Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2019-345-AC2, 2019

Biogeosciences
Discussions

Interactive comment on "Comparison of the $U^{K'}_{37}$, LDI, TEX^{H}_{86} and RI-OH temperature proxies in the northern shelf of the South China Sea" *by* Bingbing Wei et al.

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Received and published: 14 December 2019

Dear reviewer, Thank you for your constructive comments. We have addressed your comments and make changes accordingly. Please find related contents in the document file with changes marked.

Section 2.1: How as the sedimentation rate determined (1–2 mm/yr, stated in line 170)? The sediment accumulation rate is likely to vary across the offshore transect. Therefore, assuming that all core-top samples represent the mean conditions of a 7-year interval (from 2005–2012) is probably not appropriate. This assumption also does not account for bioturbation, which almost certainly has caused some mixing of material

from the past few decades into the upper few centimeters. Making sure that the core-tops in this study are "calibrated" to observational temperatures from the appropriate time interval is especially important given the large SST trends observed over the past decade in the SCS (e.g., Yu, Y., Zhang, HR., Jin, J. et al. Acta Oceanol. Sin. (2019) 38: 106. https://doi.org/10.1007/s13131-019-1416-4).

Response: Thanks for the comments. The sedimentation rate here is unknown. In the revision, we updated SSTs data from WOA18 (from 2005–2017), a wider time interval could better cover sampling time. Although SST have increased over the past decade in the SCS, but it has less influence to the average SST data within the time interval (e.g., WOA13 vs. WOA18, the average difference is less than 0.5 °C, which is lower than the calibrations error for each proxies).

Uk37-derived temperatures: A number of studies have pointed out the non-linearity of the U37K'-Temperature relationship at SSTs >24–26°C (e.g., Sonzogni et al., 1997, Conte et al., 2006, Tierney and Tingley, 2018). Since SSTs are >24°C for most of the year at these SCS core sites, it would be worthwhile to calibrate the U37K' data in this study using BAYSPLINE (Tierney and Tingley, 2018), which accounts for the attenuation of the U37K' signal at higher SSTs.

Response: We have checked the difference between linear and nonlinear calibrations, especially the BAYSPLINE (Tierney and Tingley, 2018) SST estimate. Indeed, BAYSPLINE SST estimate yields slightly higher temperature values by ∼0.5 °C in average than the linear calibration. But this is not against our conclusion of spring bias of alkenone temperature but reinforces it. Pls see related statements in lines 218–220 and 275–277.

Supplemental figure 1. If the U37K'-derived SST ends up being significantly different using alternative U37K'-SST equations, it will have implications for the inferred seasonality of the U37K' signal and needs to be addressed more thoroughly in the main body of the paper.

Response: In the revision, we moved the supplementary figure 1 to Fig. 3, and all derived U37K'-SSTs exhibited similar values that differed from the calibration of Conte et al. (2006) by <0.5 °C (0.2 °C average).

Lines 60–79: There is a substantial body of literature discussing sources of uncertainty and biases in these four biomarkers (especially TEX86 and Uk37) from sediment traps, surface sediments and culture studies. This section should be expanded to include some discussion of those factors. Just a few examples: lateral advection of sediments, light limitation, diagenesis (e.g., preferential degradation of C37:3 in sediments), sensitivity to redox conditions, etc.

Response: In the revision, we add "Nevertheless, environmental and physical parameters may also bias these proxies, including: (1) lateral advection (Benthien and Müller, 2000; Kim et al., 2009); (2) different resistance to degradation (Goni et al., 2001; Kim et al., 2009); (3) nutrient stress and light limitation (Hurley et al., 2016; Park et al., 2019; Prahl et al., 2003; Versteegh et al., 2001)." Pls see related changes in lines 71–74.

Line 167: Why not use the latest WOA18 data?

Response: We updated our data with the latest WOA18.

Line 181–182: Looking at the local hydrographic data, the depth of the mixed layer appears to vary seasonally, with a much deeper winter mixed layer. This should be considered when discussing the potential seasonal and/or depth distribution of the biomarker source organisms.

Response: The use of the water column structure data in phytoplankton ecology studies is really a common practice. However, our study sites are mostly shallow with water depths of most of them <50 m, where water stratification does not extensively occur and WOA data fail to provide high-resolution data. So we didn't rely our interpretation on those information.

Overall, there needs to be a much more thorough treatment of uncertainty in the

manuscript. There are no error bars or uncertainties shown in any of the figures, nor are they discussed in the results or the supplementary data table. Uncertainty in the various transfer functions used to convert each of these indices to temperature needs to be considered. Analytical uncertainty could be addressed via replicate measurements of samples or standards. The analytical error and calibration error should be propagated and reported when converting proxy index to SST.

Response: In the revision, both calibration and analytical errors are considered and described in the Method section. Our analytical errors for different proxies are much lower than calibration errors. Pls see related contents in lines 114, 116–118, 132, 138, 166, 170–172, 181, 185–187.

Lines 198–204: In order to maintain the organizational flow of the paper, this section belongs in the Discussion. Also, instead of making the argument that you omit these 6 samples because of their large SST residuals, it makes more sense to omit them because the river input index (%C32 1,15) values are 4x higher than those of the other 19 samples.

Response: Rephased. Pls see related contents in lines 317–320.

Section 4.2.2: Is there any correlation between the BIT and diol river input index in this sample set?

Response: BIT exhibited a linear relation with diol river input index (R^2 = 0.66, p <0.001). We added it in line 320.

TEX86: There is a large body of literature on the TEX86 proxy that is overlooked in this manuscript. Marine Thaumatchaeota are living throughout the water column in many locations, and it's likely that the TEX86 is integrating the entire water column in these shallow (<200 m) sites. As with the Uk37-SST equations, I would suggest including BAYSPAR-derived SSTs in the TEX86-SST analysis (Tierney and Tingley, 2015).

Response: We re-evaluated our data using BAYSPAR according to the comment, and

did not find substantial differences. Comparatively, the local "shallow-water" calibration yielded smallest temperature residuals. Pls see related discussion in lines 392–393 and 398–402.

Section 4.3.1: The Ring Index (RI), as defined by Zhang et al., 2015, could easily be calculated from the GDGT data used to calculate TEX86 in this study. This is another tool (in addition to the MI, [2]/[cren], and [0]/[cren]) that could be used to screen for non-thermal influences on iGDGT distribution in this sample set.

Response: We admitted that we need to be thoughtful. In the revision, we added these related contents. Pls see our discussion in lines 368–377.

Figures: The uncalibrated index values (uk37', LDI, RI-OH and TEX86) are not reported in your figures. I think there should be at least a table that shows the primary data from each of the core-top sites.

Response: In the revision, we added related contents in Table 1. Pls see.

Figure 2: This figure is not a very effective way to present these data. I would consider presenting SST maps to show the mean annual, winter and summer SST distribution in the study area (showing spring and autumn is unnecessary in my opinion). Perhaps create separate panels for each of the SST indices. If the authors decide to keep this figure, the lines connecting WOA data points need to be removed, they are distracting. The use of the same colors for the WOA SST data and the proxy-SST data is confusing.

Response: In the revision, this figure was split to a 4-panel figure with 1-panel per proxy. Although the lines connecting WOA data still exist, but they have no impacts on showing the differences between WOA SST and proxy SST.

Figure 4: As with Figure 2, panels a and b are not a very informative. Dividing the samples into "inshore and offshore" as is done in Figure 5 and presenting the relative abundances and ratios/indices as bar graphs would be more effective. Again, if the authors chose to keep these panels, refrain from connecting data points with lines.

Response: In the revision, we changed related figures. Pls see Fig. 5.

Figures 3 & 5: The inshore versus offshore comparison of the fractional abundances of diols in figure 5 illustrates the same point as the maps in Figure 3. Therefore, I think Figure 3 is unnecessary. It could be moved to the supplement or removed altogether without losing any information. Figure 6: I would suggest adding panel d from figure 3 as a second panel in Figure 6 to better illustrate the elevated influence of terrestrially sourced diols in the PRE.

Response: Yes, there are some repetition between Fig. 3 and Fig. 5, but we want to keep this map, because it not only emphasizes the similar spatial variation of C28 and C30 1,13-diols and C32 1,15-diols, but also exhibits the "unusual" data points. Fig. 5 emphasizes the comparison of LCDs composition between SPM and surface sediments in this study. In the revision, we combined Fig. 3, 5 and 6 in the new Fig. 4. Pls see.

Figure 7: There is no reason to plot the residuals of 3 different calibrations versus the BIT index. If the purpose of the figure is to illustrate that there is no systematic relationship between BIT and SST residuals, then you need only illustrate this using one of the calibrated data sets. If the purpose of the figure is to illustrate the calibration equation that results in the smallest residuals, I would suggest a simpler way of showing the distribution of the data (e.g., box and whisker plots).

Response: The purpose of this figure is to show that residuals from local calibration is smallest compared to global and Bayspar calibrations. Following your suggestion, we changed it to box and whisker plots (Fig. 6b).

Figures 7b & 9: I don't think it's terribly informative to regress any of these indices over a <2 °C temperature gradient (as in the case of summer and autumn SST gradients), however, if you are going to make this plot, why not make a 4-panel figure that does the same for all 4 indices?

Response: Yes, linear regression is not an appropriate method applied to such narrow temperature intervals here. In the revision, we deleted this plot and related discussion.

Figure 8: The source of the data from a "previous study" needs to be cited in the caption.

Response: In the revision, Fig. 8 was moved to Fig. 7, and the source of data was added. Pls see the caption of Fig. 7.

Figure 8: In the caption, the authors need to clearly state what "annual residuals" are. Also, "fitting lines" is not a mathematical term. Are these ordinary least squares regression lines? Something else?

Response: The explanation of "residuals" could see Eq. (14). We added related statements in the caption. Pls see the caption of Fig. 7.

Minor Comments: Lines 49–50: This sentence is awkward to read. I would suggest changing to something like "Due to the distinctive ecology of their source organisms (e.g., depth habitat and seasonal preference), coeval temperature records from each of these proxies may differ substantially"

Response: Changed. Pls see lines 51–52.

Line 51: Remove the "however" from this sentence.

Response: Removed.

Lines 57–59: Remove “while” from the beginning of the sentence. These two sentences seem to contradict each other. Also, what is the southeast Australian Ocean?

Response: "while" is removed. We think that these two sentences does not contradict each other. We change "southeast Australian Ocean" to "Australian southern and eastern coasts". Pls see in line 60.

Line 60: This sentence doesn’t make sense: "The accuracy of organic thermometers

is also prone to be impaired by a low specificity of related biomarkers."

Response: We changed "The accuracy of organic thermometers is also prone to be impaired by a low specificity of related biomarkers.” to “The accuracy of organic thermometers is also interfered by the diverse origins of related biomarkers."

Line 83: Should be northeasterly and southwesterly winds

Response: Changed. Pls see line 88.

Lines 171–172: I believe WOA13 defines the summer and autumn as Jul–Sept and Oct–Dec, respectively (not Jul–Aug and Sept–Dec as stated here).

Response: Changed. Pls see line 194.

Line 199: Instead of stating "three samples with low LDI values", it would be more descriptive to state, "three samples with LDI values lower than predicted from local SST".

Response: This sentence was deleted in the revision.

Line 244: Explain what is meant by "complex sedimentation processes". If the authors are talking about lateral transport of the fine sediment fraction, or diagenetic alteration of the signal, this could be expanded on significantly here.

Response: "complex sedimentation processes" means lateral advection and resuspension processes. Due to a narrow SST intervals, the impact of lateral advection may be minor. For examining the diagenetic alteration, it is better to use SPM from different water depths or downcore sediments. It is beyond our paper, because our samples are core-top sediments.

Lines 257–258: This sentence is confusing and should be rewritten for clarity.

Response: This sentence was deleted. Because if we take the calibration error and analytical errors into consideration, the slightly lower UK37’-SST observed in the river

mouth is possibly due to the above errors.

Line 279: It is unclear what is meant by "an opposite response to ambient temperature of C30 1,15-diol to C28 and C30 1,13-diols".

Response: Deleted.

Line 305: Change "methane-related" to methanotophic archaea

Response: In the revision, we changed "methane-related" to "archaea involved in methane cycling". Pls see lines 341–342.

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