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Comment on bg-2021-309

Anonymous Referee #1

Referee comment on "Performance of temperature and productivity proxies based on long-chain alkane-1, mid-chain diols at test: a 5-year sediment trap record from the Mauritanian upwelling" by Gerard J. M. Versteegh et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-309-RC1, 2021

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bg-2021-309 - Performance of long-chain mid-chain diol based temperature and productivity proxies at test: a five-years sediment trap record from the Mauritanian upwelling

In this manuscript, the authors provide an extensive and impressive dataset of sediment-trap data over multiple years at a high resolution, with the main focus on long-chain mid-chain diol (diol) indices. The authors show a strong seasonality in environmental conditions, and discuss the lipid data in terms of correlations with these seasonal trends.

I think this paper provides important information on the use of diols as climate proxies, and I think the authors provide a solid discussion. My main issue with this manuscript, which isn’t a big issue to begin with, is that the authors only look at the data from sample-point to sample-point, and not at the larger picture like at a seasonal or even annual scale.

The lipid flux data shows that diols only occur in a few short pulses during the year, and it seems to me that those periods are what matter the most. At those times, the material is formed that leaves a signal in the sediment record. The authors treat periods with high and low fluxes with similar attention, and since for most of the time, the diol fluxes are low, most of the data may be insignificant.

Rampen et al., 2008 introduced the diol index based on their observations in the Arabian Sea that 1,14-diols are mainly produced (at the beginning of) the Southwest monsoon in upwelling areas, whereas C30 1,15-diol is mainly produced in both the Southwest monsoon and the Northeast monsoon. 1,14-diols were considered to be the upwelling markers, whereas 1,15-diols were used as a more-or-less constant background against which the 1,14-diol concentrations could be plotted. By doing so, they reduced the

biomarker degradation-effect on the upwelling proxy. One might reason that therefore the diol index data should contain both the upwelling- and non-upwelling data in order to work. The index was tested on sediment core material which contains multiple years of data.
In addition to that, it was indicated that proboscia alata and P. indica are dominant during the early upwelling season or should even be considered pre-upwelling species (Smith 2001, Deep-Sea Res. II 48, 1385–1402; Koning et al. 2001, Deep-Sea Res. I 48, 2473–2495). In that case, the diol index would not necessarily record upwelling length or intensity, but possibly only the occurrence of upwelling events. There are even indications that Proboscia would not be a suitable indicator for permanent upwelling, as this species cannot compete with other diatom species when silicate concentrations are high (Sakka et al., 1999, Aquat. Microb. Ecol. 19, 149–161).

I don't consider myself an expert, but I would like to ask the authors to check their use of commas in this manuscript.

Detailed nit-picking
The title. Personal point of view; one might argue that 'long-chain mid-chain diol' is a confusing way to describe these lipids. They are not 'mid-chain diols'; 'diol' is incorrect in this context as only one alcohol is positioned at the mid-chain position, and another alcohol is positioned at the primary carbon atom; with the current name, the last alcohol is ignored.

Line 25. Perhaps stress out that, for most of the time, the wind comes from the NNE – NNW direction in which it causes upwelling. Winds coming from other directions do not cause upwelling, and as a result, the link between wind speed and upwelling should not automatically be deduced.

Lines 33-34. I find it a little disappointed that the abstract of a paper on diols ends with two sentences on the UK'37. I'd consider the last sentence of the abstract to be the take-home message, and in this case, the authors want the take-home message to be to forget about the diols and look at alkenones instead?

Line 82. Subscript 2 in HgCl2

Lines 84-86. Perhaps this is obvious, but nevertheless, I found this sentence confusing. Is it correct that the large swimmers were removed before the spits were made? And considering the 'handpicked with forceps and removed by carefully filtering...'; it is either one or the other, right?

Line 95. Perhaps explain what CBeu5 stands for. This is written both as CBeu 5 (with space) and CBeu5 (without space) in this manuscript.

Line 127. '… where than used…' should this be '… where then used…' ?

Line 136. Did the authors take differences in fragmentation patterns for the alcohol, and the saturated and unsaturated diols into account?

Line 144. Which calibration was used for this study?

Line 157. Is this correct? Table 4 from that paper indicates an R2 of 0.52 for January, and an R2 of 0.62 for February.

Lines 186-188. How does this agree with lines 124-125 "GC–FID areas of the respective diols were used for quantification by comparison to the peak area of an external n-C28 1-alkanol standard."

Line 204. Since the analytical precision is mentioned here for the UK'37; did the authors also measure the analytical precision for the other indices?

Line 212. What suggests this isn't a real climatic difference? Could the same info be given for the SSTsat, to support that there is an analytical discrepancy, and not a discrepancy in water temperatures? Fig. 2 (e.g. the trend line in 2c, and lower temperature values for 2007, and lines 297-299) seem to suggest there are climatic differences between the different years. (lines 297-300) Can it be confirmed that no such discrepancies have been observed for CBEU1—4 individually?

Line 223. Was P. alata the only Proboscia species that was present in these samples? Not an expert myself, I've been told that, because of the weakly silicified cell walls of P. alata, often only fragments can be found. Can the authors be sure about their identification at a species level?

Line 290. Would it be possible to mark those directions (NNW & NNE; upwelling directions) in figure 2c, for example by dash-lines or a grayish area?

Line 332. Without further explanation, it seems odd to place the DSI in this list, as it was originally introduced as a temperature proxy. Also, the DSI exclusively consists of 1,14-diols (probably from the same organisms) whereas the other indices probably contain diols that are probably obtained from multiple sources.

Line 340. Which calibration was used?

Line 368. provide ranges and average values for these proxies (actually, for all proxy records discussed in this paper). (lack of) correlations with other data may be meaningless if the ranges are small. Is the variation in the DIw data still relevant when most of the values are >0.95?

Line 372. I would refrain from indicating correlations as 'better' as it suggests that strong correlations are to be expected.

Line 413. Add comma between dust and SST, and perhaps also after SST.

Line 440. I think it would have been valuable to have a brief. discussion-chapter dedicated on the diol fluxes first, before focusing on the proxy data.

Lines 444-445. Did the authors consider looking for 1,12-diols (C28, but perhaps also C30)? De Bar et al. (2020) suggested 1,12-diols could indicate input from Proboscia species that also produce 1,13-diols. Those anomalies occur at times of high C30 1,13-diol and high 1,14-diols, but low(er) C30 1,15-diol, which could be an additional indication for an additional 1,13-diol source, and Proboscia would be a possible candidate.

Line 450. How well do LDI temperatures agree at times of high 1,13- and 1,15-diol fluxes?

Lines 443-455. similar sources for 1,14 diols and additional 1,13-diols could be some sort of an explanation.

Lines 456-457. Why can't the absence of unsaturated diols be considered a value of 0? In particular for periods with 'considerable' combined 1,14-diol concentrations (> 100 x detection limit) this seems legitimate.

Lines 468-469. Only P alata? Rampen et al 2014 suggested that differences in 1,14-diol composition was (partly) species related. Is it likely that the same species occurs in different seasons (Spring-Summer & Autumn-Winter)?
To me, an R2 of 0.32 between the 1,14-diols and P. alata suggests that there are also other factors playing a role. Did the authors observe dissolution of silica, which could strongly affect their analyses given the weakly silicified walls of Probosica?

Lines 469-471. Not necessary true; to me, 'insignificant' seems too strong in this situation. Only mentioning Proboscia here seems like an oversimplification, if only P alata has been analyzed for this study.
In particular C30 1,13-diol follows a pattern that's also very different from C30 1,15-diol. Could there be contribution of both Proboscia and the (possibly) eustigmatophyte source for the C30 1,13-diol peak in summer 2003?

Line 472. Sinninghe Damsté et al., 2003 and Rampen et al., 2009 reported mainly mono-unsaturated C28 1,14-diol in P. alata.

Lines 479-481. Why does this make sense, without further information? Based on which mechanism would the saturation of the 1,14-diol be related to upwelling? If there is a link between nutrients and ratio of unsaturation, it would be useful to provide references.

Lines 490-492. Do the authors expect this to be an adaptation with in organisms, or a change from one species to another?

Lines 504-506. It would be interested to have such comparisons also with P. alata fluxes.

Lines 516. According to De Bar et al., 2018, Clim. Past, 14, 1783–1803:
... the NDI record likely reflects variations in the abundance of P. alata over the last 150 kyr in the Chilean margin, whereas the diol index (which also includes the C30 1,14-diol) more likely reflects the abundance of multiple species of Proboscia. …
How does this fit with the observations in this study?

Lines 532-533. Is that how it works? According to Rampen et al., 2008, 1,15 & 1,14-diols are formed in different seasons, and in that paper it was applied on a core, i.e. multiple years combined, in an area with clear seasonality in upwelling and non-upwelling/deep wind-induced mixing. I don't see any indication that high diol index values could be considered to be an indicator for permanent high production, also because Proboscia might be unable to compete with other diatoms under high silicate concentrations (Sakka et al., 1999, Aquat. Microb. Ecol. 19, 149–161).

Lines 545-546. This is exactly the mechanism that the upwelling index initially was based on (Rampen et al., 2008).

Line 558. '… other factors than temperature influence the alkenone composition.' Based on the discussion that follows and ends in line 565, I would suggest to be more specific and change this text to '… other factors than surface water temperature influence the alkenone composition.'

Line 574. Confusing sentence, also due to the commas. I don’t think alkenone fluxes were studied in this study; only the UK’37 index.