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Interactive comment on "Constraints on the applicability of the organic temperature proxies $U^{K'}_{37}$, TEX_{86} and LDI in the subpolar region around Iceland" *by* M. Rodrigo-Gámiz et al.

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In their manuscript "Constraints on the applicability oft he organic temperature proxies UK'37, TEX86 and LDI in the subpolar region around Iceland", Rodrigo-Gamiz 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 analysed 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 the proxies' behaviour 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:

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., Müller 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.

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°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 mansucript 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.

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.

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

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.

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. Page 116, line 28: there is only one paper by Rodrigo-Gamiz listed, so please omit the “b” after 2014. 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. 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. 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. 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? Page 1124, line 15: Please provide total fluxes also in the table. 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. 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. 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”. Figure 2: Cross plots of the in-situ and satellite temperatures versus the reconstructed SST might be more revealing. Figure 3: Please consider adding vertical lines to help guide the eye.

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