We would like to thank the Editor and both reviewers for the time devoted on our manuscript and their valuable comments. We provide a point-by-point reply below or next to each comment in green.

Anonymous Referee #2

Major comments:
Concept vs new findings: The concept of the influence of lateral transport on the age and SST estimates of alkenones is more than 20 years old (see Benthien & Müller, 2000 and diverse follow-up papers). Now this manuscript presents grain-size specific SST and 14C data which support this concept and point to the fine silt fraction as being mainly responsible for the observed effects. My main concern with the Ausín et al. paper is the way this is presented. The history of this concept is described in the introduction but it nevertheless reads as if the whole concept is new and Ausín et al. are indeed the first to present the concept. I am referring to sentences like: "Our results demonstrate that selective association of alkenones with mineral surfaces and associated hydrodynamic mineral sorting processes can alter alkenone signals encoded in marine sediments (14C age, content, and distribution) and confound corresponding proxy records (productivity and SST) in the spatial and temporal domain.". In this last sentence of the abstract, the first half of the sentence reads as if this was not known before. In contrast, this has been clear before and is just supported by the new data. The second half of the sentence, in contrast, is just an inference so far. It is logically to expect these effects but it is not actually demonstrated in the manuscript. Another example is the last sentence of the conclusions: "Our results highlight the importance of considering the influence of hydrodynamic processes (e.g., lateral transport) on sedimentary alkenone signatures (amount, age, and temperature) and their relationship to surface waters overlying the depositional location.". Since the works from Mollenhauer, Ohkouchi and others from decades ago it is known that the influence of hydrodynamic sorting on organic paleo signals in sediments has to be considered. Such sentences thus read like 'constructing a strawman' to oversell own findings. That said, I think the new grain-size specific data in itself carry enough value to be reported and the manuscript does not benefit from overselling and should be adjusted to be toned down.
The influence of lateral transport on alkenone 14C ages has been largely explored in detail by other authors since the pioneering work by Ohkouchi et al. [2002] almost two decades ago. In fact, the work by Benthien & Müller, 2000 was also cited. This is explained in the introduction (lines 43-46 and lines 78-81) and we do not intend to present such knowledge as a new concept arising from our results. We have added another paragraph to provide more background information (lines 46-49). By contrast, our study explores new research questions that, to our knowledge, have not been addressed yet: For instance, what is the role of alkenone-mineral associations regarding their preservation/degradation? To date, this question has been addressed for organic carbon and other specific biomarkers like GDGTs and fatty acids [e.g., *Peterse and Eglinton*, 2017] but not for alkenones. Also, are there specific mineral grain-size fractions that play a major role in the advection of allochthonous alkenones, or do they all have the same potential to introduce allochthous alkenone signals in marine sediments? Given the lack of other studies exploring sedimentary alkenone signals in fractionated sediments, these question has remained elusive, and we believe our results are novel in that regard. Finally, what can be said about the impact of allocthonous alkenones on alkenone-proxy signals? The vast majority the studies exploring lateral transport of sedimentary alkenones emphasize that such mechanism may cause temporal offsets giving rise to biases in the recorded proxy signal, but there have been very few attempts to estimate impacts on corresponding proxy-signals (SST through Uk'37 ratios and productivity through alkenone abundance). In sum, the novelty of our study resides in: i) the assessment of the role of alkenone-mineral relationships, a dimension that is missing in other works; ii) the assessment of the impact of hydrodynamic mineral sorting (specific to size fractions) on sedimentary alkenone signals; and iii) how i and ii impact corresponding alkenone proxy signals preserved in marine sediments.
In any case, the specific sentences mentioned by the reviewer have been rephrased to emphasize which concepts are known since long ago.

Biological oceanography: Another point which seems to be wrong is the interpretation of

alkenones being indicative for highest productivity in the Peruvian upwelling system used for explaining a warm bias (line 323 to line 325). This cannot be true as upwelling activity is driven by trade wind strength which is highest in austral winter leading to deep Ekman pumping which brings dissolved Si into the surface waters causing outcompeting of haptophytes by diatoms. The warm bias thus likely arises from the fact that alkenones are actually not produced during strongest upwelling, the latter associated with lowest SST.
In the Peruvian upwelling system, upwelling activity is highest in austral winter, but surface chlorophyll concentration is highest in austral summer and decreases during austral winter, in phase opposition with coastal upwelling intensity [e.g., *Echevin et al.*, 2008]. The paradoxical seasonal cycle of this region has been studied by other authors and is out of the scope of this paper. Regarding the blooming season of coccolithophores, a general misunderstanding exists. The latter mostly derives from the fact that coccolithophores generally dominate the phytoplankton community in oligotrophic waters (they outcompete diatoms in these cases). However, this fact does not imply that their absolute abundance is higher there than in productive waters. In fact, they appear in higher numbers when Chla concentrations are higher [e.g., Flores and Sierro, 2007]. This is, they might not dominate the assemblage in productive waters, but they are more numerous than in fully oligotrophic waters/periods. Indeed, diatoms outcompete coccolithophores in more productive waters and dominate the phytoplankton community at the peak of the upwelling (highest nutrient concentration, higher turbulence and colder waters). Yet, as shown in studies of spatial and temporal variability of coccolithophore productivity in upwelling regions, coccolithophores bloom right after diatoms, showing a strong preference for more mature upwelled waters [*Ausín et al.*, 2018; *Mitchell-Innes and Winter*, 1987; *Silva et al.*, 2008]. This is, more stable, warmer and lower nutrient waters, but still during the more productive season. In the sediments, this fine-scale temporal evolution (matter of days) is lost, and higher number of coccoliths are widely used in as an indication of higher net primary productivity. Specifically for the coccolithophore species that are responsible for alkenone production, and as stated in the text: “Alkenone producers E. huxleyi and G. oceanica are generally linked to eutrophic waters and periods of maximum primary productivity (Tyrrell and Merico, 2004). Recent work reveals a significant positive correlation between C37 alkenone concentrations from a global surface sediment compilation and maxima Chla in overlying waters (Raja and Rosell-Melé, 2021)”. Therefore, we believe our reasoning remains a feasible scenario to explain our results.

Errors and precision: Please state what the analytical and propagated errors are of compound quantifications and SST estimates. For instance, in table 2 alkenone concentrations (C37:2 and C37:3 and combined) are reported to the last digit. Is this reasonable with a usual GC-FID error of at least 10% for long-chain alkenone quantification? How does this error propagate to the UK37’-SST estimates? I guess that with a good error handling many of the reported ‘biases’ will actually be within error and only a few significant offsets will remain. Also, error bars should be added to all plots showing SST estimates and, preferentially, also instrumental SST data. We would like to thank Reviewer 2 for this critic and relevant comment on our paper. We agree that the paper could be improved in this regard and have therefore modify it accordingly. Analytical precision of Uk’37 was better than 0.003 units based on repeated measurements of an in-house alkenone standard. This statement has been added to the text. Regarding SST errors, we have added corresponding 1σ uncertainties of SST estimates derived from error propagation considering the analytical precision of SST and the 1σ uncertainty of the calibration (0.5̊C) reported by Prahl et al. [1988]. These errors are now plotted in Fig. 6 and mentioned in the header of Table 2. Errors for age offsets have been also propagated, and consider both, the propagated error of SST estimates and that of the annual-mean atlas SST. When these errors are considered, warmer SST offsets in relation to instrumental values for PER and NAT (and NAM to lesser extent) remain. We have modified the text accordingly and deleted the related paragraph from the conclusions as a “general warm bias” is not observed when considering errors.

Minor comments:
Generally, please replace ‘warmer bias’ by ‘warm bias’. Done.
Line 3: Please check affiliations. Bruni and Eglinton are not in Salamanca. Done.
Line 9: “…gaps remain on…” - consider wording. Done.
Line 29: Are alkenones really a large component of total OC of Emiliania huxleyi? Please check, I doubt this statement. We paraphrased and cited Prahl et al. [1988]: “*The long-chain alkenones constitute a major component (8.0 ± 2.9%) of the total organic carbon content of living*

cells of E. huxleyi". We did not find other estimates elsewhere, but considering the wide variety of organic molecules that contribute to TOC, we agree with Prahl et al. that 8.0 ± 2.9% can be considered a major contribution.
Line 106: "…in contrast with…" – consider wording 'contrast to'. Done.
Line 113: how was the grain-size fractionation done? Wet or dry sieving? Added.
Line 201: I would presume that the statement that only SST estimates from SBB and SMB are comparable to atlas data is not true when considering errors. Avoid arguing with 'comparability' and argue with errors instead. When considering errors, the following applies: "Sediment and atlas SST values from SBB, SMB and NAF fall within the associated uncertainties whereas temperature differences ranging from -6±0.6°C to +3±1.1°C are observed at PER, NAT, NAM and BER (Fig. 5B)" Also in the discussion "Alkenones from SMB, SBB and NAF are found to reflect local instrumental SST within associated errors (Fig. 5), while a positive discrepancy ranging from 0.8±0.5°C to 3±1.1 °C (towards warmer temperatures) is observed at other locations with the exception of BER (-6 °C±0.6°C)"

Line 204: I doubt that the statement of a general warm bias is actually true when considering uncertainties. Looks like only true for 2 out of 7 samples. Rephrased to: "SST discrepancies imply core-top SST is significantly warmer than surface water temperature at PER and NAT". NAM and NAF also show a warmer bias within the error, but much smaller.
Line 208: The statement that FS overall shows the smallest temperature offsets with bulk is not true. Please look at the data from NAT. I do not understand the significance of the following statement on larger/smaller offsets. Consider removing. We have rephrased it and replaced "overall" by "Except for PER and NAT" to be more precise. At SBB, SMB, NAM, BER and NAF, fine silt is the fraction that shows a SST most similar to that of bulk, and this is important because it highlights this fraction largely contributes to the SST signal measured in bulk sediments (via its high sediment mass contribution and alkenone content). Considering FS is prone to resuspension and might have been transported from other regions, it might be introducing a bias in SST_{bulk}. In fact, alkenone 14C results from FS samples indicate they contain pre-aged or older alkenones, already suggesting the asynchronous (thus, possibly allochthonous) origin of these biomarkers.
Line 318-321: In upwelling areas coccolithophores are outcompeted by diatoms during strong upwelling. See major comment above. Please see our reply above.
Line 380: should read SMB. Done

Tables:
Table 1: Namibian core – MC or BC? It is a MC, but the original name of the sample does not include this information in it labelling code. We prefer to keep the original naming.
Table 2: see comment on precision of data given. Please see comment above.
Figures:
Figure 2: Please add comment in caption about sand fraction in BER. Done.
Figure 4: I do not see the significance of panel B, just another representation of the same data (also no reference to B in caption). Panel A are the age results, panel B are the derived age offsets. Both panels are typically presented in works of this nature (please see Mollenhauer et al. [2005]) to help age offset visualization. Reference to B has been added to the caption.
Figure 5: panel B: see comment on errors. I suspect that data from SBB, SMB, NAM and NAF are actually within error with instrumental SST. Preferentially add error bars to SST estimates and instrumental SST data. In figure 5B, errors of SST offsets between measured and annual means are propagated considering the error of SST estimates, and the propagated error of the annual SSTs obtained from Ocean Data View (ODV), which considers the error given by ODV along latitude and along longitude. The propagated error of annual mean SSTs is given in Table 1.
Figure 6: captions for panel B and C are mixed up. Corrected.
Figure 8: What is the significance of the solid regression line considering all data. Consider removing. Removed.

References

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