

Interactive comment on “Constraints on the applicability of the organic temperature proxies U_{37}^K , TEX_{86} and LDI in the subpolar region around Iceland” by M. Rodrigo-Gámiz et al.

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Received and published: 15 July 2015

Referee: Gesine Mollenhauer

General comments: In this manuscript, Rodrigo-Gámiz and co-authors examine the applicability of three organic biomarker based proxies for sea surface temperature (SST) using samples of suspended matter collected from near-surface waters, sinking particles collected using a sediment trap, and surface sediments. All samples are analyzed for the three biomarker SST proxies, and the results are compared with in-situ SST, satellite SST estimates, and World Ocean Atlas temperatures. The data presented are interesting and have the potential to lead to a better understanding of

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the proxies' behavior in the study region. There are, however, several major aspects that require consideration and likely will result in a much different interpretation of the results and different conclusions:

We thank the reviewer for her comments and suggestions.

1) The core piece of data stems from samples collected using a sediment trap moored for one year at a water depth of 1850 m. There is abundant literature discussing sinking rates of particles based on data obtained on samples collected with sediment traps, all indicating that considerable time elapses between the formation of a biogenic particle and its settling to deeper water depths (e.g., MuñLler and Fischer, 2001, DSR, Fischer and Karakas, 2009, Biogeosciences, Yamamoto et al., 2007, DSR, 2012, OG, etc.). Some of these papers include the finding that settling rates might be different for different types of particles. Sinking rates are often calculated from the phase shift between proxy records and satellite observations, which in turn means that the time the sinking requires needs to be considered when comparing proxy data and observed SST. This is completely ignored when discussing differences in temperature estimated using the lipid biomarker proxies and satellite derived temperatures.

With respect to the comparison between proxy derived SST and satellite derived SST in the settling particles from the sediment trap, we agree with the idea that usually there is a time lag between the production of the biomarker and its sinking to deeper water depths, arriving at the sediment trap. However, in the present case, almost synchronous variations in net primary production and the fluxes of the various lipids at 1850 m are observed (Fig. 3). This suggests that there is no major difference within the resolution of the sediment trap between signals generated in the upper part of the water column and those received in the sediment trap at 1850 m. A rough estimation of the sinking velocity based on the resolution of the trap suggests that this would correspond to a sinking rate of up to ca. 230 m per day, which is quite fast. Reported lipid fluxes for GDGTs are ca. 260 m per day in the NW Pacific (Yamamoto et al., 2012) and for alkenones are ca. 280 m per day in the filamentous upwelling region off Cape

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Blanc (Müller and Fischer, 2001). These fluxes compare well with our estimated lipid fluxes.

Based on these estimations, the differences observed between proxy derived SST and satellite derived SST are likely not related to delayed signals. We have provided alternative explanations in the manuscript to explain the differences between proxy-derived SSTs and satellite-derived SSTs.

2) All UK'37-based SST estimates are based on the core-top calibration by Müller et al. (1998), even though this calibration is explicitly derived for sediments. Since the publication of this seminal paper, however, more efforts have been undertaken to refine the calibration of the UK'37 proxy, in particular for samples of suspended matter. In their paper published in 2006 in GC, Conte and co-authors compile a large data set obtained on SPM and compare it with core-top data. Their calibration for SPM is polynomial, while the best fit for core-top sediments is linear. The largest discrepancy between the two is approximately between UK'37 values of 0.15 and 0.45 and can amount to up to $>3.5^{\circ}\text{C}$. This is a) exactly the range of UK'37 values observed in this study and b) very similar to the temperature offsets between UK'37-SST and in-situ values. Moreover, the authors provide an explanation for the discrepancy between the two, which should be considered in this manuscript as well. It has furthermore been previously observed that the polynomial calibration also results in better agreement between observations and reconstructions from samples collected by sediment traps (Mollenhauer et al., 2015, DSR). I thus suggest that the authors re-calculate their temperature estimates using the polynomial regression for SPM and sediment trap samples and the core-top calibration for core-top sediments. I expect that the agreement between observations and reconstructions will be much improved and, as a result, the conclusions will be substantially different.

We appreciate the comment to use the specific SPM calibration proposed by Conte et al. (2006). However, when we apply the SPM calibration of Conte et al. (2006), we do not find reduced offsets between UK'37-SST and satellite SST for the SPM around

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Iceland, with an average offset of 2.7°C . For the SPM transect south of Iceland, we do observe a slightly reduced offset ranging from 0.5 up to 2.5°C .

Figures: comparison between Müller et al. (1998) and Conte et al. (2006) calibrations (see figures at the end of the document).

For the sediment trap, the UK'37-SSTs (black line) using the SPM calibration of Conte et al. (2006) overestimates satellite SST by about 3°C on average (dashed orange and purple lines), whereas the UK'37-SSTs (green line) using the calibration by Müller et al. (1998) overestimate satellite SSTs on average by only 0.3°C .

Therefore, we prefer to keep using the calibration by Müller et al. (1998).

3) In the abstract and conclusions, it is fairly strongly stated that a good agreement is observed between TEXL86 0-200 m temperatures and WOA observations of annual mean and winter depth-integrated temperatures. This is, however, only based on data from the core-top sediments ($n=10$), while the entire data set on settling particles ($n=21$) does not support this conclusion. In contrast, the TEXL86-temperatures for 0-200 m are substantially overestimated with respect to the WOA data. In my view, this discrepancy mandates further investigation and does not allow to draw the conclusion presented in the manuscript.

We agree and we will rephrase these sentences in both the abstract and conclusions.

4) The language requires improvement. There are several errors in grammar and a number of awkward expressions and overly long sentences.

In the revised version we will modify the text and improve our phrasing.

5) The data obtained within this study are not completely presented in the tables. In Table 1, information on sampling stations is given, for all samples. In Table 2, however, where proxy data are presented, only the samples from the sediment trap are listed. In situ temperatures used to compare the proxy results with are missing entirely, as well as total fluxes. Please add missing information.

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We thank the reviewer for this suggestion. We will add the bulk fluxes and main lipid fluxes to Table 2. Furthermore, a new table, as supplementary material, will be included with the index values and estimates SSTs of each proxy and temperatures derived from WOA and NOAA.

Below I list a number of specific comments: Page 1115, lines 25 and following: It is a bit too simplistic to state that soil-derived contributions of isoGDGTs can be neglected at $BIT < 0.3$; please reword.

The reviewer is correct in that it will depend on the location whether a BIT index threshold of 0.3 is sufficient to exclude an impact of terrestrial input (cf. Schouten et al., 2013). More clues may be obtained by correlating the BIT index with TEX86 values where a significant correlation indicates an impact of terrestrial input. In our case the correlation is negligible ($R^2 < 0.08$) and thus the TEX86 is unlikely to be affected by a terrestrial influx of GDGTs. We will rewrite the sentence about the BIT index in the revised text.

Page 1116, line 28: there is only one paper by Rodrigo-Gámiz listed, so please omit the “b” after 2014.

We will adjust this.

Page 1117: Please add information on the productivity regime and the timing of phytoplankton blooms, in particular on coccolithophorid blooms, to the description of the study area. Nutrient regimes might also be interesting.

We will add some information on this in the revised text.

Page 1118: The fact that the cruises during which the samples were collected are specifically named suggests that additional information on these cruises (e.g., a cruise report) is available. However, no reference is made to such information. Please clarify.

We appreciate this observation. There are two cruise reports where all the material collected was described. We will add the cruise report numbers and references.

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Page 1119, lines 14 and following: At which temperature was the saponification carried out? The method description for the extraction of the filters is not clear: How can you extract water with a mixture of water and methanol? It seems to me that there is an error. Please clarify.

With respect to the saponification method in the SPM, we understand that it is a bit unclear. In the revised version we will clarify the method and explain it better, as also requested by the other Reviewer.

Page 1120, line 1-2: Why was there only the diol standard added to “some” samples? On which grounds was decided which of the sediment trap samples were treated with copper to remove sulphur?

No internal standards were used in the first extractions of surface sediments, and then we used only the diol standard for the rest of surface sediment samples and SPM collected in summer 2011. After the second cruise in summer 2012, we decided to expand the study to the other two lipid proxies and we added standards for GDGTs as well as for alkenones. With respect to the treatment to remove elemental sulfur; we treated those samples in which we detected an elemental sulfur by GC-MS analysis.

Page 1124, line 15: Please provide total fluxes also in the table.

We will add this to Table 2.

Page 1129, line 18 and following: This line of arguments is not convincing: Usually, the TEX86 paleothermometer is determined on core lipids, not on IPLs. Therefore, a mismatch between core-lipid SST estimates and observations is a relevant signal. Including IPL-TEX86 does not help in resolving the discrepancies.

We agree that TEX86-derived SST is commonly based on core lipids, but IPLs might better reflect in situ conditions. We will rewrite some parts although we have already provided some explanations about the mismatch between proxy estimated SST and satellite derived SST in the manuscript.

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Page 1131, line 4 and following: The fact that fluxes are highest in the summer does not necessarily mean that GDGTs were produced during this time and represent summer SST. Considering that the TEX-based SST estimates are lowest during the high-flux periods, as can be seen in Figure 4, this is a rather unlikely scenario.

We agree and we will address this observation in the revised manuscript. Indeed, based on previous studies (e.g., Wuchter et al., 2006; Huguet et al., 2007; Mollenhauer et al., 2015), high GDGT flux periods in summer are explained by a preferential transport of GDGTs due to high phytoplankton bloom.

Table 1, caption: The caption is incomplete: What does “Flow meter (l)” and “Cross cut (l)” mean? Column headers should be “core length” and “volume” instead of “Long” and “Flow meter” and “Cross cut”.

We agree, “cross cut” will be delete as it gives similar information to “flow meter”, which we will be replaced by “volume pumped”.

Figure 2: Cross plots of the in-situ and satellite temperatures versus the reconstructed SST might be more revealing.

We appreciate this comment. However, the cross-plots suggested (such as shown in the above comment about UK’37 calibrations and attached pdf file) are, in our opinion, not adding additional information. Adding these plots to the current Fig. 2 would results in many more panels in Figure 2 (i.e. cross-plots of proxy values against satellite SST, in-situ SST and summer mean temp. at 50 m). Thus, we prefer to keep the original Figure 2.

Figure 3: Please consider adding vertical lines to help guide the eye.

We appreciate and agree with this suggestion. We will add some vertical lines in the figure.

Interactive comment on Biogeosciences Discuss., 12, 1113, 2015.

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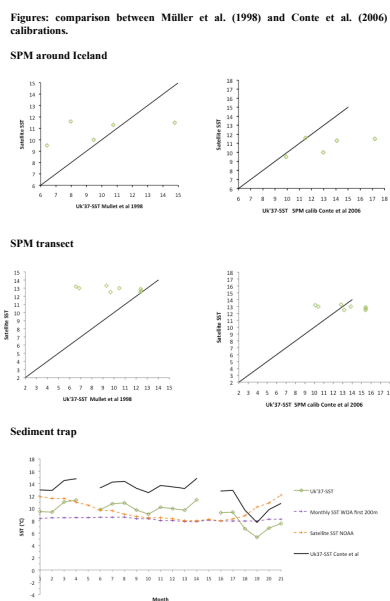


Fig. 1.

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