed micro XRF data are used for the subsequent statistical analyses, simple ratios or raw data (see also comment in the pdf regarding ln(x) or ln(x/y) or centralised log ratio). Please double check as that may impact the results/interpretation.

Line 116-117. How was it decided to use a cut off value of 1000cps for elements to be excluded from the dataset? I imagine this has a significant influence on the interpretations, especially since some interesting elements are excluded this way. In this context, I did not understand clearly whether the volcanic facies (tephra and lapilli) and LT layers are included in the calculation of the mean. If yes, this surely favours some elements in a potentially dubious way.

Line 250 Clastic-related elements in the first eigenvector are explained mostly by silicates from the volcanic watershed. If I understand correctly, the volcanic facies were excluded, so this refers to reworked volcanic material in the other facies? But why is Si not dominant (according to the listed elements) if the detrital signal of the first eigenvector (which I agree with) is to be explained by silicates?

I am curious about the calibration between ICP-OES and micro-XRF samples. How

did the authors ensure that the correct points were compared with each other? Are the discrete samples scanned or how does one know that a discrete sample (of which thickness? Same as the micro-XRF resolution?) matches exactly with a specific scanning step? However, this is not very important (in the context of the paper) as I do not see where the calibrated fully quantitative data are used instead of just the semi-quantitative XRF core scanning data.

Volcanism

Line 541 Volcanic/seismic activity are used interchangeably. Is there any chance the authors could discriminate between the triggers?

Does the inferred change in magma composition in the Late Holocene have any impact on the depositional dynamic of LdM, the climate or societies (given the topic of the special issue this is included in)?

Technical corrections

The supplementary material has two figures named "S5" resulting in wrong references and wrong numbers for figures S6-S13. I may not have marked all occurrences of this, please double check.

Further technical corrections are marked in the attached pdf of the manuscript and supplementary material.

Please also note the supplement to this comment:
https://www.clim-past-discuss.net/cp-2019-147/cp-2019-147-RC2-supplement.zip

Interactive comment on Clim. Past Discuss., https://doi.org/10.5194/cp-2019-147, 2020.