

Dear Editor,

Please find submitted our rebuttal and the revised version of the paper “Constraints on the applicability of the organic temperature proxies  $U^{k'}_{37}$ ,  $TEX_{86}$  and LDI in the subpolar region around Iceland” by Rodrigo-Gámiz et al. As outlined in our on-line responses to the referee’s we have revised parts of our manuscript to reflect their comments. For a detailed rebuttal I refer you to our response (**in bold**) below to the comments of the reviewers (*in italics*).

We hope you find the revised version acceptable for publication.

On behalf of all co-authors,

Sincerely,

Sebastiaan Rampen

Response to reviewer Salcup:

*General Comments:*

*In this work the authors investigated the applicability of  $U^{k'}_{37}$ ,  $TEX_{86}$ , and LDI to paleotemperature reconstructions around Iceland using filtered water, sediment trap, and surface sediment samples. The results suggest that while there is good agreement between proxy derived temperatures in surface sediment samples and seasonally averaged temperatures, there are large discrepancies between the proxy inferred temperatures in suspended and sinking particulate matter and in situ temperature, as has been seen previously. The authors contribute these discrepancies to production seasonality, diagenetic alteration, and/or lack of producing species. This work represents an important study of three widely used paleotemperature proxies in a climatically important setting in which previous work has shown them to have difficulty reproducing meaningful temperature estimates. The authors adequately addressed nearly all of my prior remarks, except in one instance, noted below. It warrants publication after some minor edits for clarity.*

**We thank the reviewer once more for his kind words and the comments and suggestions given below.**

*Specific comments:*

*Lines 93-95: The link between increased stratification and a deep mixed layer is counter-intuitive. One would think that increased stratification would lead to a thinning of the mixed layer. Please clarify how these two phenomena co-exist.*

**The reviewer is correct; this was erroneous, and has been corrected in lines 93-95.**

*Lines 140-142: Were DCM and copper added to sediment and SPM samples too? If not, why?*

**There was not sulphur present in the SPM and sediment samples; hence there was no need to desulphurize those samples.**

*Lines 144-161: The authors addressed my initial concerns regarding the complexity of the sample work-up, but the steps are still a bit unclear. I might have trouble repeating this work-up based solely on the*

text. Would the authors consider using a flow chart to illustrate the steps performed on each of the many fractions?

**We made some additional changes in this part of the method-description (lines 156-158). However, in our opinion, a method flow chart is not necessary and it does not fit within the format of the manuscript.**

*Section 2.3.1, 2.3.2, & 2.3.3: Please list the calibration errors associated with each of the  $U_{37}^K$ ,  $TEX_{86}$ , and LDI Indexes.*

**Calibration errors for the  $U_{37}^K$  and the LDI are given in lines 190 and 269, respectively. The calibration error for the  $TEX_{86}$  was already given in line 225.**

*Lines 541-542: "This suggests the July is not the period of highest productivity for long chain diols around Iceland." Why the focus on July? Did you hypothesize their abundance should be highest at this time?*

**The focus is on July, as SPM was collected in this month. Our sediment trap data showed relatively high fluxes of long chain diols for this month for both 2011 and 2012.**

**We do agree that not necessarily the highest production would be expected during this month, and we slightly modified this text (Lines 562-563).**

Response to reviewer Mollenhauer:

**We are also very grateful for the comments, questions and suggestions from the second reviewer.**

*Methods:*

*Were the SPM samples taken in 2011 at ~5 m water depth also taken with a MacLane pump?*

**The information on SPM sampling, provided in lines 112 – 114, concerns both 2011 and 2012 sampling cruises.**

*Details on the sediment trap: Surface area of the collection funnel? At what depth was the trap moored?*

**Additional information concerning the sediment trap is now given in line 117. Water depth of station 1, where the trap was moored, is now indicated in line 107 (2255 m).**

*Results:*

*Statements on the flux maxima occurring exactly at the beginning and at the end of the trap deployment period are problematic. Please re-word to „highest fluxes were recorded...“*

**We modified our text accordingly in lines 302, 308, 313, 316, 319 and 323.**

*Discussion:*

*Lines 392-395: From the data displayed in Figure 4, a sinking rate of 230 m per day seems unrealistically high to me. From the offset ( $U_{37}^K$  maxima in trap cups 13-17, SST maximum in July 2011) it looks more like roughly 10 m per day to me. Please explain in more detail how the sinking velocities were derived (delay in days).*

**We have modified our text concerning the sinking rate. Indeed, when calculating the sinking rate based on comparison of alkenone-derived and satellite derived SST, sinking rates are much smaller. However, we also point out that the calculation of sinking rates is problematic, and the values are not in agreement with other data (lines 397-413)**

*Lines 475-478: Here it is ignored that GDGTs cannot be exported instantaneously to depth (1850 m water depth). There must be a delay in the temperature signal produced at the surface and collected in sediment traps, which relates to the average sinking velocity of GDGT-bearing particles, which is likely smaller than that for alkenone-bearing particles. The discussion here is in contradiction to the newly inserted paragraphs on the UK'37 record and its implications for sinking velocity of alkenones and with what follows in lines 497-499.*

**Also here, we have modified our text (lines 512-522).**

*Line 490: There is of course no difference in the result when using different calibrations for comparing flux-weighted TEX86 and sediment TEX86! They are conversions of the TEX value, and that is similar for flux-weighted mean and surface sediment.*

**The reviewer is correct and we removed “independent of calibration used” (Line 509).**

*Data: I have not been able to find supplementary table 1, in which the according to table caption 2 the proxy values are*

**This supplementary table has now been added.**

*Typos etc.:*

*Line 319: ....LDI-based temperatures vary....*

**Corrected (lines 326-327).**

*Line 411: omit one “only”*

**Corrected (line 429).**

*Line 422: “northermost” instead of “most northern”*

**Corrected (line 440).**

*Line 437/438: delete “values” and re-word “the difference with summer temperatures ....”*

**Done (lines 455-457).**

*Line 453: “from 0.7  $\mu\text{m}$  pore diameter filters”*

**Corrected (line 471).**

*Table 1: refer to “upper trap” and “lower trap” instead of “trap top” and “trap bottom”*

**Corrected.**

*Table 3: I am surprised to read that satellite temperatures are available for 20 m water depth? I was always informed that satellites could only record surface temperatures. Please clarify.*

**The reviewer is correct and this has been modified.**