REFEREE 1 Anonymous

The manuscript title "Paleoenvironmental and paleoclimatic reconstruction in the Western Mediterranean during the Late Early Pleistocene" by Catrain et al. is a nice paleoenvironmental reconstruction from ~1.25 to 1.06 Ma in the westernmost Mediterranean region, a key region for climate and human evolution. The study is based in the well know marine record ODP 976 located in the South Iberian margin. Authors used a multiproxy approach (planktonic foraminifera, pollen and clay mineralogy) to reconstruct past climate conditions. The data obtained have been compared with other pollen sequences from records throughout the Mediterranean and with climate reconstructions from various paleontological sites in southern Spain. The results obtained and these comparisons have led them to conclude that temperature reconstructions based on pollen from marine core 976 yield cooler temperatures than expected, and that there was a significant temperature gradient across the Mediterranean during MIS 31. The data obtained from clay minerals have been interpreted as indicators of source areas (Saharan and Sahel).

This study is closely related to a previous study by nearly the same authors that was recently published (Catrain et al., 2025 Vegetation and climate dynamics in the south-western mediterranean during MIS 37–31 (~1.25 - ~1. 06 Ma): Insights from the marine core ODP site 976 in Quaternary Science Reviews). Since the pollen database were already published and interpreted in that earlier study, this new paper is less relevant in terms of the new data it provides; however, it remains original overall because it presents new approaches and conclusions.

We would like to thank the first reviewer for their comments on this study. We would like to emphasise the new data introduced by this paper: foraminifera assemblages, sea surface temperatures (SST), temperature and precipitation reconstructions, and clay mineralogy.

This is a very interesting and comprehensive piece of work; however, I can only recommend its publication after major revisions for the following reasons:

We would like to thank the reviewer for constructive and insightful comments on clay minerals, as well as for their interest in clay mineralogy, as this will help us to clarify and improve this section of the manuscript. Two sections were added to the manuscript: 6.2 Provenance of clay minerals in the Western Mediterranean (line 442) and 6.3 Atmospheric and oceanographic implications (line 501).

***Clay mineralogy**: The authors indicate that: The error on measurement reproducibility is estimated to be ±5% for each clay mineral. (Line 187). Based on the methodology they have used and the significant issue posed by the preferential orientation of the clays, I believe this error range is optimistic; but even if we accept it as valid, the data obtained should reflect that error. The authors correctly represent the error range in the figures, but in the data, the values should be multiples of 5; I believe this would bring them closer to reality and make the limitations of the data more clearly. I think that indicate contents for palygorskite (~7%), kaolinite (~17%) or chlorite (~17%), should be changed to palygorskite (~5%), kaolinite (~15%) or chlorite (~15%) or (5% ±5), kaolinite (~15% ±5) or chlorite (~15% ±5).*

Lines 308-325: We acknowledge that the estimation of clay content remains only semi-quantitative, as clay minerals are assessed relative to one another. The error presented in the paper relates to the reproducibility of the measurements, i.e. the calculation based on three aliquot that underwent the entire separation process, followed by the analysis of each oriented mounts on three runs: it does not correspond to the precision of the measurements. Consequently, this reproducibility error does not

reflect the absolute or even relative error. As this calculation is semi-quantitative, we believe that the preferential orientation would likely lead to the same error range. In this context, we focus on the semi-quantitative variations in the respective clay minerals. This is why we prefer to use ratio, as I/K, which limits this effect.
We have decided not to round the data, but the error bars have been shown and we are cautious to avoid over-interpreting the data.

Based on this observation, I believe that distinguishing VI phases based on the signal from the clay minerals is an overinterpretation of the data (Line 285 indicate five main phases, but VI described, typo?). If error bars were placed around each data point, as was done with the temperature reconstructions, there would be two periods: one before middle MIS 34 (low Smectite) and one after MIS 34 (High Smectite). Defining period VI with a single point may not be ideal.

Lines 308-325: In accordance with the reviewer's recommendations, we have added error bars and reduced the number of phases to four main phases, with particular emphasis on two of them: one, with a high smectite content, spanning MIS 34 to MIS 32, and the other, with a low smectite content, during MIS 36 and 35. Furthermore, we also kept the characteristics phase enriched in K at the boundary between MIS37 and MIS36 and the uppermost phase, which shows a slight depletion in smectite during MIS31. We change the Figure 3 to have 4 zones (line 328).
We follow the recommendation of the reviewer and have now examined several assumptions in detail interpreting the clay mineral data.

I/K ratio used to retrace aeolian supplies and Smectite variations as oceanic advention proxy. I find that the evidence provided by the authors does not support this interpretation.

Line 502 and 527: I/K is commonly used as a tracer of provenance for eolian particles in the Mediterranean area. We do agree that not enough arguments are provided to support the hypothesis that both I and K are likely to be transported by wind at the studied site and we will complete the discussion accordingly.

At least since the Zanclian (Juan et al., 2020 Marine Geology), the Alboran Sea has been influenced by deep-sea currents that shape sedimentary formations, undoubtedly affecting the clay minerals that enter the basin and possibly also the types of pollen. Site 976 is located in a plastered drift that is sensitive to variations in the ocean currents flowing through the western Mediterranean (e.g., LIW or WMDW), which have changed over time.

Section 6.2: We fully agree with that point of view; smectite appears to be a good candidate for retracing intermediate and deep-water masses. Some previous studies based on clay mineral analysis of sediment trap have provided evidences that smectite could serve as a tracer of these water masses, assuming that smectite variations could tentatively be used in order to retrace modifications of oceanic transport.

Given the current state of knowledge, I do not believe it is acceptable to assume that the water column is transparent to detrital or pollen input. Eolian input is also transported to the ocean by the rivers in the present-day Iberian margin (Camperrós et al., 2026 EPSL), changing the original proportions of the source clay areas (e.g., Sahel or Sahara). It should also be noted that hemipelagic sediments could be enriched to 60% in eolian input but in conturites (sediment drifts) when sedimentation rates are high, *the eolian input becomes diluted and is therefore impossible to distinguish in the marine sediment, and site 976 has high sedimentation rates.*

Comparisons between hemipelagic sediments and contourites along the Iberian margin make it clear that heavy mineral and clay minerals (represented by elements such as K (illites)) undergo processes of focusing or advection when the intensity of bottom currents changes (e.g., Bahr et al., 2014 Geochemistry, Geophysics, Geosystems).

Section 6.2 : Indeed, the mentioned study provides evidence that heavy elements, such as Zr, can be used to retrace current velocity. The context if the study of Bahr is somehow different, as the studied site is located in the Gulf of Cadiz directly under the influence of the Mediterranean Overflow Water which has built the contourites. In this study, element ratios as Zr/Al are used to retrace the velocity of the MOW, as Zr is enriched in the coarse fraction, and is therefore linked to grain size distribution and current velocity. Nevertheless, the clay mineral study of the same sites by Moal-Darrigade et al. 2022, has shown that clay mineral variations in the contourite are controlled by the activity of the Mediterranean overflow water.

Changes in current intensity also cause significant changes in sedimentation rates, a phenomenon observed in core 976 during the period under study but for which no explanation is provided.

The sedimentation rate comprises biogenic, organic and detrital components that are difficult to distinguish clearly.

Changes in sea level also cause alterations in the deep-sea sediments of the Iberian margin (de Castro et al., 2021 Sedimentology); specifically, lowering/rising sea levels capture/carry away sediments from the continental shelf, which could account for the variation observed in the clay minerals known as Phase II, during transition periods.

Following the reviewer's advices, we now make a stronger case of sea-level changes.

Lines 484 to 489 : « During glacial period, low sea-level would promote the transfer from South Spain riverine systems enriched in illite and kaolinite and depleted in smectite, which Is not supported by the clay mineral assemblages at site ODP976. Similarly, during deglaciations, sea level rise would favour the rapid the remobilization of sediment from the shelf. By contrast, deglaciations are not associated with major modifications of the clay mineral fraction, given low confidence in this hypothesis. »

In that context, the increase of kaolinite during phase II, associated with the transition from Interglacial to glacial conditions is likley resulting from increased aridity as proposed at line 524.

Another possible explanation is that the increased influx of smectites may be due to greater soil erosion on the continent as a result of increased aridity and cooling in the region, and that the change in the I/K ratio may be due to a greater intensification of bottom currents as the climate becomes colder.

Section 6.3 beginning at line 503: We also now discuss this hypothesis in the paper.

"Physical erosion and chemical weathering are responsible for the diversity of clay assemblages, among which the very thorough studies of Thiry et al. 2000 on the paleoclimatic interpretation of clay mineral in marine deposits, shown that direct interpretation of variations in the composition of the clay fraction may be tricky. In particular, processes as chemical weathering and hydrolysis, which are responsible for the transformation of clays in soils, are slow-acting mechanisms, which raises issues of synchronisation and time lag between the moment when hydrolysis conditions change, the moment when the nature of the minerals formed changes, and the moment when they are finally transported and then deposited in the oceanic environment. Based on these assumptions, any direct link between modifications of weathering conditions on the continent and contemporaneous variations in clay composition within the Alboran sea is difficult to assess and is not supported by the whole dataset. »

It is undoubtedly difficult for the authors to know exactly what is happening (whether the currents are stronger, whether more river input, whether the sources of soils or wind-blown material are changing, etc.), but they should make it clear in the manuscript that there may be other factors not considered in this manuscript version could affect the record interpretation.

Section 6.2 and 6.3 : We follow the recommendations of the reviewer and are now discussing thoroughly several hypotheses while interpreting the clay mineral data.

Pollen record: *Can we rule out the possibility that variations in bottom currents do not affect pollen content? If the eolian material is primarily of North Africa origin and the riverine input is of Iberian origin, couldn't this introduce a bias in the interpretation of the pollen record, given that some pollen grains are primarily dispersed by the wind while others are not?*

We cannot rule out the impact of variations in ocean currents on pollen composition. However, the pollen data obtained at ODP Site 976 do not show variations in spectral composition that would imply a major impact of ocean circulation on them. Furthermore, wind-borne pollen originates from both Spain and North Africa; for this reason, we present these results as vegetation and climate data from these two regions.

The site ODP 976 precipitation and temperature shows marked changes with other near records (Palominas or Monte San Giorgo). It could be that the database being used isn't the most appropriate one? Authors also use the EAPDB database that covers Eurasia and the Mediterranean. Why do you not choose another one that covers only the Mediterranean?

Lines 235 to 247 : We chose the EAPDB database because it covers a wide range of environments, allowing us to best approximate the fossil pollen assemblages that record taxa no longer present in the Mediterranean basin today. To best compare the Palominas, ODP 976, and Monte San Giorgio site, we decided to use the same database for all three.

Assemblages of planktonic foraminifera. *Not only are pollen-based temperature reconstructions cooler than those obtained based in faunal assemblages, planktonik based SST also are significantly cooler than other records from the Iberian margin based in other proxies (e.g., alkenones). For site U1385 Oliveira et al., 2017 QSR reconstruct SST up to 20 ºC in SW Iberian margin and Temperate forest (%) shows clear correlation with STT. I find the explanation that the temperature data from Site 976* *represent a regional signal that differs from the local signal (paleontological sites, Line 562) to be somewhat inaccurate, because the data from Site 976 almost always show cooler temperatures than one might expect. Could this be due to local oceanographic factors such as upwelling activity in the Alboran Sea?*

Section 6. 1, line 399 : The SST reconstruction data appears cooler at our site than at sites U1385 and U1387 in the 2017 paper by Oliveira et al., because our SST data are derived from planktonic foraminifera assemblages, whereas in that publication they are derived from the Uk’ 37 index. However, the abundance of Neogloboquadrina pachyderma (with affinity to subpolar-polar environments) in our assemblages significantly lowers the values. The values themselves should be interpreted with caution, but variations in this parameter provide insights into what occurred during this time period. We do not believe that these lower values are related to local ocean dynamics.

I hope you find all these comments helpful for this nice study.

Typo in Line 110 Almeria Oran Front
Line 115 : Done

REFEREE 2: Timme Donders

My review of Catrain et al., "Paleoenvironmental and paleoclimatic reconstruction in the Western Mediterranean during the Late Early Pleistocene". This paper is a good multiproxy data and site comparison and is potentially a valid contribution that fits well in the CotP scope. However, there are a number or errors and missed opportunities that need to be resolved before publication. The research focus can be specified better (see below) and there is more clarity needed on what analyses have already been published and what not. For me and other readers the introduction should give a little more background into what method has what scope in general terms before providing the technical details in the methods. The language is mostly clear and the figures are well made.

The title suggestion can be a bit more attractive, e.g. Temperature and precipitation gradients in the Western Mediterranean during the Late Early Pleistocene. I have read the manuscript with attention to the overal resarch questions and approach, and palynology specifically. The X-ray methodology is not my background and so I have assumed methods to be up to standards.

We would like to thank Timme Donders for his comments, suggestions, and interest in our work. Below there are our responses:

The introduction addresses and interesting topic revolving around the intensification of glacials and mentions many important connected impacts, like the various driving factors in the EMPT discussion, and rightly addresses aridity as an important signal. However the manuscript fails to define a clear research focus for the paper. Yes multiproxy is important but what aspect of the climate system is addressed? This is unclear; is there reason to believe the aridity signal is misinterpreted or is the scale unknown? How does that relate to the orbital and atmospheric circulation drivers? The general problem and data scarcity are well laid out but the actual climatic problem needs sharpening.

Introduction lines 36 to 103In this study we try to understand the different responses of the climate system (land and sea) at the beginning of the EMPT, for different Mediterranean basins. The study of Catrain et al. 2026 shows some regional specificities in vegetation records around the Mediterranean region between the MIS 37 to MIS 31, and we want to see if these specificities are present in all of the the proxies of our study to try to understand the mechanisms and the conditions present during the EMPT for the whole basin.

In the introduction the (lines 899/90 the study does not make clear what data are new and what are based on earlier study (a few references are given but not very explicit). The pollen data for example are based on earlier work (Catrein et al, 2025) but it is presented in this paper as if it was new work, which is confusing.

We have followed the recommendation of the reviewer and better explained which data are new (planktonic foraminifera assemblages and precipitations, temperature and SST reconstruction) and which are based on earlier study Catrain et al. (2025) (pollen assemblages and oxygen isotopes) in the introduction (lines 92 to 100) and in the organisation of the Material and method (lines 152-156) and of results (Lines 275 to 281) where we remove the presentation of pollen method/results from Catrain et al. (2025).

The 3rd aim of methodological discussion of pollen-based climate reconstruction methods seems out of place as a focus, and would require a more in-depth methodological focus while here a multiproxy comparison is the key, as well as regional offsets. A thorough methodological discussion would require a different set up.

Regarding the transfer function approaches; the CAM method is specified only at the end of the method and should move up. Regardless of the approach, all methods have to deal with the non-analogue problems on these timescales as well as the presence of relict taxa outside the calibration range, and the suitability of marine data vs predominantly terrestrial calibration samples. While a multi-method is in principle good, not all these methods can handle non-analogue problems equally well. The methods are not interchangeable and some, like MAT, are particularly weak in non-analogue conditions. This requires more critical discussion in the methods section and prioritization of the (potentially) best suited methods based on e.g. Salonen et al, 2012;2019. (B)RT for example are statistical tools not grounded in ecological theory, although they perform well at the margins of the calibration range.

Section 4.4, lines 200 to 257: In response to this comment, we have reorganized the section on climate reconstruction methods to attempt a classification of the various methods. The method that seems most appropriate for the period under study is CAM, as it takes each taxon into account individually, particularly relict taxa. We then prioritize machine learning methods that do not rely on ecological theories, and finally, the MAT appears to be the least appropriate because there is no sufficiently close analogue to this Pleistocene vegetation.

Regarding the MAT results, also for the foram assemblages, I would also expect to see the nearest analogue distance reported.

We have added a map showing the distribution of the analogues to the supplementary data (Fig. S2), and the reported distances to the manuscript (Tab. S1).

The reporting of the climate results for method and per MIS is not adding much. It would be more useful to discuss in terms of mean-method offsets and trends over MIS stages. This listing of numbers is not helping the reader. Better organize along point like are the offsets the same in glacial and interglacial stages?

Section 5.3, lines 328 to 350: We have taken your comment on board and have amended our text to focus more on the discrepancies and variations between the MIS.

Table 1 is incorrectly labeled as Pearson correlation coefficient, which would be r., R^2 is the coefficient of determination, which is fundamentally different (I assume the R2 has actually been calculated, please check!).

Lines 363 to 368: We have corrected the caption for Table 1 because we are indeed presenting R^2, the coefficient of determination.

Also, do these (R2 and RMSE) represent values based on bootstrapping or leave-one-out validations as is common? The RMSE should be accompanied by the % of the climate calibration range and max bias, which are not given. Also the number of WAPLS components is not provided, not the evaluation plots (obs vs predicted) to assess this and the other methods. This is a shortcoming of the paper and needs improvement.

Lines 363 to 368: The R^2 and RMSE values are based on bootstrapping for MAT and WAPLS, and on leave-one-out validation for RF and BRT. The WAPLS has 3 components. We added these informations on the Table 1 caption and in supplementary data. Climate calibration range is provided in the table S2 and plots of obs vs predicted in figure S3.

The PERMANOVA tests are poorly explained (what are you testing?) and more suitable to the assemblage data itself rather than the derived climate variables; If anything, an ANCOVA approach would be more suitable here applied per MIS. In the current approach, pretending that the PERMANOVA tests are n.s. is not the same as stating that all methods are giving similar results which is clearly not the case. Since the test is not suitable the conclusions on this point are not valid. The variability between reconstruction methods is clearly different as one can see in a single glance in Fig. 5, as is the amplitude of the signals. So likely the test is not well defined (or chosen) to detect that. Is the centroid value for each MIS? Are the ranges normalized? Since the variability then might be similar but the absolute values clearly are not. For example, the RF and BRT range do not overlap at all and therefore cannot be the same. On a more fundamental level, it is highly unlikely that the marine SST changes have a higher amplitude than the terrestrial changes (as seem from fig. 8) given the thermal inertia of ocean water.

Lines 259-273: Clarifications has been provided to explain how to use PERMANOVA.

Lines 371 to 383 : We better explain and present the results of the PERMANOVA.

However, we cannot perform ANCOVA because the assumptions for this analysis are not met, as confirmed by the Mardia test: our data does not have multivariate normality, nor homogeneity of variances. Therefore, a non-parametric alternative such as PERMANOVA is the only solution. We have also added more details about PERMANOVA's null hypothesis and to explain the results.

To better respond to this statistical aspect of the reviews we had some help from our colleague Amandine Viala and we add her as co-autthor.

In summary, there are some important issues to resolve regarding data source, reporting of the data, the calibration data properties. The suitability of a transported marine assemblage relatively far from shore for surface-based calibration methods is not mentioned at all, while the low diversity data suggest important taxa selections happened that likely bias the reconstruction results. There is no link made between the clay mineralogy data and the potential source of the pollen, that could bias the ODP 976 data relative to the other sites. There is also no statement on sharing of data in online repositories (e.g. Neotoma, pangaea) which can be considered standard. This is a missed opportunity. Since the basic data are good I recommend a thorough re-think of some of the assumptions, the statistical tests, suitability/differentiation of the various reconstruction methods, and the research focus of the paper in the introduction. The result with the MIS 31 as not the main warm interval is surely of importance and can be more prominent.

We have followed the reviews of Timme Donders to improve our manuscript. The clay mineralogy results do not show similar trend with our pollen data, they suggest a large possibility for the sources of our pollen signal. The data will soon be available on Pangea.

Minor comments

L.41: important to specficy some key feedbacks, such as ice dynamics, CO2, regolith etc

L. 42 Done

L.52 the start of the EMPT .

L. 55 Done

L54/55 what archaeological evidence? No reference here or substantiation, neither for the subsequent statement on climate sensitivity

L. 56-60 We develop this sentence to clarify the point.

L59 …aridification in the Mediterranean was a major parameter.

L. 64 Done

L 70 'as support for climate reconstruction' do the authors mean that only these have yielded quantitative climate reconstructions? Or that there are pollenrecords at all with paleoclimate significance. The lake Ohird and Tenaghi Phillipon sites of course also fall in the latter category.

L. 75 : This sentence is misleading. What we meant to say is that, for the time being, only three data sets provide climate reconstructions based on pollen around the Mediterranean basin.

L74/75 specify time windows

L. 79 Done as specific

L.87 This study provides
Line 92 Done

L 91 'pollen data to reconstruct mean air temperature and precipitation' integrate this section with the first mentioning of pollen data in L. 89
L. 94 Done

L.93/94 What is a ' sea-contintent enrvironmental and climate outline'? Do you mean a Coupled marine-continental climate reconstruction?

Lines 97 to 100: We present the first comparison between two studies linking the marine and continental environments in the central Mediterranean (the Montalbano Jonico study ; Joannin et al., 2008) and our own, and we also compare several sequences (ODP 976, Palominas, and Monte San Giorgio) with climate reconstructions for the western Mediterranean basin.

L.96 again authors mention a comparison between central and western med; but they do not explain why this coimparison is useful or expected to carry a different climatic signal

We already have regional signal in the reconstruction of the vegetation based on pollen, we wanted to investigate if this observation can be seen also in the ocean tracers and the climate reconstruction.

L.148 HF (70%) and HCL (37%) digestion/treatment. What sieving was used, if at all? If this method however refrers to ealier work already published there is no need to report palynological methods here.
Lines 152 to 156: Indeed, the inclusion of pollen data extraction methods from the publication by Catrain et al. (2025) is confusing. We have therefore decided to cite the publication only once, at the beginning of the Materials and Methods section, and to refer to it from there onward.

L.149 is the minimum grains and taxa count an aim or result of e.g. counting one slide?
Line 153: This refers to the minimum number of grains counted per sample required to obtain a reliable representation of the vegetation. We have clarified this sentence to make it clearer.

L161 How was foraminiferal preservation? Any signs of diagenesis?
Line 162: No signs of diagenesis were identified.

L 207 "MAT … foraminifera" this phrase in not completed
Line 232: We correct this sentence

L222 and following. It is not clear whether the list of biomes are the ones excluded or included.
Lines 238 to 242: We clarify this part.

L240 Indicate the aim of the PERMANOVA analysis at the start of this paragraph
Lines 259 to 273: This clarification has been added at the beginning of the paragraph.

L308 .. a general mean value of around 10ºC…
Line 335: Done

L.364, "…the methods are adequate" in what way are they, in that they don't differ in mean value? In mean values they might not but the variability is strongly different

Line 371 : We have rephrased all the paragraph about PERMANOVA's results for clarity. Indeed, the result of PERMANOVA that comparing the methods for each MIS shows that, for most of them, the values reconstructed by the methods belong to the same centroid. And the one that compare the glacial and interglacial periods with each other for each method, however, all reject H0.

L.400 this quantitative terms this conclusion is only supported by the MAT reconstruction, which should be mentioned (while I agree regarding the assemblage data).
Line 415: We have added a clarification in the sentence, because the MIS 37 looks the warmest of the period in the temperature based in pollen but also in the SST, and these reconstruction are driven by the increase of the temperate forest and *G.ruber*.

L 452 "lesser influence" lesser than what?
Line 539: The influence of Atlantic input is lesser in central Mediterranean than in the Alboran sea.

L.455 this sentence on T. quinqueloba shoud be moved up to 451
Line 536 : We have moved the sentence up as indicated by the reviewer.

L 466-469 this section belongs in the methods and not repeated here
This section has been moved to the "Materials and Methods" section (lines 208 to 211)

Fig. 9 please indicate modern day value reference lines or points
Line 590: Modern values have been added to Figure 9, and the legend has been revised accordingly.

EDITOR

Dear authors,

since the first reviewer already brought up the issue with planktonic foraminifera assemblage based SST being too cold during some periods, I am posting this information already as an open comment and not only in my response as editor.

The too cold SST are driven by the presence of N. pachyderma that during that time period has not yet fully developed as a dominantly polar variant. We encountered the same issue at IODP Site U1387 in the Gulf of Cadiz and this has been discussed in:

Mega, A., T. Rodrigues, E. Salgueiro, M. Padilha, H. Kuhnert, and A. H. L. Voelker (2025), The Early–Middle Pleistocene Transition in the Gulf of Cadiz (NE Atlantic) – an interplay between subtropical gyre and extremely cold surface waters, Clim. Past, 21(5), 919–939, doi: 10.5194/cp-21-919-2025.

and to a lesser extent in Trotta, S., M. Duque-Castaño, T. Rodrigues, A. H. L. Voelker, P. Maiorano, B. Balestra, J.-A. Flores, A. Siniscalchi, M. Addante, and M. Marino (2025), High-frequency glacial climate instability during the Early Pleistocene: Insights from IODP site U1387 (Gulf of Cadiz), Palaeogeography, Palaeoclimatology, Palaeoecology, 674, 113041, doi: https://doi.org/10.1016/j.palaeo.2025.113041.

For the Mediterranean Sea, there is also the publication by Serrano, F., and A. Guerra-Merchán (2012), Sea-surface temperature for left-coiling Neogloboquadrina populations inhabiting the westernmost Mediterranean in the middle Pleistocene and the Pleistocene-Pliocene transition, Geobios, 45(2), 231–240, doi: https://doi.org/10.1016/j.geobios.2011.04.003.

So, in general, planktonic foraminifera SST reconstructions prior to 1 Ma in the mid-latitudes will have a "cold" bias and should be used with great caution.

Antje Voelker

We would like to thank the editor for this comment regarding the SST reconstructions and for sharing the relevant bibliography. Given this cold bias in our reconstructions, we are more interested in the variations in the parameter over time than in its absolute values. We have, in fact, added a sentence to this effect in the section interpreting the results.