Response to referee #2

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 C1 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.

We thank the referee for the positive assessment and for the comments, which we will discuss below.

Specific comments

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

We will rephrase this sentence.

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

We will correct this.

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

We will indicate the position of the ITCZ during boreal winter in Figure 1.

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

We will insert this abbreviation.

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

We will correct this.

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

We will correct this accordingly.

#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). C2

Thank you for this correction, we will revise this accordingly.

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

Yes, this is correct. We will change this accordingly.

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

We will rephrase this accordingly.

#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.

We will rephrase this.

#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.
Correct. We will emphasize 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.

We will correct this.

#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.

We will add the original references such as Conte et al (1998), Goñi et al. (2001) and Sicre et al. (2002).

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

We will correct this.

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

We will correct this.

#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).

We agree, and we will move the methods and results with respect to the time-series analysis to the supplement.

#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 C3 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.

We agree that the results section is a bit on the long side , and we will try to shorten it.

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

We will correct this accordingly.

#Line 368-369: Confusing. Rephrase please.

We will rephrase this sentence.

#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.

We will rephrase accordingly, and we agree that for M4 this decrease in TEX_{86}^{H} temperatures is not clearly visible and we will remove this statement.

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

We do not believe this (visual) agreement is strong enough to make a statement about this. Therefore, we would like to refrain from discussing this.

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

We will add the 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. . ."

We will show the wavelet analysis in the supplements. However, we cannot mark the cool water events in Fig 8b since we do not know the timings of these events for this specific time interval. We merely wanted to emphasize that Malauene et al. (2014) reported bimonthly frequency and a boreal winter timing for these cold events, which we also observe in our wavelet analysis. We will clarify this. Below are the wavelet results which we will include in the supplements:

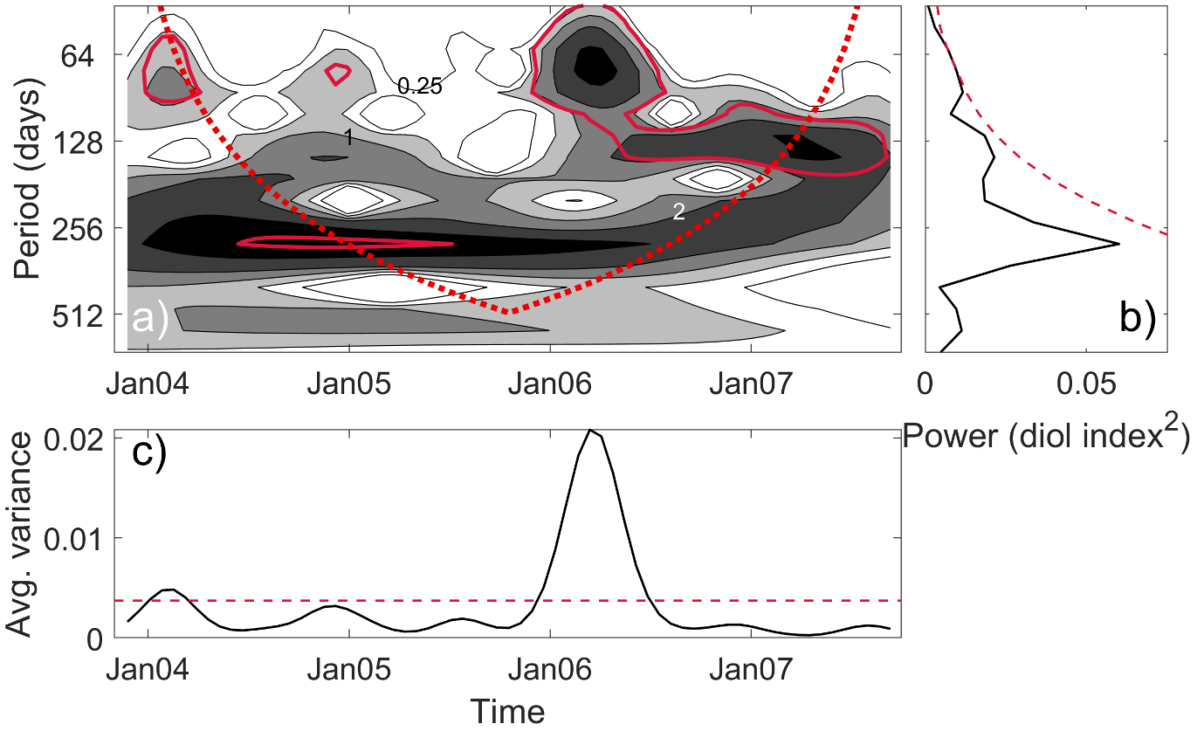

Fig. S1. a) The local wavelet power spectrum of the Diol Index in the sediment traps of the Mozambique Channel using the Morlet wavelet, normalized by the standard deviation. On the *x*-axis is time, and the *y*-axis shows the Fourier period in days. The shaded contours are at normalized variances of 0.25, 0.5, 1, 2, and 4. The bold red contour encloses regions of greater than 95% confidence for a red-noise process with a lag-1 coefficient of 0.72. Regions below the dotted red curve are where edge effects become important (Torrence and Compo, 1998). b) Global wavelet spectrum of Diol Index – the wavelet spectrum averaged in time over the whole time series. The red dashed line is the 95% confidence level. c) Wavelet power averaged over the range of scales from 42 to 90 days. The black line is the time series of the average variance within the 42-90-day range. The red dashed line is the 95% confidence level.

#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.

With a sampling interval of 21 days, the highest frequency we can detect is half the sampling rate, i.e. 1/42 cycles per day (or 8.7 cycles per year). As we describe on line 492 – 494, and now show in figure S1, the wavelet analysis showed significant variability at about bimonthly frequency (60-day period) during some parts of the time series, most notably the first half of 2006. We rephrase the sentence on line 496 – 497 to: "The strongest variability of the Diol Index at about bimonthly frequencies occurred in the first half of 2006."

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

This is a good suggestion; however, it is not completely straightforward to do this in a thorough way. We first need to decide on a definition of a passing eddy – there are several possibilities, for

example using the instrumental records of temperature, salinity, or current velocity at the moorings (one useful criterion could be, for example, lateral velocity shear between the eastern and western side), or an independent record such as dynamic height derived from satellite altimetry. Because of this uncertainty we refrain from indicating this.

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

We will correct this.

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

We will correct this.

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

We will correct this accordingly.

#Line 523: "NW Africa" This is mentioned only once in the text. Spell out NW. C4

We will correct this.

#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).

We will mention the *r* and *p* values here.

#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.

Both are functionalized polar lipids with alcohol groups and thus are chemically relatively similar. Compared to e.g. fatty acids (carboxyl group) or *n*-alkanes (no functional groups) they are expected to have relatively similar degradation rates.

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

We will correct this.

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

We will correct accordingly.

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

We will correct this accordingly.

#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.

We will rephrase this accordingly.

#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.

We agree, and we will indeed emphasize that temperature differences are within calibration error and we will rephrase this more nuanced.

#Line 676-690: This discussion is not supported by the < 2 ^{o}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.

We agree with the referee that this discussion is outside the scope of this manuscript, and that indeed we are discussing temperature differences which are within calibration error. We therefore will remove this part of the discussion.

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

We will tone down this statement in terms of the proxy uncertainty.

#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.

We will create a stacked bar chart in the revised version of this manuscript.

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

We will correct this accordingly.

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

We will correct this accordingly.

#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.

We agree, and we will adjust the figures accordingly.

#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.

We will rearrange the order of figures.

#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.

The annual mean WOA SSTs are specific for the coordinates of the surface sediments; we will emphasize this more.
Since we will remove the discussion part on the subsurface TEX_{86}, we will remove the left panel (a) and the annual mean T0-T150m and TEX_{86}-subsurface temperatures in panel d.