Response to Referee 1

Comments: de Bar and co-authors use biomarkers (long chain diols, TEX_{86} and $U^{K'}_{37}$) to reconstruct sea temperature variations in the river-influenced upwelling ecosystem off southern Chile during the past 150,000 years. They also compare the Diol Index and the nutrient dial index with other paleoproductivity indicators, including bulk organic matter total organic carbon (TOC), organic matter stable carbon isotopes (δ13C), as well as phytoplanktonic lipid biomarkers. The data set is interesting for a broad audience and the technical aspects of the manuscript are correct. In general, the manuscript is well organized. However, (i) there are a several assumptions, which need strong re-thinking and (ii) the MS would have greatly benefited from a quick read by native speaker before submission (several sentences are convoluted and difficult to understand, more a grammar than a scientific problem.) Below I list major comments.

We thank the referee for the positive assessment and for the comments, which we have seriously considered.

Abstract:

all abbreviations should be first fully written. Readers less familiar with them have no clue what the authors are referring to.

We will adjust this.

Introduction:

l. 15-20: The statement "Proboscia diatoms grow in the early stages of upwelling when nutrients strongly increase in concentration (Koning et al., 2001)" is wrongly interpreted and does not support the authors' interpretation that Proboscia is a diatom indicative of high productivity in the Chilean coastal upwelling system. If you keep reading Koning et al. (2001), these authors also mention that "The dominance of these pre-upwellers before the onset of the upwelling season was probably caused by their ability to adjust their buoyancy, which allows them to migrate to deeper levels below the euphotic zone to obtain the nutrients trapped there before the actual upwelling starts (Villareal, 1988)." Moreover, "The upwelling period was characterized by the successive dominance of three diatom species, Th. nitzschioides, N. bicapitata and Chaetoceros resting spores. T. nitzschioides dominated the assemblage in July, when the two-gyre upwelling system was firmly established, temperatures were the lowest and H4SiO4 concentrations in the surface waters were high"

We do not state that *Proboscia* is indicative of high productivity along the Chilean margin. Instead we refer to Tarazona et al. (2003) and Herrera and Escribano (2006) who both describe *Proboscia alata* as being dominant when upwelling is less intense, and thus when general productivity is likely to be lower. Moreover, we suggest that the Diol Index should perhaps be considered more as an indicator of *Proboscia* productivity, rather than general productivity.

Concerning the reference to Koning et al. (2001), we will rephrase the sentence: "*Proboscia* grow during the early stages of upwelling since they need little silica and they are able to migrate to deeper waters to obtain nutrients."

Specimens of Proboscia spp. are hardly found in sed traps samples (Romero et al., 2001, Deep-Sea Res. 48, 2673), and in surface and/or downcore sediments along the Chilean margin, and have never been associated with high productivity along the Chilean margin (Romero and Hebbeln, 2003, Mar. Micropal. 48, 71; Mohtadi et al., 2004, J. Quater. Sci., 19, 347; Romero et al., 2006, Quat Res. 65; Mohtadi et al., 2007, Quaternary Sci. Rev. 26, 1055).

This is correct. However, the main reason for its rare occurrence in sediments and sediment trap is likely the weak preservation potential (Jordan and Priddle, 1991; Koç et al., 2001; Jordan and Ito, 2002). Furthermore, we also do not claim that *Proboscia* is a dominant diatom genus along

the Chilean margin, and as stated above, we do not claim that *Proboscia* is indicative of high productivity in this region.

P. 3, l. 13: "...several glacial and interglacials periods”. Several can be four, but can also be 15. Your study extends only the past 150 kyr, be more concrete.

We will correct this.

Results

P. 9, l. 10-15: (i) “The average TOC content varies between 0.4 and 2.6%.“: average is not the same as range!; (ii) “The TOC content is significantly higher during the interglacial periods (MIS 1, 3 and 5) compared to glacial periods (MIS 2, 4 and 6)“: not quite true, values for MIS 3 are hardly distinguishable from MIS2 and 4. (iii) “During Termination 2. . .”: Terminations should be accordingly identified in Figs 3-5; (iv) “the TOC and TN contents increase rapidly (within < 1 kyr) towards interglacial values“: is the sampling resolution high enough to state that the increyse occurred within less than 1,000 years?

(i) We will correct this.
(ii) We agree that this is not formulated clearly, and we will adjust this.
(iii) We will indicate the terminations Figures 3-5.
(iv) We thank the reviewer for pointing this out, as this should indeed be “within < 2 kyrs”. We will correct this.

P. 9, l. 16-20: very convoluted sentence. Revise

We will revise this sentence as follows:
“The organic matter $\delta^{13}C$ record ($\delta^{13}C_{OM}$) also reveals a glacial-interglacial variation (Fig. 3c), corresponding to slightly ^{13}C-enriched values during interglacial times ($\delta^{13}C_{average}$ = –21.2‰) as compared to the glacials ($\delta^{13}C_{average}$ = –21.8‰). Although small, these changes are statistically significant (5% significance level, two-tailed $p < 0.001$).”

P. 10, l. 11-25: much of this information is related to Methods. It should be placed accordingly.

We will move the following sentence to the Methods section: *“Additionally, we quantified dinosterol, a biomarker for dinoflagellates (Boon et al., 1979; Volkman et al., 1998), as well as loliolide, an indicator of diatom abundance (Klok et al., 1984; Repeta, 1989).”*

P. 11, l. 26: see my comment above for the sampling resolution.

We will correct this.

P. 11, l. 26-31: this needs more accurate description. Revise.

We will extend this section in order to more accurately describe the different temperature trends.

Discussion

P. 12, l. 20-32: this part of the Discussion is very intricate and unclear. Please rephrase.

We will rephrase this.

P. 13, l. 5-10: why was no Chaetoceros peak during MIS4 when the MAR TOC was high?

We thank the reviewer for this comment, as we should indeed discuss this. As can be seen in Fig. 3, the high MAR_{TOC} during MIS 4 is linked to high sedimentation rates, whereas the peak in MAR_{TOC} during MIS 5 is not. This would suggest that the MAR_{TOC} maximum during MIS 5 actually resulted from increased primary productivity, whereas during MIS4 the high MAR_{TOC} resulted from the increased sedimentation rate. This would in turn explain that there is no peak in *Chaetoceros* counts for this age.

P. 13, l. 17-18 & l. 25-26: since Proboscia is not a secondary component of diatom assemblages in coastal upwelling systems not it is not associated with high productive waters along the Chilean margin, these statements should be thoroughly revised. See my comments above for Introduction.

We feel that the reviewer might have misunderstood our conclusions. As explained above, we argue that *Proboscia* is likely less abundant during intense upwelling in this region, and is therefore in fact <u>not</u> indicative of high productivity. In the introduction we explain that initially the Diol Index was proposed as an upwelling indicator since in general *Proboscia* is often associated with upwelling conditions. However, the actual conditions during/under which the species is abundant is often described as post-bloom, stratification, early upwelling season and/or the oceanic side of the upwelling front (e.g., Hart, 1942; Takahashi et al., 1994; Katsuki et al., 2003; Moita et al., 2003; Tarazona et al., 2003; Herrera and Escribano, 2006; Sukhanova et al., 2006; see references in Table 1 of Rampen et al., 2014b), likely because *Proboscia* is only able to compete with other diatoms when silicate concentrations are low.

P. 14, l. 2-5: This statement needs appropriate references/lab studies. Have different species of Proboscia been cultured the biomarker content measured in living cells?

We will clarify that Sinninghe Damsté et al. (2003) cultured these *Proboscia* species and measured the lipid composition. Moreover, we will also add the results of Rampen et al. (2007) who assessed the long-chain diol composition of *Proboscia inermis*, which also consisted for more than 90% of the C_{28} 1,14-diol.

P. 14, l. 7: It is not correct stating that "P. alata needs little Si to build its frustule". For diatom standards, frustules of Proboscia are long and build long chains (see Jordan et al., 1991, Diatom Research 6, 63).

Indeed, *Proboscia* diatoms have long chains but this is not the issue. Both Goering and Iverson (1981) and Sakka et al. (1999) suggest that *Proboscia* is capable of living under very low silicic acid concentrations because of weakly silicified frustules. Moreover, Jordan et al. (1991) state: "*The valves of modern Proboscia spp. are lightly silicified and thus their distribution in Antarctic sediments is restricted to regions of good preservation*". Thus, though the chains might be long, they are thin and weak.

P. 14, 4.3. Sea surface temperature evolution: the discussion in this section jumps back and forth between different time windows. This is not reader-friendly. Revise

We will revise this.

P. 14, l. 22-25: Does your SST record following "global climate pattern" refers to MIS5 or the entire record? Please clarify

We refer to the entire record. We will clarify this.

P. 15, l. 3: A correlation test helps to supports this statement.

We have performed correlation tests and we plotted these in Fig. 6.

P. 15, l. 7-8: This should be more rigorously discussed.

We will try to extend our discussion here.

P. 15, l. 26-27: Looking at your Fig 7, several mismatches in the SST behavior of compared records are recognizable. This should be more critically and rigorously discussed (see for instance ODP1241 and GeoB3327-5 vs ODP1234).

We will extend the discussion on this topic. However, we merely wanted to show here that our $U^{K'}_{37}$ record overall agrees with other records in the vicinity of our site, but that in fact these records also show many discrepancies which is likely linked to the latitudinal movement of the ACC. For instance, site GeoB 3327-5 is located at the northern extent of the ACC, and thus largely influenced by its latitudinal movement (Ho et al., 2012), whereas for ODP 1234 this influence might be less. The $U^{K'}_{37}$ record of ODP 1241 is especially hard to compare with our record since this site is low-latitudinal (6°N) whereas our site is located at 36°S, and thus the glacial-interglacial variability is much weaker at ODP 1241 as compared to our site. More extensive discussions are also getting us a bit outside the scope of this paper as our main focus is to test the applicability of the proxies based on long-chain diols.

The authors should comments and discussed on: - "The production/export depth of TEXH86 is not well constrained, thus complicating the comparison of TEXH and SST (for example, UK) based records." (e.g., Kim et al., 2012, EPSL 339, 95-102.; Ho & Laepple, 2018, Nat. Geosc. 9, 606).

On page 15 we already mention that the TEX_{86} might potentially reflect a subsurface signal, but that for this region it has been shown earlier that it likely reflects SST: *"For the TEX_{86}, it has been shown to potentially reflect subsurface rather than surface water temperatures (Huguet et al., 2007; Kim et al., 2010; 2015; Schouten et al., 2013; Chen et al., 2014) due to the production of isoprenoid GDGTs below the surface mixed layer. Overall, the TEX^{H}_{86} record agrees reasonably well with the $U^{K'}_{37}$ record for ODP 1234, suggesting that it mainly reflects SST. Also, Kaiser et al. (2015) who established a regional TEX^{H}_{86} calibration suggested that this proxy mainly reflects SST."*

- "glacial–interglacial amplitude of TEXH86- derived SST change in the tropics is overestimated relative to other proxy evidence, a result also independently found by a multi-proxy study in the subpolar region" (Ho & Laepple, 2015, Earth Planet. Sci. Lett. 409, 15–22; Seki, O. et al. 2014. Prog. Oceanogr. 126, 254–266).

We do not see why this is relevant as ODP 1234 is a subtropical site, and we do not see a larger TEX^{H}_{86} glacial-interglacial amplitude as compared to the $U^{K'}_{37}$ and LDI. Furthermore, Zhang and Liu (2018) recently showed that core-top TEX_{86} data between 30°N and 30°S strongly correlate to SST, which is in contrast with Ho and Laepple (2015) who proposed that TEX_{86} data potentially reflect subsurface water temperatures which would cause the overestimation in glacial-interglacial SST change. Moreover, as our primary focus is to test the long-chain diol proxies we suggest that such a discussion is outside the scope of this manuscript.