Response to Referee 2

de Bar at al. test the applicability of different paleoenvironmental proxies based on long chain diols (LDI, Diol Index, and NDI) by studying the ODP Site 1234 located within the Peru-Chile upwelling system and covering the last 150 kyrs. They compare LDI-derived SSTs with other temperature proxies (TEX^{H}_{86}, $U^{K'}_{37}$) and with the Diol index and NDI with other phytoplankton production proxies (accumulation rates of TOC and lipid biomarkers). Their results suggest that the Diol Index should not be considered as an upwelling proxy per se, and that the NDI might not be suitable as a more general paleonutrient proxy. I find this is an interesting study. I particularly appreciated the multi-proxy comparison for SST and productivity reconstructions. The overall manuscript is well structured and well written, even though some parts would need clarification. The data are robust and in general the conclusions are well supported by the data. I however think that some points in the discussion could be clarified/more detailed, as it is sometimes difficult to understand. Please find my comments below.

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

P. 2, l. 12: “mean annual sea surface temperature” instead of “annual mean sea surface temperature”.

We will correct this.

P. 4, l. 16: throughout the text you use either “ka” or “kyr”. I would be consistent and choose one or another.

We will correct this.

P. 9, l. 10: delete “average” as you talk about ranges.

We will correct this.

P. 9, l. 13: it would be good to indicate the Terminations on the figures.

Yes, we will indicate this.

P. 10, l. 8-9: the alkenone AR does not show this decrease around the boundary of MIS 4 and 5.

Yes, this indeed true, and we will mention this in the new manuscript version. This difference between the concentration and AR is likely caused by the increase in sedimentation rate around the MIS 4-5 boundary.

P. 12, l. 16-19: this sentence is not very clear and in contradiction. Please clarify.

We agree that this sentence is not clear and seems contradictory. We will clarify this.

P. 12, l. 19-32: this part is not really clear and relatively difficult to follow. Please rephrase.

We will rephrase this part.

P. 13, l. 1: “individual lipid biomarkers” instead of “individual biomarker lipids”.

We will correct this.

P. 13, l. 1-12: How do you explain the peak of MARTOC in MIS 4 which does not correspond to a peak in loliolide concentration/Chaetoceros abundance? There is also no peak for the other lipid

biomarkers, except for the 1,13 and 1,15-diols. Who are the potential producers of the 1,13 and 1,15-diols?

We thank the reviewer for this comment, since this part indeed requires discussion; a similar comment was also given by reviewer #1.
Fig. 3 shows that the increase in MAR_{TOC} in MIS 4 is concomitant with an increase in sedimentation rate, whereas during MIS 5 this is not the case. Therefore it is likely that whereas the peak in MAR_{TOC} during MIS 5 is mainly caused by increased primary productivity (as also indicated by the other records), the MAR_{TOC} maximum during MIS 4 is the result of enhanced particle settling due to the increase in sedimentation rate. Henceforth, we also do not observe any phytoplankton lipid biomarker peaks during this period.
The reason for the increase in both the concentration and AR of 1,13- and 1,15-diol around this period is unclear, as is the potential producers of these biomarkers. As explained in the Introduction, 1,13- and 1,15-diols have been observed in cultures of Eustigmatophyte algae, but these distributions do not agree with the distributions we observe in the marine realm, and thus the producer is still elusive.

P. 15, l. 12-15: this sounds surprising as you state in P. 4, l. 2 that ODP 1234 "lies in the vicinity of two large Andean river systems". Has the influence of terrestrial input on diols been observed for recent times in this site?

Yes, the core is located relatively close to the coast of Chile (~65 km) and the mouths of the Río Bio-Bio and Río Itata, which both drain large basins. However, this distance is likely still too large to detect riverine influence in the organic matter deposited in the sediment. For instance, in the study of de Bar et al. (2016), the BIT index and the fractional abundance of the C_{32} 1,15-diol was assessed in surface sediments along the river-influenced Iberian margin. High BIT and C_{32} 1,15-diol values were only observed for stations which were located < 15 km from the coast.

P. 15, l. 25-30: you could provide more details on the comparison with the other sites. Even though the general trend agrees well, there are clear differences with sites at proximity of ODP 1234 (GIK 17748-2; GeoB 3302-1).

We thank reviewer #2 for this comment. Reviewer #1 has a comparable comment which we answer here in a similar way. We will try to extend our discussion. 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 display many discrepancies that are 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. Moreover, we believe that this is somewhat outside the scope of this paper as our main focus is to test the applicability of the proxies based on long-chain diols.

P. 28, l. 8: a bracket is missing.

We will correct this.

Figures 3, 4, 6, A1: the tick marks on the axes (especially the axis with the Age) are missing. Please add them so the reader can associate more easily the values with the data points.

We will add this.