

Interactive comment on “A Late Quaternary climate record based on long chain diol proxies from the Chilean margin” by Marijke W. de Bar et al.

Anonymous Referee #1

Received and published: 12 September 2018

1. Does the paper address relevant scientific questions within the scope of CP? YES
2. Does the paper present novel concepts, ideas, tools, or data? Data are novel. Concepts, ideas and tool have been in several earlier publications.
3. Are substantial conclusions reached? YES
4. Are the scientific methods and assumptions valid and clearly outlined? The scientific method is clearly outlined. Most of the assumptions are properly outlined.
5. Are the results sufficient to support the interpretations and conclusions? Yes (in general). Some conclusions need to be re-considered (see detailed review)
6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? YES
7. Do the authors give proper credit to related work and clearly indicate their

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own new/original contribution? YES 8. Does the title clearly reflect the contents of the paper? YES 9. Does the abstract provide a concise and complete summary? YES 10. Is the overall presentation well structured and clear? Mostly 11. Is the language fluent and precise? It could win from a revision by a native speaker before resubmission. 12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? YES 13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? The Discussion needs clarification 14. Are the number and quality of references appropriate? YES 15. Is the amount and quality of supplementary material appropriate?

Comments:

de Bar and co-authors use biomarkers (long chain diols, TEX86 and UK $\delta^{13}C$) to reconstruct sea temperature variations in the river-influenced upwelling ecosystem off southern Chile during the past 150,000 years. They also compare the Diol Index and the nutrient dial index with other paleoproductivity indicators, including bulk organic matter total organic carbon (TOC), organic matter stable carbon isotopes ($\delta^{13}C$), as well as phytoplanktonic lipid biomarkers. The data set is interesting for a broad audience and the technical aspects of the manuscript are correct. In general, the manuscript is well organized. However, (i) there are several assumptions, which need strong re-thinking and (ii) the MS would have greatly benefited from a quick read by native speaker before submission (several sentences are convoluted and difficult to understand, more a grammar than a scientific problem.) Below I list major comments.

Abstract: all abbreviations should be first fully written. Readers less familiar with them have no clue what the authors are referring to.

Introduction:

I. 15-20: The statement “Proboscia diatoms grow in the early stages of upwelling when nutrients strongly increase in concentration (Koning et al., 2001)” is wrongly interpreted and does not support the authors’ interpretation that Proboscia is a diatom indicative of

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high productivity in the Chilean coastal upwelling system. If you keep reading Koning et al. (2001), these authors also mention that “The dominance of these pre-upwellers before the onset of the upwelling season was probably caused by their ability to adjust their buoyancy, which allows them to migrate to deeper levels below the euphotic zone to obtain the nutrients trapped there before the actual upwelling starts (Villareal, 1988).” Moreover, “The upwelling period was characterized by the successive dominance of three diatom species, *Th. nitzschoides*, *N. bicapitata* and *Chaetoceros* resting spores. *T. nitzschoides* dominated the assemblage in July, when the two-gyre upwelling system was firmly established, temperatures were the lowest and H_4SiO_4 concentrations in the surface waters were high”.

Specimens of *Proboscia* spp. are hardly found in sed traps samples (Romero et al., 2001, *Deep-Sea Res.* 48, 2673), and in surface and/or downcore sediments along the Chilean margin, and have never been associated with high productivity along the Chilean margin (Romero and Hebbeln, 2003, *Mar. Micropal.* 48, 71; Mohtadi et al., 2004, *J. Quater. Sci.*, 19, 347; Romero et al., 2006, *Quat Res.* 65; Mohtadi et al., 2007, *Quaternary Sci. Rev.* 26, 1055).

P. 3, l. 13: “...several glacial and interglacials periods”. Several can be four, but can also be 15. Your study extends only the past 150 kyr, be more concrete.

Results

P. 9, l. 10-15: (i) “The average TOC content varies between 0.4 and 2.6%.”: average is not the same as range!; (ii) “The TOC content is significantly higher during the interglacial periods (MIS 1, 3 and 5) compared to glacial periods (MIS 2, 4 and 6)”: not quite true, values for MIS 3 are hardly distinguishable from MIS2 and 4. (iii) “During Termination 2...”: Terminations should be accordingly identified in Figs 3-5; (iv) “the TOC and TN contents increase rapidly (within < 1 kyr) towards interglacial values”: is the sampling resolution high enough to state that the increyse occurred within less than 1,000 years?

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P. 9, l. 16-20: very convoluted sentence. Revise.

P. 10, l. 11-25: much of this information is related to Methods. It should be placed accordingly.

P. 11, l. 26: see my comment above for the sampling resolution.

P. 11, l. 26-31: this needs more accurate description. Revise.

Discussion

P. 12, l. 20-32: this part of the Discussion is very intricate and unclear. Please rephrase.

P. 13, l. 5-10: why was no *Chaetoceros* peak during MIS4 when the MAR TOC was high?

P. 13, l. 17-18 & l. 25-26: since *Proboscia* is not a secondary component of diatom assemblages in coastal upwelling systems not it is not associated with high productive waters along the Chilean margin, these statements should be thoroughly revised. See my comments above for Introduction

P. 14, l. 2-5: This statement needs appropriate references/lab studies. Have different species of *Proboscia* been cultured the biomarker content measured in living cells?

P. 14, l. 7: It is not correct stating that “*P. alata* needs little Si to build its frustule”. For diatom standards, frustules of *Proboscia* are long and build long chains (see Jordan et al., 1991, *Diatom Research* 6, 63).

P. 14, l. 8-11: These lines contradict your own interpretation of *Proboscia* as a component of high productive waters diatom assemblages. The fact that *P. alata* is “often observed in high nutrient and/or upwelling regions” does not mean that this diatom is dominant nor it is a reliable proxy for high productivity.

P. 14, 4.3. Sea surface temperature evolution: the discussion in this section jumps

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back and forth between different time windows. This is not reader-friendly. Revise.

P. 14, l. 22-25: Does your SST record following “global climate pattern” refers to MIS5 or the entire record? Please clarify.

P. 15, l. 3: A correlation test helps to supports this statement.

P. 15, l. 7-8: This should be more rigorously discussed.

P. 15, l. 26-27: Looking at your Fig 7, several mismatches in the SST behavior of compared records are recognizable. This should be more critically and rigorously discussed (see for instance ODP1241 and GeoB3327-5 vs ODP1234).

The authors should comments and discussed on: - “The production/export depth of TEXH86 is not well constrained, thus complicating the comparison of TEXH and SST (for example, UK) based records.” (e.g., Kim et al., 2012, EPSL 339, 95-102.; Ho & Laepple, 2018, Nat. Geosc. 9, 606). - “glacial–interglacial amplitude of TEXH86-derived SST change in the tropics is overestimated relative to other proxy evidence, a result also independently found by a multi-proxy study in the subpolar region“ (Ho & Laepple, 2015, Earth Planet. Sci. Lett. 409, 15–22; Seki, O. et al. 2014. Prog. Oceanogr. 126, 254–266).

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