**Community Comment #1**

## Summary

In this manuscript Novak and co-authors use published data to propose a correction for the original alkenone-based (Uk37') sea surface temperature data from ODP Site 882 (North Pacific) that spans the Plio- and Pleistocene and was published as part of an not public PhD thesis and (partly) in (Haug et al., 2005).

The reason for this correction is that the original alkenone data was not obtained using the established GC-FID technique, but with GC-CI-MS, which could introduce a bias. For this purpose, the manuscript presents an approach based on comparing the original GC-CI-MS data with more recently published GC-FID based data from a brief Pliocene interval for Site 882 (Studer et al., 2012) as well as published GC-FID based data from a Pliocene interval from nearby Site 883 (Novak et al., 2024). The main conclusion of this manuscript is that the original SST data from Site 882 is biased, predominantly overestimating the magnitude of SST change at Site 882, but that the main conclusions of the influential (Haug et al., 2005) paper still hold.

## Main Conclusion

The fundamental basis for this paper; namely that for the brief Pliocene interval covered by both datasets (~2750-2650 ka) the comparison between the GC-CI-MS-based SST data from Site 882 (Haug et al., 2005) with the GC-FID-based SST data from Site 882 (Studer et al., 2012) the data do not fall on the 1:1 line (e.g. Figure 3), is a valid observation. The other basis that GC-MS based approaches can lead to different UK37' and hence SSTs is also well-known (in this case I also suggest to include studies like (Hefter, 2008) into this manuscript). So there is clearly a basis that warrants a correction of the data and I appreciate the effort to correct (published) data.

However, in my opinion the approach presented in this version of the manuscript is too simplistic and needs to be more comprehensive. As such I recommend major revisions for this manuscript.

David Naafs 11th November 2025

Dear Professor Naafs,

Thank you for taking the time to evaluate our work. Your comments will result in a substantially improved revised manuscript. We are particularly appreciative of your

comments pointing out the flaws in our proposed approach to correct the ODP 882 sea surface temperature record. We outline the steps we will take in the revised manuscript to address your comments below. In particular, we would like to draw your attention to the addition of further GC-FID alkenone data from ODP 882 from (Yamamoto & Kobayashi, 2016) that broadens the "calibration" dataset for the correction to span the entire timespan of the Haug (1995) GC-CI-MS dataset. These additional data permit us to better characterize the nature of the systematic bias in the GC-CI-MS dataset and the associated uncertainties with our proposed correction to those data. Please find our specific responses to your comments below.

Warm regards and on behalf of the coauthors,

Joseph Novak

**Main Problems**

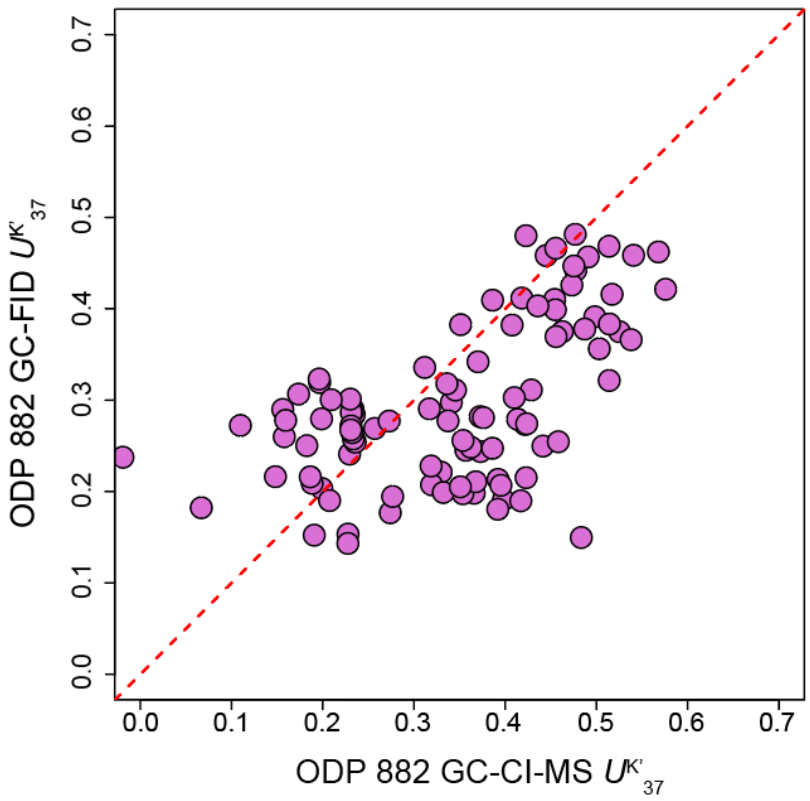
1. **Basis for (linear) correction for whole dataset is not well explained or supported by data**

The basis for the specific linear correction applied here is that the GC-CI-MS and GC-FID data for Site 882 do not fall on a 1:1 line AND that the same holds for the GC-CI-MS data from Site 882 and GC-FID data for Site 883 (shown in figure 3). However, the justification for why a linear correlation (eq. 3) is the best option to correct the data is not explained. Other options appear not explored using statistics. This while previous work suggests that the expected bias between GC-MS and GC-FID methods could be non-linear (Hefter, 2008).

Thank you for pointing out the Hefter (2008) paper to us. We were not aware of it and it provides useful further information about the nonlinear offset between GC-FID and GC-MS $U^{K'}_{37}$ values.

Our choice of a simple linear regression to correct the ODP Site 882 data was because of the distribution of the $U^{K'}_{37}$ values from Site 882 generated by GC-FID available from (Studer et al., 2012). Specifically, the Studer et al. (2012) data fall within two clusters rather than provide a continuous sampling of the full range of $U^{K'}_{37}$ values (and therefore SST estimates) in the Site 882 GC-MS $U^{K'}_{37}$ record.

The addition of the GC-CI-MS data from Yamamoto & Kobayashi (2016) substantially clarifies the nonlinear nature of the differences between the ODP 882 GC-FID and GC-CI-MS $U^{K'}_{37}$ datasets (see figure pasted below).




*$U^{K'}_{37}$ data from ODP Site 882 generated by GC-CI-MS vs. GC-FID.*

The comparison of the GC-CI-MS vs. GC-FID data from ODP 882 shown above closely corresponds with the comparison between the ODP 883 GC-FID $U^{K'}_{37}$ dataset and the ODP 882 GC-CI-MS $U^{K'}_{37}$ data shown in the original manuscript submission (Figure 3b); this comparison will be shown in a new supplementary figure. We will use this expanded ODP 882 GC-FID dataset as the basis of new discussion that addresses the following topics:

1). Statistical exploration of the linear vs. nonlinear relationship between the ODP Site 882 GC-CI-MS vs GC-FID $U^{K'}_{37}$ values.

2). The implications of this analysis for the shortcomings of the proposed correction to the ODP 882 dataset.

Lastly, we would like to add that we expect a likely outcome of the analysis presented in this work is that the ODP 882 alkenone dataset (both the original and our proposed correction) will no longer be used in data-model comparison exercises. We think the most important contribution of the manuscript to the literature is documenting the

issues with the ODP 882 record – the modelling community can choose to use the corrected values, or they may view the high degree of uncertainty associated with the correction as problematic. The important thing here, in our view, is that the issues with the ODP 882 record are documented for the wider community in a way that is understandable to non-specialists interested in using paleo sea surface temperature estimates to address hypotheses about past climate states.

Similarly, it is not clear why SSTs are used for this correction and not the raw UK37' indices. It is the index that is potentially biased, the SST is just a result from that biased index. And with the use of BAYSPINE, using SSTs might introduce an additional (non-linear) bias.

This was done to simplify the error propagation since the uncertainties of Equations 3–5 are all in terms of SST. We also thought that framing the offset in terms of sea surface temperature would be easier for non-specialists to interpret since translating the $U^{K'}_{37}$ index values to a sea surface temperature is not immediately intuitive to those who do not regularly perform this calculation. In the revised manuscript, we will correct the $U^{K'}_{37}$ values rather than transform the SSTs, since the only difference is that it requires some additional calculations.

Regarding BAYSPLINE: this does not make a difference here since this calibration function is linear within the range of $U^{K'}_{37}$ / SST relevant to the ODP Site 882 record (Tierney & Tingley, 2018).

In addition, the assumption that SSTs at Site 882 should be identical to those at Site 883 during the Pliocene and across periods of major climate change (e.g. iNHG) is not well justified in the current manuscript. Present-day SSTs are the two sites are not given for reference and we know that during past climate states like the Pliocene, sites in the same ocean basin can display differences in absolute as well as SST evolution (Naafs et al., 2020).

Site 882 and 883 are both located on the Detroit Seamount ~49 nautical miles (~91 km) apart. Given their proximity and the spatial autocorrelation of sea surface temperatures on this short length scale (Hosoda & Kawamura, 2005; minimum e-folding scale of SST variability is ~1° in the Kuroshio Region), we think it is reasonable to assume that the sea surface temperature records at these two sites should be very similar to each other, at least within the uncertainties of the alkenone proxy system. We will add sea surface temperature contours to Figure 1 to better justify this assumption.

Lastly, on several occasions the statistical evidence that is needed to support statements (and importantly the correction) is lacking. For example, in lines 132-134,

the manuscript states that the SST data from Sites 883 and 882 do not appear offset and this is used to justify the correction, but no statistical evidence is given. Same for lines 153-155, stating "more closely resemble" and "improved agreement" without statistical evidence to support these claims.

We will include statistics-based assessments of the corrected ODP Site 882 dataset in the revised manuscript. This will take the form of correlation exercises, t-tests, and f-tests to assess the similarity of the corrected ODP 882 dataset to the independent dataset from ODP Site 883.

The revised manuscript needs to take these comments into account, provide a proper justification of the methods used, as well as provide statistical evidence to support the approach.

We will do so.

1. **Correction applied outside calibration range**

The entire correction for Site 882 is based on a brief Pliocene interval (~2750-2650 ka) were both a GC-CI-MS-based and GC-FID-based SST data exist. For most of the GC-CI-MS-based data that spans the last 5500 kyr, there is no GC-FID-based SST data available (outside of calibration range). Thus, the entire correction assumes that the offset remained stable across all analyses. The manuscript provides no data to support this fundamental assumption. Details are missing, but I assume that the original GC-CI-MS data were obtained across a period of time, during which MS conditions might have varied. Normally, for each batch of GC-MS runs we would run a calibration curve to correct GC-MS to GC-FID UK37' values. Hence the assumption that the correction holds across the whole record might be invalid. I wonder whether other temperature records are available, for example for during the (late) Pleistocene for Site 882 to test this hypothesis of a stable offset?

The revised manuscript needs to at least acknowledge this caveat, but ideally addresses it with other published data and/or add a couple of new GC-FID SST data from across the last 5.5 Myr from Site 882 to confirm that the offset is constant. If not properly validated, I propose to only apply the correction to the Pliocene where GC-FID data is available.

Fortunately, we found additional published GC-FID alkenone measurements from ODP Site 882 that will allow us to directly address this comment (Yamamoto & Kobayashi, 2016). These data better characterize the nonlinearity of the offset between GC-FID and GC-CI-MS $U^{K'}_{37}$ values at ODP Site 882 (see figure in above response) and span both the

early Pliocene and Pleistocene portions of the ODP 882 record (Yamamoto & Kobayashi, 2016).

1. **Implications of corrections not clearly explained**

Assuming that following my comments in the revised manuscript the correction still holds, the authors need to expand on the implications of this correction for Plio/Pleistocene climate. Site 882 is quite an important site and the current correction leads to lower maximum SSTs and higher minimum SSTs (e.g. lines 132-134). For example, given the corrected record shown in figure 4b, the original warming across the iNHG (~2.7 Myr) that formed the foundation of the (Haug et al., 2005) paper, appears to be largely reduced (if not removed), especially when the data from around 2850 ka is taken into account.

We will take steps to discuss the implications of the proposed correction to regional reconstructions of late Pliocene climate. Please see our response to Reviewer #1 for a detailed plan of the additional sections we will add to the revised text.

We suspect that the comment about the implications of the proposed correction to the findings in the Haug et al. (2005) paper stems from our lack of a detailed discussion of the implications of the correction for a broader understanding of Plio-Pleistocene climate. Specifically, the warming feature in the ODP 882 record at 2.7 Ma was independently verified by Studer et al. (2012) (this is the GC-FID dataset we use in the proposed correction here) and is also seen at the nearby site 883 (see Figure 4c of Novak et al., 2024). We will add an additional section discussing the Plio-Pleistocene transition in the corrected record and the extent to which the shortcomings of the analytical methods of the Haug et al. (2005) paper amplified the warming signal at Site 882 across the 2.7 Ma transition.

**Minor comments:**

Line 32: both marine and terrestrial temperatures can be used for this purpose

The word “terrestrial” will be added here.

Line 50-53: also reference (Hefter, 2008) that introduces a method to use GC-MS to quantify UK37’-based SSTs (including a discussion on correcting for offset with GC-FID).

Thank you for bringing this work to our attention. We will cite the Hefter (2008) paper here also.

Line 73: justification for non-linear BAYSPINE calibration is needed

See response to previous comment. The BAYSPLINE calibration is linear in the temperature range considered here.

Line 85-90: The discussion of "instrument A/B" is not clear, revise and expand to clearly explain what this represents.

We will clarify this discussion. The intention is to make it possible for interested parties to look at the Chaler et al. (2003) paper and understand which equations we used and why.

Figure 2: the x-axis (time) stops at 2800 ka, but there is younger data shown. Make sure axis covers whole record

This will be corrected.

Figure 5: give r2 values for both panels

We understand the ask for a statistical test here, but correlation does not seem like the appropriate tool here. For example, two datasets can be correlated while not falling onto a 1:1 line, which is more so what we are interested in here. While we will report $r^2$ values, we suspect that the more important value will be whether the slope of the best fit line of the ODP 883 GC-FID $U^{K'}_{37}$ data and the corrected ODP 882 dataset approaches 1.

Line 200: Why is Prof Gerald Haug not co-author of this manuscript? It looks like the other scientists involved in creating the published Site 882 and 883 data are co-author and he was involved in discussions (line 208-209), looks weird to me. It would be a strong signal if the original author of the data is part of this correction.

We asked Prof. Haug if he would like to co-author this manuscript, but he indicated that he did not have the time to take this on given his other commitments.

## References Cited in Response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