RESPONSE TO RC1 Chris Brierley

We thank Chris for his helpful review of our paper, and for providing suggestions about how to make various aspects of the work clearer. Our specific responses are given below (in blue italic) and changes in wording are indicate in normal blue script.

I found Sect 3.1 a bit of a strange. It is very short and perhaps more importantly provided a substantial jump in meaning and technical depth from the previous section. You never redefine what is meant by "model", so I was stilling thinking of climate models here. You also do not provide much detail about what analysis is being done here. Clearly, this is not the focus of the manuscript. I wonder if it might be more appropriate to subvert this paragraph into the methods section. Alternatively, perhaps you could expand on this section and go explain what analysis is being undertaken at a little more length.

Our intention here was to provide an evaluation of how well the fxTWA-PLS statistical model performs and hence the reliability of the downcore reconstructions, but we agree that a section entitled model performance coming just after a description of climate model simulations could be potentially confusing and that the text here is terse. We will re-name this section and expand it to make the significance of these results clearer as follows:

3.1. Performance of the fxTWA-PLS statistical model

The assessment of the fxTWA-PLS statistical model through cross-validation showed that it reproduces the modern climate variables reasonably well (Table 1, Supplementary Table 4). The best performance is achieved by α (R^2 = 0.73, RMSEP = 0.15) and MTCO (R^2 = 0.73, RMSEP 3.7°). The models for GDD0 (R^2 = 0.69, RMSEP = 880) and MTWA (R^2 = 0.63, RMSEP = 3.22) were also acceptable. The slopes of the regressions ranged from 0.78 (MTWA) to 0.86 (MTCO), indicating that the degree of compression in the reconstructions in small (Table 1). Thus, the downcore fxTWA-PLS reconstructions of all the climate variables can be considered to be robust and reliable.

The definition of the region of interest seems to vary each time it is written. Please be consistent and accurate. L71 gives a region that seems to extend 10o further east than the final fossil site in Fig 1, and to start further poleward. If there is a change in extent due to the quality controls steps in Sect 2.2, then you ought to discuss this. L189 specifies a region within the Atlantic Ocean, but even after switching oW to oE does not correspond to any previous definition used.

The original definition (line 71) is the definition of the region as given by the EMBSeCBIO project i.e. 20°E – 62°E, 29°N – 49°N. It is true that we do not have sites beyond 52°E but we do have sites as far north as 49°N in Figure 1. Although we could define the region of interest as less extensive eastward, this might cause confusion with the EMBSeCBIO publications, so we prefer to keep the same definition. We use the same definition in line 109 but reverse the lats and longs, which is unnecessarily confusing. Thanks for pointing out the mistake about E and W in Line 189, which we have now corrected. The area used for the extracting the climate model outputs is somewhat smaller in terms of longitude, to ensure that the grid cells did not include regions outside our pre-defined EMBSeCBIO area of interest and because, as you rightly point out, there are no pollen sites east of 55°E. We should indeed have clarified this. We originally used all the model grid cells within this domain, but have now recalculated the model outputs using land grid cells only (see response to comments on this below). We will modify the text as follows:

Line 71: Here, we provide new quantitative reconstructions of seasonal temperature and plant-available moisture for 71 sites from the Eastern Mediterranean region (defined by the Eastern Mediterranean-Black Sea-Caspian Corridor, EMBSeCBIO, project as the region between 20°E – 62°E, 29°N – 49°N).
Line 109. ..... from the region between 20°E and 62°E and between 29°N and 49°N.
Line 189 (new line number 191). Outputs from each simulation were extracted for land grid cells in the EMBSeCBIO domain (20°E – 55°E, 29°N – 49°N; this region extends slightly less far eastwards than the EMBSeCBIO region as originally defined but there are no pollen sites beyond 55°E).

The impact of the CO2 correction is noted as having no major effect (L159). I'm not sure I agree after looking at Supplementary figure 3. Although the magnitudes don't alter, there is a 1000 year shift in the timing of the early wetting, which would shift the trend in Fig. 5 to visually appear anti-correlated with MTWA, rather than correlated with MTCO trends.

Thanks for raising this issue. We re-examined the calculations and realised that we were using mean annual temperature rather than mean growing season temperature as specified in Prentice et al. (2022) in our implementation of the correction. When we implement the α correction using the mean growing season temperature inputs, the reconstructed plant-available moisture is higher as expected and there is no lag in the timing of the wetting compared to the uncorrected curve. We have now corrected Figures 4, 5 and the supplementary Figure 3. We have revised the results in section 3.2 (line 228) as follows:

The trends in α differ from the trends in temperature. Conditions were similar to present around 11.5 ka (Figure 5). Between 11 and 10 ka, there was a rapid increase in α. Values of α were higher than present (>0.1) between 10 to 6 ka. Subsequently, there was a gradual and continuous decrease in α until the present time. The correction for the physiological impact of CO_2 levels was, as expected, largest during intervals when CO_2 was lowest (i.e. prior to 11 ka) (Supplementary Figure 4). The reconstructions with and without the correction are not statistically different between 10 and 5 ka, taking account the uncertainties in the reconstructions, but the correction produced marginally wetter reconstructions after 5 ka, with a maximum difference of 0.08. However, the gradually declining trend in moisture availability towards the present is not affected by the CO_2 correction.

Note that we have expanded the discussion of the α trends in response to a comment from Reviewer 2 (see below).

Fig. 5-9. Please can you consider keeping your temperature records on the same scale. The spacing is changes both within a plot (between MTWA and MTCO) and between the plots.

We agree that it is not entirely satisfactory to use different scales for the different figures. The difficulty that we have here is that there are marked differences in the range of changes in MTWA and MTCO (Figure 5), and between the simulated temperatures and the reconstructions (Figs 6, 7, 8), and indeed between the three reconstructions (Figure 9). This means that if we use the same ranges for MTWA and MTCO across all of the figures, it will be difficult to see the trends. Since we feel that the similarity of the trends is more important than the magnitude of the changes, particularly between the reconstructions and the simulations, we feel constrained to use different scales to make these more apparent. To improve readability, we have increased the number of scale categories and exclusively utilized whole numbers.

Fig. 9 needs a key, rather than the colors being provided in the caption.

We have updated this figure, adding a key indicating the three different sources for the reconstructions.

Your description of colors in the figure captions can sometimes vary from the colors that I've printed out – e.g. I see no green line on Fig. 5.

We have checked the descriptions. The caption for Fig. 5 should read "the dark blue line" rather than "the green line" and has been amended.

Please check the accuracy of your discussion of time trends. I feel that the cooling on L300 should be a warming - as time goes towards the present day.

We are indeed talking about time progressing towards the present day. The records show relatively warm conditions at the beginning of the reconstructed interval (i.e. the late glacial/Holocene transition) followed by a cooling towards 11ka and conditions remained cooler than before until 10ka after which the reconstructions show warming again. We have tried to make this clearer by amending the text as follows:

.... relatively warm conditions around the late glacial/Holocene transition (ca 12 ka) followed by a cooling that was greatest between *ca* 11 and 10 ka.

I was confused by the statement about 50% overlap on L152. Do you mean that there is a bin centred on every 150 year interval (so overlapping 50% with the previous bin)? If so, that bin would also have a 100% overlap in total, as it's other half is shared with the next bin. I don't have problem with this approach, but can you please be clearer as to what you are doing.

Yes indeed we have a 300-year bin centred on every 150 year interval so there is a 50% overlap with the previous interval and a 50% overlap with the subsequent interval. We have tried to make this clearer by rewriting the text as follows:

We then averaged the reconstructed values in 300-year bins (slightly larger than the average resolution of the records, 228 years) with 50% overlap. The first bin was centred on 150 yr BP, and subsequent bins were centred at 150 yr increments throughout the record. We excluded any bins with only one sample.

L339. You make a point about excluding marine records because they are not representative of the local climate. Your regional average does encompass the whole of the Black Sea, You might want to revisit your justification here.

The statement here is about potential reasons why the reconstruction by Herzschuh et al. differ from those we have made and also the reconstructions made by Mauri et al. The fact that Herzschuh et al. do not exclude marine records (whereas we do) is one factor that could explain this difference, but it may also be due to other methodological differences as specified. We exclude marine records in making our reconstruction because, according to pollen source area theory, they sample a very large area - in the case of the Black Sea this area is extremely large.

We recognise the potential confusion arising from excluding marine records in the reconstructions but not in the model simulation composites. We have now recalculated the model composites using only land grid cells. This does not significantly affect the magnitude or trend shown by the climate simulations, probably due to the models' coarse resolution, and thus this change has no impact on our results or discussion. As described above, we have now replaced the figures and clarified that we are only using land grids in the Methods (see response above).

L215 describes the earliest portion of the MTCO as having a cooling trend. I feel that a flatline could be drawn through this period in Fig 5.

It is true that the uncertainties associated with the early part of the record are larger than those after 10 ka, so some caution is required in describing these trends. Neverthless, there is an apparent cooling from 12-11ka and the coldest period is between 11-10ka, after which there is a marked warming. Given that there is a much more pronounced cooling to a minimum around 11ka registered in MTWA and GDD0, we feel that the MTCO trend is probably real. However, we agree that we should be more circumspect here and have rewritten this as follows:

Winter temperature showed a cooling trend between 12 and 11ka, with reconstructed MTCO ca 8°C lower than present at 11 ka (Figure 5). There was a moderate increase in MTCO after 11 ka, followed by a more pronounced increase of ca 5°C between 10.3 and 9.3 ka. Winter temperatures were only ca 2°C lower than present at the end of this rapid warming phase. There are relatively large uncertainties on the MTCO reconstructions prior to 10.3 ka, so the trends in the early part of the record are not well constrained. However, the phase of rapid warming between 10.3 and 9.3 ka (and the subsequent part of the record) is well constrained. MTCO continued to increase gradually through the Holocene, although multi-centennial to millennial oscillations were superimposed on the general trend.

L299: Please be consistent with the subscript on GDDO

Thanks for pointing out this inconsistency. We have used GDD0 through most of the manuscript and in the figures, so we have now changed the occasions when this was subscripted for consistency.

L193: Please spell out Moisture Index

We defined the moisture index in the Methods section (lines 97-97) as follows "and a moisture index (MI) defined as the ratio of annual precipitation to annual potential evapotranspiration". However, we are happy to spell this out again in the context of the model outputs. We have amended the text as follows:

and used to estimate the moisture index, MI, i.e. the ratio of annual precipitation to annual potential evapotranspiration, through the R package smpds (Villegas-Diaz & Harrison, 2022) before converting this to α following Liu et al. (2020).

L121: please add years after 6594

We have amended the text to read:

have a mean length of 6594 years and a mean resolution of 228 years

L196: Can you please specify temporal and/or spatial resolution?

The models are all transient, so here we are refering to differences in the spatial resolution. We have amended the text to read:

Since the spatial resolution of the models varies (Supplementary Table 3),

L230: Please remove “and approximately linear”

We have removed this phrase.

L274: regions -> regional

Thanks for pointing this out. We have made this modification to the text.