Interactive comment on "Long chain diols in settling particles in tropical oceans: insights into sources, seasonality and proxies" *by* Marijke W. de Bar et al.

Anonymous Referee #2

Received and published: 22 February 2019

General comments

In this study, de Bar et al. presented long-chain diol (LCD) data from five sites; three along a longitudinal transect in the tropical Atlantic, the Cariaco Basin and the Mozambique Channel. LCD derived indices, i.e. Long-chain Diol Index (LDI) and Diol Index, are used to reconstruct past SST and upwelling, respectively. These proxies are relatively new compared to those based on alkenones and GDGTs, thus have not been as well studied. This is where this study comes in. de Bar et al analyzed LCDs from sediment traps and underlying sediments. For the sites where alkenones and GDGT data do not yet exist, the authors also analyzed these biomarkers in addition to LCDs

to allow multi-proxy comparison for all the sites. The well-designed experiment thus allows the authors to investigate various aspects of the LCDs and their associated proxies, including the temporal evolution (seasons to years), settling processes, as well as comparison with other commonly applied biomarker proxies. The data presented by de Bar et al. generally show that LDI-derived temperatures agree within error with instrumental data in the Atlantic, albeit with different amplitude of change. At upwelling sites, the Diol Index seems to either record a pre-upwelling signal or show the same trend as in primary productivity.

The study fits the scope of Biogeosciences, and will also be of interest to readers from other community such as paleoclimate. The manuscript is generally well-written and accessible. I do, however, feel that some figures could be further improved for clarity. I find the "Results" section too long and some discussion unclear or not fully supported by the data, especially in section 4.3. Below are suggestions and comments that I hope will help the authors in further improving the manuscript. Once the concerns are addressed, I strongly recommend the publication of this manuscript.

Specific comments

#Line 34-36: Clunky sentence. Please rephrase.

#Line 43: "Conte 2006" should be "Conte et al 2006"

#Line 96-97: "ITCZ migrates southward during boreal winter" - would be useful to have this marked in Figure 1 too.

#Line 100: Insert abbreviation (SEC) after South Equatorial Current.

#Line 116: replace "/" with either a space or comma.

#Line 119: "as result" should be "as a result"

#Line 125: "latitudinal transect" is a transect across latitudes. What you have is a "longitudinal transect", i.e. with sites spanning longitudes at a fixed latitude (∼12°N).

#Line 183-184: Varved sediments have annual resolution. Since you mentioned "annually to decadally resolved climate records", do you mean "laminated sediments" instead?

#Line 224: "weight sub-aliquots" is confusing. Suggested rephrasing "sub-aliquots (by weight)".

#Line 237-238: Confusing sentence. Sounds like you analyzed both ketone and GDGT fractions by both GC and GC/MS - which is likely not the case. Please rephrase.

#Line 285-287: Technically this is a variant of the original BIT index proposed by Hopmans et al 2004. Please rephrase the paragraph to reflect this.

#Line 296: This is not the first time GC is mentioned in the manuscript. Spell out "gas chromatograph" at the first mention instead of here. Also, there is no need to define the abbreviation at each mention.

#Line 308-309: Tierney and Tingley (2018) is not the first to notice the warm-end limit of UK'37, i.e. an issue which has been in debate since the 90s. Please include the original references.

#Line 313: "gas chromatograph (GC)" see comment to Line 296.

#Line 314: "mass spectrometer (MS)" see comment to Line 296.

#Section 2.5 Time-series analysis: Since the result of the time-series analysis is not a main part of the results and discussion, I would suggest to either (A) remove this rather long section or (b) move it to the supplement and add a supplementary figure depicting the result (which is briefly discussed in the text but not shown).

#Section 3. Results: I had a hard time going through the 4-page long results section. Given the large data set spanning several sites and including several biomarkers and their associated proxies (for which I applaud the authors), this is perhaps inevitable. But I think that it will make the section more accessible for the reader if the authors

could reduce the text by 10 to 20%, either by restructuring the text, tabulating some of the results and/or limiting the result description to only the main findings that are discussed in the following section.

#Line 362: "longitudinal" not "latitudinal".

#Line 368-369: Confusing. Rephrase please.

#Line 430-431: "during January and July" - replace with "between January and July". Also, it is not clear at all in Fig 5d that the TEX86H temperatures are lower during these months. Please rephrase.

#Line 444: I'd argue that there's some structural similarity between the Diol Index and chlorophyll-a records.

#Line 482: What are "15 and 18°"? Latitude?

#Line 491-497: I strongly urge the authors to at least show the wavelet analysis in the supplementary info to support their claim. Please also mark the cool water events in Figure 8b to support the claim that "...the timing of the observed time periods of enhanced Diol Index variability are similar to those of the cool water events..."

#Line 496-497: I am not following this. Assuming a sampling interval of 21 days - that would give us about 21 data points per year. With so few data points in the time series, it would be impossible to detect 4 cycles in the first half of 2006. Please clarify.

#Line 498-499: It would be helpful to mark the timing of the passage of eddies in Fig 8b.

#Line 504: "Fig 5c"shows LDI not Diol Index.

#Line 508: "Fig 5e" shows LDI not Diol Index.

#Line 522: Change "due its closer vicinity" to "due to its closer vicinity"

#Line 523: "NW Africa" This is mentioned only once in the text. Spell out NW.

#Line 556: r (and p) values are more appropriate as a metric to describe the correlation between two variables than r2 (which is used to describe how well the data fit the linear regression model).

#Line 570-571: Explain briefly why one can expect LCD and levoglucosan to have similar response to degradation, e.g. in terms of their chemical behavior/structure.

#Line 578: "for" or "in" the Atlantic?

#Line 583-586: Include in the sentence the producers of 1,13- and 1,15-diols.

#Line 614: Replace "minimal differences" with "minimal variations/variability".

#Line 625-627: It is true for LDI and UK'37 that the difference between proxy temperatures and instrumental SST increase during the warmer months, but not for TEX86H. The difference between TEX86H and SST for the cooler months are almost as large as that during the warmer months, and these differences are within the calibration error. Please rephrase the sentence to reflect this.

#Line 638-640: Taken into account proxy uncertainty, I do not think it is possible to discern if the LDI temperatures are closer to SST or 20m (some temperatures are even higher than SST!), as the isotherms of the upper 30m are so close to each other anyway during the upwelling season. In any case, a habitat depth of the upper 20m is consistent with previous studies as well (as mentioned in line 646 - 649). Please rephrase the sentence.

#Line 676-690: This discussion is not supported by the $<2\,^{\circ}C$ of temperature difference between TEX86H and satellite-SST that is well within the calibration error of TEX86H. In fact, the difference is even smaller than that between the LDI temperature and satellite SST in the North Atlantic (Fig 5), which the authors did not discuss since the differences are mostly within the calibration error. The authors also need to justify why they compared the 0-150m (instead of from the same water depths as the calibration) temperatures with the temperature estimates calculated using the 0-200m calibration.

Since the focus of the paper is on LCD proxies, and this subsurface TEX86 finding was not mentioned in the abstract nor the conclusions, I would suggest to remove this paragraph.

#Line 700-703: See comment on #Line 638-640.

#Fig. 2: It took me a while to understand this figure. I think stacked bar chart would make a better option here, so instead of 12 panels with 3 bars each, you'd have 12 stacked bars which give you the same amount of information.

#Line 1184: Change "concentration" to "concentrations".

#Line 1185: Change "than" to "then".

#Fig. 3: It is impossible to tell which lines/variables correspond to which y-axes without going through the caption. I would suggest to change the color of the right y-axis and its label (Total mass flux) to grey, i.e. the same color as the plot for the variable.

#Fig. 4: This figure is mentioned for the first time at line 5XX in the section "Discussion" - I suggest to renumber it according to the order of its appearance in the text.

#Fig. 7: Specify at least in the caption if the annual mean WOA SST is averaged over latitudes or at a fixed latitude. I would also remove the panel on the left and the annual mean T0-150m in panel d if line 676-690 are removed.