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Reply on RC1

Gerard J. M. Versteegh et al.

Author 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-AC2, 2022

We followed all suggestions for improvement. Below you will find a detailed list (reviewers comments are in bold typeface, phases (φ) are in days (d)):

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.

In this study we focus on a sediment trap signal collected about 1500m above the sea floor and we are aware that the signal can be distorted its way further to the ocean floor by (selective) degradation and transport processes. Nevertheless, we completely agree that the influence of the different fluxes on the signal that ultimately will reach the sediment is important for an adequate evaluation of sedimentary data. Therefore we included a separate chapter elucidating the effects of flux data on the signal that would be preserved in the sediment in case the complete flux as recovered by the sediment trap would reach the ocean floor without selective alteration by preservation and transportation processes.

First of al, although periods of high fluxes may seem all that matters, these periods of high fluxes, their timing and (relative) importance for the sedimentary record are defined by the periods of low fluxes and the former can not be evaluated without the latter. We evaluate the importance of the few high fluxes relative to many low fluxes by calculating the cumulative SST_{SAT} at φ=41d (the phase relation with the best correlation). This signal is defined as the integrated production temperature (IPT_{SAT}) that has been calculated for a moving window of 19 cups (about one year) by integrating the LDI diol fluxes with the $SST_{SAT.}$ Through this we obtain a signal that hypothetically should be reflected by the LDI diol proxy in the sediment by taking into account the differential fluxes (IPT_{SAT} blue line in Figure RC1-1). With a similar procedure we recalculated the LDI diol signal of all values, including outliers, for a moving average of 19 cups and integrating the LDI dial fluxes (orange line: ITPLDI (19 samples+ outliers) as well as with avoiding outliers (red curve, ITPLDI (19 samples)).

It appears that IPT_{SAT} values from January 2004—September 2005 (IPT 4 to 23) are higher than the moving average SST_{SAT} values due a single flux maximum in early December 2004. Considering a φ=41d, this production peak records high SST_{SAT} in early autumn 2004. Thus, even if the trap cups would perfectly reflect SST_{SAT}, a single flux peak may cause the IPT to differ considerably from the SST_{SAT} mean over the same period. From Autumn 2005 to Spring 2008 the IPT_{SAT} values reflect SST_{SAT} as a result of a more constant flux level.

It is obvious that even if the LDI is corrected for diol flux data (IPT_{LDI} with, and without outliers), values are considerably higher than what should be reflected in the sediments (IPT_{SAT}). They are comparable to summer temperatures. The differences between the IPT_{LDI} with, and without outliers demonstrate the effect of the outliers on the IPT_{LDI}. Especially the outliers at cups centred at 204 d (10.9°C) and 1530 d (14.9°C) have a considerable impact since they combine low reconstructed temperatures with significant fluxes (Fig. RC1-1). Our experiment therefore clearly shows that single high fluxes and outliers in the data might have large effects on the sediment signal. These factors should therefore be considered when interpreting sedimentary archive data.

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).

It is not clear to us what suggestions for improvement of the manuscript we have to distill from these remarks. Possibly the occurrence of *P. alata* is impacted by the cycle of the Mauretanian upwelling dynamics, mainly due to the elevated nutrient levels as compared

to the more nutrient poor conditions prevailing in the open ocean. However, neither the flux nor the relative abundance (%) of *P. alata* in CBeu samples show a clear temporal pattern and no straightforward correlation can be seen. We do not propose the flux of *P. alata* to be an upwelling proxy. We assume that its occurrence is upwelling related in this setting (*P. alata* is absent when conditions turn oligotrophic). In fact, our reasoning respect to *P. alata* is based on the observation that 31% of the variation in 1,14 diol fluxes is shared by the variation in *P. alata* fluxes. Correlation with 1,15 and 1,13 diols and *P. alata* is insignificant. We also know that *Proboscia* species produce the 1,14 diols. Correlation between ln 1,13 diol fluxes and %Upw is significant.

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.

The title has been changed by changing 'mid-chain' to '1,mid-chain' so that now both hydroxy positions are addressed.

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.

Statements on wind direction and its persistence have been added.

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?

We changed the end of the abstract to make this clearer.

Line 82. Subscript 2 in HgCl2

Corrected.

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?

Clarified this issue.

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

An explanation has been added.

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

Corrected.

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

Yes, MS response factors of each diol were obtained through parallel analyses of samples by GC-FID. This has been more explicitly stated now at the appropriate position a few lines earlier in the paragraph.

Line 144. Which calibration was used for this study?

For the present study both calibrations differ at most <0.1°C. This deviation is insignificant compared to the standard deviation of the proxy calibration and analytical accuracy. As such there is no preferred calibration and we chose arbitrarily to use the calibration of Rampen et al. 2021. We added a statement on which calibration we used to the text.

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.

That paper deals with samples from the southern hemisphere and thus the austral summer is from December to March. We added the respective month (February) to the text.

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."

Good point, the difference is in the use of an external versus an internal standard. We changed the text accordingly.

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?

We did not.

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?

The issue is a bit more complicated. For the first four CBeu deployments the proxy follows the temperature differences as reflected in the seasonal cycle of SST_{SAT}. For the last deployment (CBeu5) the proxy also closely follows the SST_{SAT} but with a consistent offset to higher values for all seasons.

We performed several tests comparing daily SST_{SAT} 28/03/2003 - 27/03/2007 with SST_{SAT} 28/03/2007 - 27/03/2008. The time intervals are such that entire years are compared so that there is no offset due to a different representation of seasons. The temperature records are significantly different (the means 21.4°C and 20.8°C respectively, are significantly different $p=2.1\ 10^{-7}$ as are the variances (4.2 vs. 1.9) $p=9.1\ 10^{-9}$, also the Epps Singleton Test clearly indicates two different distributions with $p=4.0\ 10^{-48}$). Although the data are cyclic and thus not perfectly normally distributed, we have still

confidence in the statistical tests since the deviation from a normal distribution is small and the p-values are very low. The difference between the years is also clearly seen in the SST_{SAT} plot in what is now Fig.3b where the last year shows clearly lower summer temperatures. Thus the (summer) SST_{SAT} values are significantly 0.6°C lower than these values during the previous years (we added this result to paragraph 3.1.1)

The uncorrected SST_{UK} of CBeu1-4 and CBeu5 also belong to different distributions with CBeu5 being significantly 1.3°C higher than CBeu1-4 ($p=1.8\ 10^{-3}$).

Most important, for CBeu1-4 the SST_{UK} and binned SST_{SAT} (with φ = 35) agree well and belong to the same distribution (mean p=0.12, variance p=0.5, Epps-Singleton p=0.12). This means that SST_{UK} accurately reconstructs SST_{SAT} by means of the global $U^{K'}_{37}$ calibration (of Conte et al., 2006). As may be expected for CBeu5, uncorrected SST_{UK} and SST_{SAT} belong to different distributions with SST_{UK} on average 2.7°C higher (mean SST_{UK}=23.4, mean SST_{SAT}=20.7 mean $p=7.1\ 10^{-7}$ variance p=0.48, Epps-Singleton $1.0\ 10^{-12}$).

Thus, with respect to the statistical analyses: for CBeu1-4, the SST_{SAT} and SST_{UK} are the same but for CBeu5, uncorrected SST_{UK} is 2.7°C higher. For CBeu1-4 vs. CBeu5, daily SST_{SAT} are in CBeu5 0.6°C lower and uncorrected SST_{UK} 1.3°C higher.

Additionally, the SST_{UK} of the last four samples of CBeu4 in winter 2007 closely follow the decrease to winter temperatures of the SST_{SAT} and the first sample of CBeu5 which started collecting only 5 days after closure of the last CBeu4 cup is the first sample starting the period with the consistent offset to higher SST_{UK}. The only sensible conclusion is that this offset is analytical.

Testing for any offset between SST_{UK} records of successive deployments is not an easy enterprise since the different deployments include different sections of the annual cycle and as such differ in the relative contribution of summer (high) and winter (low) values and thus intrinsically show offsets. However, we can test to what extent the SST_{UK} and binned SST_{SAT} for φ =35 agree for the different trap deployments. If we do this, the means of each of the first four CBeu deployments do not differ significantly from the means of the SST_{SAT} for the corresponding four intervals. This is also true for the variances except for CBeu2 (p=0.11) which may be expected from differences between the SST_{UK} and SST_{SAT} (18/4/2004-20/7/2005 with cup 4-8 missing; see what is now Figure 6).

Thus, on the basis of the individual deployments and the combined CBeu1-4 deployment the SST_{UK} and SST_{SAT} have statistically the same mean in each case. However, this is not the case for CBeu5 and the combined CBeu1-5 ($p=2.3\ 10^{-8}$). Whereas in 2008 SST_{SAT} are 0.6°C lower than the previous years, the SST_{UK} increase at the transition from CBeu4 to CBeu5. The one remaining explanation for this is that there is an analytical offset in the SST_{UK} for CBeu5.

We extended the figure caption to supplementary figure S3 to make this more clear.

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?

It is correct that several species of *Proboscia* have been described. However, the only species identified in CBeu samples is *P. alata*. It is also correct that due to weakly silicified girdle bands, most of the frustules of *P. alata* dissolves before sinking below 300-400 m water depth. However, the proboscis (uppermost part of the frustule) is stronger silicified

than the rest of the frustule, easily identifiable and can be unmistakably assigned to the corresponding species. In the studied CBeu samples, mostly the proboscis of *P. alata* was found, while girdle bands were less abundant. We added a few lines on this subject in paragraph 2.6

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?

Yes, we added a grey area for 7°E to 17°W.

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.

Rephrased and clarified.

Line 340. Which calibration was used?

This has now been clarified in the methods section, the beginning of chapter 2.4.1

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?

Linear correlations are insensitive to linear transformations, they only change intercept and slope. As such, the maximum, minimum and average provide no indication of the degree of correlation. The correlation is based on similarities in behavior: how well can I predict variable Y from X using a prescribed mathematical relation such as Y=aX+b. We therefore see no added value in adding ranges and averages.

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

We replaced 'better' by 'higher'.

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

Added.

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.

It has been added.

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.

Yes, we did but could not find any. We added a short statement on this at the end of chapter 3.3

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

We presume the reviewer means agreement with SST_{SAT}.

The highest flux of LDI diols deviates only 0.5°C from the corresponding SST_{SAT}. The next seven highest fluxes of LDI diols deviate on average 4.7°C (2.7-6.1°C). The cumulative effect of this is seen in the integrated production temperature (IPT) weighing the temperature according to the flux and for the successive samples the temperature deviation climbs to 3.6°C. The 8^{th} highest LDI diol flux is the first where the SST_{LDI} is below the SST_{SAT} (deltaT=-0.3°C). Despite this high deviation for the combined highest fluxes, here is no statistically significant correlation between deltaT and LDI diol flux, neither directly nor applied to flux rank number (the latter compensating for the logarithmic nature of the flux data). We added this information to the supplementary figures.

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

Indeed, this is stated in line 454-455.

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.

Although the DSI becomes undefined if no unsaturated diols are encountered, one can argue that if the combined diol responses are >100 x the detection limit of the $1,14C_{30:1}$ diol, the DSI would be >0.99, provided the response factors of these components are equal. However, the response of the $1,14C_{30:1}$ is 2.35 times lower than for the other 1,14 diols so that the ratio should be 1:235. Applying this we would include an extra 11 samples (2 from CBeu1 and 9 from CBeu5). The result is that the negative correlation to SST_{SAT} increases slightly (φ= -124 r^2=0.44 p=5.5 10^{-10}), wind strength shifts in phase (to φ =19) but its correlation and that of NDI show no change. All other correlations decrease (r^2 DCI=0.40, r^2 %Upw=0.30, SST_{SAT} φ =77d r^2=0.22).

There is thus no advantage in adopting this idea in this case. Nevertheless, we made a statement on this idea in the results section (3.4.1).

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)?

As seen in what is now Figure 3i, *P. alata* is present in different seasons. Off Mauritania, upwelling is a permanent, year-through feature. However, the upwelling strength and the seaward extension of upwelling filaments varies, e.g. upwelling filaments may overlay the sediment trap at any time although it seems more frequent during spring (Fischer et al., 2019). Since *P. alata* is one of the species associated with this upwelling, it also may be present anytime (as we already stated in the manuscript). Due to the short-term dynamics of the system we can not tie the presence or absence of *P. alata* to specific upwelling conditions or to upwelling related plankton succession. We made this more clear in section 2.6 and added in section 4.4 that *P. alata* is the only species of its genus encountered.

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?

The occurrence of a weakly silicified diatom as *Proboscia alata* in sediment trap samples is always affected by dissolution/preservation, and we agree with the reviewer in that other factors might have impacted its occurrence. Despite the fact that some dissolution might have affected *P. alata* valves, it would only be partly responsible for the unexplained variance. Considering the widespread occurrence of diols in the Ochrophyta and the tiny number of ochrophyte species analysed, we do not assume that *P. alata* is the only source of 1,14 diols in the Mauritanian upwelling system. Neither do we suppose that the diol concentration per cell is constant. As such, it is unlikely that there is a 1:1 correlation between 1,14 diols and *P. alata* frustule abundance. We are in fact rather pleased to find any significant correlation in such a complex system and to us this strongly suggests that *P. alata* is an important contributor of diols in the CBeu samples.

We extended the statement on the 32% explained variance by adding the option of other possible interfering factors.

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.

The entire diatom assemblage has been analysed to the species level and other *Proboscia* species are absent.

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?

This could well be true, but we have no means to further substantiate this.

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

We do not further specify the distribution of the 1,14 diols for this species for the following reasons.

In Sinninghe Damsté et al., 2003, *P. alata* is not reported to produce significant amounts of 1,14$C_{28:1}$ diols. In their Figure 2, the only unsaturated diol reported for this species is the 1,14$C_{30:1}$ diol and it is noted in the caption that there is also a contribution of the 1,14$C_{28:1}$ diol co-eluting with the 12-hydroxy methyl alkanoate but any reference to its abundance is absent. The text furthermore states that the 1,14$C_{28:1}$ diol in stead of the $C_{30:0}$ diol shows up in the 1,14-diol distribution but it is unclear if this is only a statement on presence/absence or if it also is meant to provide an indication on the relative abundances. Finally, in the same Figure 2 of that paper sediment samples from the Skagerak and Benguela current report no 1,14$C_{28:1}$ diols despite the likeliness of *P. alata* being a component in the overlying surface waters. Sinninghe Damsté et al., 2003, also state: 'We acknowledge that physiologic factors may influence the relative abundance of the lipids and that the differences in distributions between the two cultured *Proboscia* species (and between cultures and natural samples, [..]) may be better explained by physiologic differences rather than by genetic differences'.

In Rampen et al., 2009, *P. alata* is represented by only one sample derived from a culture of this species at 2°C and according to a footnote in their table 2, those data are derived from Sinninghe Damsté et al., 2003. There are no other *P. alata* data in that paper. Here, indeed, it becomes clear that at 2°C the 1,14$C_{28:1}$ diol dominates the diol distribution. However, if we assume that the physiological response to temperature is similar for all

three *Proboscia* species in that study (as is done by the authors), a drastic decrease in the relative abundance of the 1,14$C_{28:1}$ diol is observed with increasing temperature. For the temperatures in our study >18°C the relative abundance of this diol (in *P. indica*) is always below 18% and below 10% for temperatures >22°C.

Interestingly, the 1,14$C_{28:1}$ diol did not play a role for *P. indica* in Sinninghe Damsté et al., 2003 but it is clearly present in the cultures of the same species in Rampen et al., 2009, supporting the view cited above that physiologic factors may influence the diol relative abundances in cultures and may account for differences with natural samples.

The rarity of the 1,14$C_{28:1}$ diol in our data and natural samples as well as from the distribution of the 1,14$C_{28:1}$ diols in *Proboscia* cultures in relation to temperature imply that the presence or absence of 1,14$C_{28:1}$ diols in natural samples is of very limited use in our case. However, the ability of *Proboscia* to produce large amounts of 1,14 diols is important.

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.

The relation between upwelling and saturation is that stronger upwelling induces lower temperatures in the photic zone which would increase unsaturation according to the principle of the known physiological response of higher membrane fluidity (more double bonds) at lower temperatures. We thus do not assume a physiological response of *P. alata* leading to more unsaturation due to higher nutrient levels (although that may exist). If the occurrence of *P. alata* is upwelling related, lower temperatures in the absence of upwelling would obscure a correlation between diol saturation and temperature since 1,14 diols may be produced by organisms other than *P. alata*. We stress again that analysis of the complex relation between environment, the presence of species, and their physiological responses is vital for understanding the sediment signal. Here, the combination of temperature AND upwelling dynamics needs to be investigated to understand the *P. alata* contribution to a temperature proxy.

We addressed this in the text.

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

This is currently not known but considering that low temperatures are generally recorded in spring, it seems reasonable to assume that adaptation to low temperatures plays a role.

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

The DSI, DSI with samples lacking unsaturated diols and the DCI show neither significant correlations with the *P. alata* fluxes nor with the log transformed *P. alata* fluxes ($p>0.1$ in all cases, Table 4). The data basis for such a calculation is very poor since for the DSI only 35 samples show both *P. alata* and DSI values whereas there are only 44 samples available for the DCI - *P. alata* correlation.

We added this information to the text.

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?

In our study there is no significant correlation between *P. alata* abundance (both absolute and log transformed) and the NDI. Neither do we have additional *Proboscia* species. As such we are not able to either confirm or reject these hypotheses. As far as we know, the only culture derived diol composition for *P. alata* is obtained from an experiment at 2°C. Therefore, statements on the contribution or absence of this species on the basis of the abundance of the $1,14C_{30}$ diol in sediments seem premature.

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).

Good point Strictly, the high production during part of the season may also lead to high DI_R values. We rephrased the text accordingly.

NB. the 1,14 and 1,15 diols are often produced simultaneously but not always (see the data presented here and by Rampen et al., 2007 or Gal et al., 2021). In less eutrophic systems (e.g. de Bar et al., 2019) the 1,13 and 1,15 diols seem to dominate throughout the year whereas the 1,14 diols show flux maxima during short periods in spring (e.g. de Bar et al., 2019 in traps M1 and M4 closer to the continental margins) or not at all (trap M2 in the oligotrophic open ocean). In general, the 1,15 and 1,13 diols are also produced when 1,14 diols are produced but the opposite is not true.

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

Yes, we added references here.

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.'

Done.

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.

It has been changed by moving 'alkenone' further to the end of the sentence.

Please also note the supplement to this comment:
https://bg.copernicus.org/preprints/bg-2021-309/bg-2021-309-AC2-supplement.pdf