

Interactive comment on “Revisiting the humid Roman hypothesis: novel analyses depict oscillating patterns” by B. J. Dermody et al.

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Received and published: 16 January 2012

Response to reviewer 2

We are very grateful for the detailed review provided by reviewer 2. We have included a general response to both reviewers at the beginning of our response to reviewer 1 which should be read first. To avoid repetition we have numbered all reviewer comments and refer to these by reviewer number and comment number. For example, our response to the 5th comment by reviewer 1 is referred to as (reviewer 1, comment 5). Following is a point by point response to reviewer 2's comments.

1) The paper uses the new term ‘CNAO’ (Centennial North Atlantic Oscillation), interpreted as a low frequency version of the NAO, and distinct from the NAO on

the basis of different forcing mechanisms being relevant on different timescales. This is the first use of this term that I am aware of, and comes largely from the study by Rimbu et al (2003). The Rimbu et al paper suggests that the observed change in (mean annual) SST over the Atlantic-Mediterranean area during the Holocene is the result of a change in the mean state of the (winter) NAO. While the model in this case shows a change in the NAO during the Holocene, it is also clear that the magnitude of the simulated change in NAO is small, along with the impact on SST's and the climate system. For instance, the pattern of winter temperature anomalies in this and other models are dominated by radiative forcing from insolation change, and do not show (winter) warming in the continental interior of North America, Europe or Siberia that would be expected under a +NAO. Gladstone et al 2005 (Mid-Holocene NAO: A PMIP2 model inter-comparison) found at best only a weak NAO change in a few model simulations of mid-Holocene climate (with stronger forcing than would be expected during the Roman period), while more recent studies have found even less evidence such as Lu et al 2010 (Arctic Oscillation during the Mid-Holocene and Last Glacial Maximum from PMIP2 Coupled Model Simulations). The sub-tropical warming from mid-late Holocene in ECHAM3 and other simulations that is compared with the SST record is not due to change in NAO in the model (as is implied by the Rimbu et al study), but due to the small increase in annual insolation over low latitudes and the retreat southward of the tropical Monsoon. Therefore, while I am supportive of the interpretation of the data as being NAO driven, I do not believe that climate models are capable as yet of reproducing the magnitude of NAO variability necessary to explain the data in comparison with other alternative processes.

The authors do not undertake a simulation of the Roman period climate (no account is made for Roman period boundary conditions, including orbital and solar forcing for instance), but perform a sensitivity experiment based on 20th Century SST's. It is not established whether these SST's are a realistic representation of

SST's during the Roman period. The Rimbu et al (2003) (eg Fig 2) study shows conditions close to modern at this time, while Holocene SST's variability is anyway mostly within the uncertainty of the alkenone method (+/-1.5C) so this would be difficult to do. Perhaps it would be better to a) establish NAO changes during the Roman period using a comparison between data from Southern and Northern Europe (cool dry winters in the south should be related to warm wet winters in the north etc) and b) do not use a climate model, but use the instrumental record from analogous periods during the 20th Century. If a model is to be used, then it should use Roman period boundary conditions and evaluated against the data at a European scale.

a) Most of the issues mentioned above are addressed in the general response (Reviewer 1, General response). Additionally, we respond below to comments from reviewer 2 related to the experimental setup of our CNAO simulations. To reiterate though, we have excluded the CNAO simulations from the revised manuscript for reasons given in the general response, paragraph g.

b) The reviewer highlights that Earth system models of intermediate complexity (EMICs) fail to capture NAO-like changes during transient Holocene simulations. One of the main reasons for this is that EMICs have serious problems capturing the dynamics of the AMOC (Weber et al., 2007) which is a prominent driver of low frequency SST variation in the North Atlantic (Bond et al., 2001; Knight et al., 2005). Fully aware of the limitations of EMICs to capture low frequency SST changes we chose to prescribe SST anomalies based on the hypothesis of SST change during the period of investigation taken from the Rimbu et al. (2003) paper.

c) In the discussion paper we termed the atmospheric manifestation of low frequency SST change the Centennial North Atlantic Oscillation (CNAO) to distinguish it from the NAO which the reviewer rightly points out (reviewer 2, comment 14) has a very specific definition. On longer timescales the NAO index is demonstrated to exhibit correlation with low frequency SST variation (Roberston et al., 2000). However, there

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is considerably more noise in the NAO index compared with SST variations as it is derived from atmospheric SLP values. Therefore, as we mention in the discussion paper (P 2361, Lines 12-20), at shorter timescales NAO index values may not have a consistent relationship with SSTs due to the highly dynamic nature of atmospheric circulation compared with SSTs. Thus we applied monthly average SSTs kept constant year on year to examine the average atmospheric response, with noise reduced to a minimum, of the hypothesised change in SST and compare this with patterns exhibited in our proxies. This average atmospheric response is considered comparable with the low frequency atmospheric response captured in proxies arising from centennial (millennial might be more appropriate given the uncertainty of the alkenone method mentioned by reviewer 2, comment 1) changes in SST. Therefore, to be clear, our intention was not to force an NAO but examine whether the SST reconstruction of Rimbu et al. (2003) was mechanistically consistent with the NAO-like patterns exhibited in our composite proxy analysis. The details of our experimental setup are outlined on P2366 lines 4-16 of the discussion paper. We accept that we should have been more explicit in describing the motivation behind the CNAO experiments.

d) We accept the reviewer's concerns about the suitability of the Rimbu et al. (2003) dataset for comparison with our composite analysis given the uncertainty in the alkenone method. As mentioned (reviewer 1, general response, paragraph d) the revised EOF analysis has prompted us to reinterpret the signals in our proxies as a result of millennial scale oscillations in SST (Bond et al., 1997, 2001) rather than a Late Holocene trend in SST (Rimbu et al., 2003) as was done in the discussion paper.

2a) The authors find that Roman period deforestation had little effect on climate, although perhaps it might be better to say that deforestation had little effect on the model. I think that it is fair to say that the change in forest cover during the Roman period was locally significant, but relatively limited at the largest scale.

We realise that we have not been explicit enough in describing the research goals of the deforestation simulation experiments. The motivation of this paper is to build

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a clearer picture of climatic development during the lifetime of the Roman civilisation in the Mediterranean by assessing the respective contribution of anthropogenic and natural forcings. In this regard we were compelled to revisit the results of previous studies that proposed basin-wide aridification as a result of deforestation (Reale and Dirmeyer, 2000; Reale and Shukla, 2000; Dumeil and Gates, 2001). We agree with reviewer 2 that it is likely that changes in landcover around the Mediterranean were locally significant as can be seen in present day Israel for example (Perlin and Alpert, 2001). In the revised manuscript we have been explicit that our simulations are only indicative of basin-wide sensitivity to landcover change.

2b) However, the authors use a low resolution EMIC that I would not generally consider appropriate to study these processes over the Mediterranean region. The climate of the Mediterranean is strongly influenced by its diverse topography and geography, something that is simply not resolved in the model used which has an artificially low and uniform relief and simplified coastline. For instance, the mountainous relief of the Mediterranean results in strong orographic effects (eg rainshadows), while the land-sea temperature contrast in autumn and winter provides regional instability. This is important because the model is to be compared with proxy records that are located at many sites that may be subject to these local influences.

I would have thought it would have been better to use a GCM with a high-resolution downscaled regional model and/or a larger number of proxy records that can be scaled up to the resolution of the model.

The resolution used here is appropriate to register a large-scale, basin-wide signal as we demonstrate in Central Europe and as demonstrated by previous studies (Reale and Dirmeyer, 2000; Reale and Shukla, 2000; Dumeil and Gates, 2001). The reviewer states that it would be better to use a GCM with a high resolution downscaled regional climate model (RCM) that better captures the influence of orography on precipitation and compare this with a larger number of proxy records scaled up to the resolution

of the model. We would have reservations about the direct comparison of GCM or RCM output with proxies. In a paleo context an RCM usually receives its boundary conditions from an EMIC and therefore propagates the errors of the EMIC simulation at higher resolution. Therefore despite the higher resolution, there may be extremely large errors when comparing local climate in the model to proxy records. RCMs can be very informative in a paleo context for local sensitivity experiments as they can resolve a number of the important factors such as orography and mesoscale circulation the reviewer mentions. Therefore one may register feedbacks on the local climate as a result of deforestation which can highlight locations where proxy records may record climatic signatures of deforestation as a result of land atmosphere feedbacks. However,

2c) ..and/or a larger number of proxy records that can be scaled up to the resolution of the model. This could be obtained from pollen data (<http://www.europeanpollendatabase.net/data/>) for instance. The vast majority of pollen records in the European Pollen Database (EPD) do not provide transfer functions for precipitation.

We have gone through the EPD on a country by country basis and found only 1 record in the Mediterranean covering the period of interest that includes a transfer function for precipitation (Peñalba et al., 1997). We have excluded this record from the EOF in the revised manuscript as the amplitude of change between 1000 yr BP – 3000 yr BP is small in relation to the proxy uncertainty. Unless such transfer functions are supplied this data cannot be used in a quantitative way by researchers who are non-experts in the interpretation of pollen diagrams. We address the use of pollen data in landcover reconstructions in a separate comment (reviewer 2, comment 18).

3a) The data and the model are not integrated in a logical way. For instance, most of the proxies used to compile the data record reflect changes in annual precipitation, but model results are shown as change in evapotranspiration.

We have presented the simulation output of JJA large-scale and convective precipita-

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tion in the revised manuscript. The same regions that exhibited statistically significant changes in evapotranspiration do so for precipitation with reduced precipitation coinciding with reduced evapotranspiration. The reason we presented evapotranspiration anomalies in the discussion paper is because we considered that yearly precipitation anomalies would be dominated by the synoptic winter regimes and thus the signal of predominantly summertime convective precipitation would be drowned out. On the recommendation of reviewer 2 (comment 3c), we have displayed the summer precipitation anomalies so that the response to land cover changes are captured.

3b) The data is also shown at 100 year resolution, but it is not clear that the data has the sampling frequency and chronological control to be interpreted to such high resolution. The data is interpreted as indicating wet/dry conditions according to whether it lies above or below the mean for the study period. No account is taken of the uncertainty or magnitude of these fluctuations, which in many cases are probably less than the uncertainty associated with the proxy. Data synthesis should take into account chronological uncertainty and sampling resolution, as well as the uncertainty attached to the proxy itself if it is to be interpreted at this resolution. Certainly these should be incorporated into the figure, such as 14C dates with uncertainties, missing values rather than interpolated values where these are not available for the 100 year period in question, and some appreciation of uncertainty so that the smallest ‘wiggle’ in the reconstructed value is not interpreted as a significant change in climate.

See general response to both reviewers (paragraphs a and b).

3c) The data and the model should be compared using a more appropriate common parameter such as precipitation. This should be considered on a seasonal basis, since although the NAO is an important influence on Mediterranean winter rainfall, summer rainfall is influenced by different factors and may have contributed more to annual precipitation in the past than at present.

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We regard it is appropriate to interpret Mediterranean proxies of climatic humidity as primarily capturing winter precipitation given that the majority of rainfall occurs in winter (Peel et al., 2007). We do not expect the seasonality of rainfall to have changed significantly as the changes in insolation are minor between Roman times and present (Berger and Loutre, 1991). However, in the revised manuscript we have included a deeper discussion of the relative contribution of non-winter rainfall to annual totals in different parts of the Mediterranean to assist interpretation of the proxies.

Abstract:**4) P2356, Line 11-12: Change ‘since’ to ‘after’.**

Changed to after in the revised manuscript.

5) P2356, Line 11-12: Any conclusions drawn from the archaeological evidence presented from the Middle East is necessarily limited to that region. The authors also cite numerous examples where the evidence may be explained by non-climatic events. I would not have thought historical texts represent reliable documentary evidence, even if they are interesting.

It is fair to say that direct interpretations of climate based upon archaeological site distribution in the Middle East should be restricted to that region and we will be explicit about this in the revised manuscript.

At no point do our conclusions rely solely on historical texts. However the use of historical texts has widespread acceptance in the paleo community as a brief review of manuscripts in *Climate of the Past* will demonstrate. We are very careful about the interpretations we make based on historical texts and highlight the carelessness with which they can be used in other studies. However, given that many potential natural proxy archives are corrupted during the Late Holocene owing to deforestation, human induced soil erosion etc. it seems negligent for the paleo community to ignore historical texts which can provide a valuable supplement to the natural archives. For example

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the historical data given on P 2370 of the discussion paper describes the climate and landcover of specific locations for specific years in such detail as to facilitate direct comparison with present.

1. Introduction

6) P2357, Line 2: 1500 yr not 500 yr?

Changed to 1500 yr BP in the revised manuscript.

7) P2358, Line 9: **Tinner et al 2009 show evidence of Neolithic agricultural activity, not large-scale clearance; in fact they talk about natural afforestation after this period driven by climate change to more humid conditions.**

We misunderstood the description of 500 years of intense Neolithic agricultural activities as evidence of early forest clearance when it appears that agriculture took place in naturally open regions until climatically driven successional processes may have caused these opens regions to be populated by forest species, thus reducing their productivity. The use of large-scale is inappropriate.

8) P2358, Line 18-21: **As far as I can remember, the book of Joshua also says that God made the Sun stand still, stopped the flow of the Jordon river, and that the walls of Jericho collapsed as a result of some loud shouting. I am not sure that the Old Testament can be cited as a historically trustworthy document.**

I agree that much of the Old Testament is entirely unreliable as an historical document; however many other parts are quite specific about issues such as agriculture and other day to day activities of Iron Age life and are not religiously charged like the passage the reviewer mentions (see Borowski, 1987). However, the passage quoted is given in addition to a number of other sources and the argument that deforestation occurred prior to the Roman period is not dependent on it. Therefore we have removed it from the revised manuscript.

9) P2359, Line 14: **The Kaplan et al is a model simulation so to call it 'data' is**

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a little misleading since it is not measured land cover in the same sense as the word is used elsewhere in the paper.

In the Methodology section (P2364, Line 21) we state that the Kaplan dataset is derived from estimates of population and technological advances. In the revised manuscript we will refer to the Kaplan dataset as ‘maps of simulated preindustrial deforestation’.

10) P2360, Line 13-14: The authors will not be aware of this, but the study by Cheddadi et al 1998 is based on pollen surface sample data from Morocco by Fatima Saadi that has subsequently been found to have been corrupted. The reconstruction shows a large increase in precipitation in the late Holocene when most other evidence indicates that North Africa became more arid at this time (the period after the ‘Green Sahara’). In any case, you should always consider the uncertainties when interpreting these types of reconstructions, they often exceed the ‘wiggles’ shown in the data. See later comments.

We have excluded this dataset from our analysis (Cheddadi et al., 1998). We address the second concern in our general response, paragraphs a and b.

11) P2360, Line 17-19: The Jura is located on the Swiss/French border north of the Alps and is not in the Mediterranean.

We have excluded this proxy from the EOF analysis because we did not determine a satisfactory method to convert the count of high and low lake level scores to a quantitative value for input into the EOF. We have retained a discussion of the record in the context of the EOF results in the revised manuscript (Magny et al., 2004).

In addition we have excluded the $\delta^{18}\text{O}$ record of Wick et al. (2003) because that record from Lake Van, Turkey is incorporated by Eastwood et al. (2007) with pollen data from the same lake taken from van Zeist and Woldring (1978) and converted to precipitation (mm/y) estimates.

12) P2361, Line 8-9: The Rimbu paper does not establish that millennial scale

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variations in Alkenone SST's can be interpreted as centennial scale variations in the NAO. Only that the pattern of SST's during the mid-late Holocene is consistent with SST patterns that can be correlated with the NAO on inter-annual timescales. For instance 1) the chronological control of the Rimbu study is not sufficiently robust to interpret centennial scale variability, 2) the uncertainty of the Alkenone proxy is ca.+/- 1.5C and is in excess of most millennial (let alone centennial) scale variability (do not get confused with experimental uncertainty often quoted for this proxy at 0.2-0.3C). For instance on this basis 5 out of 10 Atlantic/Mediterranean and 7 out of 8 tropical Holocene Alkenone records in their studies are not statistically significant 3) Alkenones are used to reconstruct mean annual temperature, but it is not shown how mean annual SST's should reflect a pattern of SST's associated with the winter NAO.

As mentioned (reviewer 2, comment 1d), we accept the reviewer's concerns about the suitability of the Rimbu et al. (2003) dataset for comparison with our composite analysis given the uncertainty in the alkenone method and have modified our interpretation based on the results of our reanalysis (general response, paragraph d).

13) P2362, Line 1-2: If the majority of the rainfall falls in the winter months and the NAO is primarily a winter mode, why should proxies that reflect mean annual change in precipitation or moisture balance be better than winter specific proxies for investigating the NAO? It would suggest the opposite; that winter sensitive proxies are better for studies of the NAO.

The meaning of the sentence in the discussion paper is slightly ambiguous. Winter specific proxies are of course preferable but we found only limited examples of these for the period and location of focus. Therefore we use proxies of annual changes in climatic humidity as the Mediterranean receives the majority of its rainfall in wintertime and therefore annual proxies should capture a good deal of winter variability. In the discussion paper we also included two proxy indicators of autumnal climatic humidity for Israel (these have been excluded from the EOF analysis in the revised

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manuscript (general response, paragraph a)) as autumnal precipitation amounts in Israel are demonstrated to be linked to the westerly storm tracks (Ben-Gai et al., 2001). As mentioned (reviewer 2, comment 3c), in the revised manuscript we have included a deeper discussion of the relative contribution of non-winter rainfall to annual totals in different parts of the Mediterranean to assist interpretation of the proxies.

14) P2362, Line 10-12: This indicates a study that looks at the sensitivity of Mediterranean precipitation to Atlantic SST's and not the NAO, or 'CNAO'. The NAO has a very clear definition based on the pressure differential between the Azores/Lisbon and Iceland (or similar.. see Gladstone et al 2005). If you want to show that the NAO is responsible, then you also need to demonstrate the relevant change in pressure gradient.

Our reasoning for terming the prescribed pattern the CNAO is discussed previously (reviewer 2, comment 1c). The change in SLP arising from the prescribed SSTs is shown in Figure 6a-d with the associated change in surface air temperature (SAT) (Fig. 6b), 800 hPa winds (Fig. 6c) and precipitation (Fig. 6d). The SLP and climatic patterns are consistent with the anomaly between an NAO- - NAO+ (Hurrell, 1995; Hurrell et al., 2003). As mentioned, on long timescales SSTs are demonstrated to correlate with long-term anomalies in the NAO and AO index. Therefore we accept that a calculation of the NAO or AO index would have been instructive to relate the CNAO pattern hypothesised in this study to these indices. However, as we have stated (reviewer 2, comment 1c) it was not our intention to prescribe an NAO.

2 Methods

15) P2362, Line 24: The archaeological data is from the Middle East, and should not be interpreted as representative of the Mediterranean as a whole. Perhaps Eastern Mediterranean might be sufficient.

In the revised manuscript we have made this clear in the methodology section rather than waiting to the discussion section (P2372 Line 9-13).

16) P2364, Line 20-21: The Kaplan et al reconstruction is a modeled estimate and not empirically measured. You should be careful both here and elsewhere that the reader understands this. This dataset has yet to be evaluated using pollen based data for instance. It is also based on an invariant modern climate and soils, being driven only by changes in estimated population and technology.

In the revised manuscript we have rewritten as: Ancient deforestation was prescribed as a forested fraction of potential vegetation from 27.5N to 55N and 15W to 50E using a simulated reconstruction of landcover change based on population estimates and the contribution of technological advances (Kaplan et al., 2009).

17) P2364, Line 28: The Gaillard paper does not mention the Mediterranean specifically and is mainly focused on Northern Europe. Woody fraction can be estimated from pollen data (eg see the Tarasov et al 2007 paper cited in Gaillard et al 2010, and Williams papers cited therein).

Indeed, the Gaillard paper does not mention the Mediterranean explicitly (although a number of papers cited therein do) but is a rather comprehensive review of the issues surrounding Holocene land cover reconstructions. Therefore we will remove the use of the phrase 'for the entire Mediterranean' from the sentence to indicate that these are general issues not restricted to the Mediterranean. The Tarasov et al. (2007) method is interesting; however to our knowledge this method has not been applied to the region of interest in our study.

18) P2365, Line 1: I find it very surprising that the authors did not use pollen data in their study. Whilst it is true to say that there are some arid areas without many pollen records (North Africa for instance), it is also true to say that these areas have also not seen any significant vegetation cover in the past. There are many hundreds of pollen sites available to download from the European Pollen Database.

As the Gaillard et al. (2010) paper demonstrates reconstructing late Holocene land

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cover using pollen is no simple task and we believe it is beyond the scope of this study. Therefore we used a simulated reconstruction of landcover change based on population estimates and the contribution of technological advances (Kaplan et al., 2009). We would be more than happy to use a quantitative reconstruction of landcover based on pollen were it available. Such regional reconstructions based on pollen data have the potential to unlock a huge amount of climatic and land cover information that could be utilised by researchers in wide range of disciplines.

19) P2365, Line 3: 20 years appears to be a very short length of time to ensure equilibrium of the system, as well as to establish that the different experiments are demonstrably the result of the different forcings and not just internal model variability.

With prescribed SSTs the atmosphere in the AGCM of the Planet Simulator reaches equilibrium within a few years at T42 resolution. To check that the results were not a model artefact we added random anomalies to the SST field for the first year of simulations and used the original SST fields for the final 29 years and ran the simulations 3 times. The atmospheric response in the final 20 years was consistent, indicating that it was responding to the forcings rather than an artefact of internal variability.

20) P2365, Line 16 There are many proxy reconstructions of Mediterranean climate not used by the authors see for instance Mediterranean Climate Variability in Press. Link removed as causing problems in Latex

This is a chapter reviewing Mediterranean climate in the last 2000 years from an as yet unpublished book about Mediterranean climate. We are grateful to the reviewer for the recommendation and have used a 3 of the records in our reanalysis (general response, paragraph a).

21) P2365, Line 23-24: Unfortunately the authors do not appear to take into account the uncertainty of these reconstructions before interpreting their every wiggle. For instance, pollen-climate based reconstructions of annual precipita-

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tion can be expected to have errors in the region of 50-200mm.

We have addressed these issues in our general response, paragraphs a and b.

22) P2365, Line 27: The data is presented at 100 year intervals, implying a 100 year resolution, but then they are saying that they have interpolated when no sample was available. If there was no sample, then they should leave a blank in the figure. In any case, there is also the problem of chronological control at such a high resolution. When I have seen such figures in the past, they are also shown with the dates and their errors for each record, which at least helps the reader understand the probable uncertainties.

We have addressed these issues in our general response, paragraphs a and b.

3. Results

23) P2366, Line 4 onwards: I still do not understand where the CNAO has come from in the literature, and the link with SST's. Certainly it has been shown that SST's are important in determining the NAO, and have a weak predictive ability for the modern seasonal NAO, but this is not very clear in a GCM let alone an EMIC. For instance, the shift to a strong positive NAO that occurred in the late 20th Century is not well reproduced in model simulations.

As Robertson et al. (2000) demonstrate NAO patterns of atmospheric pressure are resolved in a lower resolution GCM than the one used here when SSTs are prescribed. However, as the reviewer states such models have trouble capturing NAO-like patterns when a dynamic ocean is used (further see reviewer 2, comment 1b). The reasoning behind referring to the low frequency atmospheric response to SST change as the CNAO was given earlier (reviewer 2, comment 1c).

24) Has it been shown that the model used by the authors (the Planet Simulator) is capable of reproducing the NAO?

The Planet Simulator was an ideal candidate EMIC for this purpose because it contains

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a dynamical core atmospheric general circulation model (AGCM) that has been shown to be well suited to studies investigating low frequency atmospheric teleconnections arising from SST variations (Donders et al., 2009; Fraedrich et al., 2005; Grosfeld et al., 2007; Romanova et al., 2006).

25) The two time periods chosen have different mean SST's but do these periods also show significantly different mean NAO? Using the CRU NAO dataset for DJF, I find the the NAO for the SST periods shown was 1904-14 NAO 1.30+/-0.5, 1984-94 NAO 0.95+/-1.2. This would suggest no significant difference in NAO occurred between the periods 1904-14 and 1984-94 despite the difference in SST.

The intention is not to force an NAO signal but to examine the response of climate in the Mediterranean to the prescribed SSTs based on an hypothesis derived from the Rimbu et al. (2003) paper. On longer timescales the NAO index is demonstrated to exhibit correlation with low frequency SST variation (Roberston et al., 2000). However, there is considerably more noise in the NAO index compared with SST variations as it is derived from relatively dynamic atmospheric SLP values. Therefore, as we mention in the discussion paper (P2361, Line 12-20), at shorter timescales NAO index values may not have a consistent relationship with SSTs due to the highly dynamic nature of atmospheric circulation compared with SSTs. It is not surprising therefore that the NAO index for a 10 year segment of the instrumental record does not correlate in a consistent way with SSTs. Additionally see reviewer 2, comment 1c and reviewer 2, comment 14.

26) It seems better to not use the model at all, and base the study on positive and negative modes of the NAO.

See general response to both reviewers, paragraph g.

27) P2367, Line 13-15: Is the change in ET flux over the Mediterranean the result of deforestation over Northern Europe? This would be a novel and interesting result. What is the importance of ET flux in relation to the proxies investigated?

These reflect precipitation and moisture balance so would these not be more appropriate?

The region we refer to (P2367, Line 13-15) is probably unclear from the text. We do not see any significant change in evaporative fluxes over the Mediterranean. What we refer to here is the change over the entire region where we applied deforestation in our simulations (P2364, Line 19-21). Therefore, we propose that the change climate caused by deforestation in the Mediterranean is not large enough to be registered in the proxies used in the composite (EOF) proxy analysis. As the reviewer states earlier though, it is possible that individual proxy records register local changes in climate owing to deforestation in the period of analysis. Thus the use of many records using varied proxies here is significant. The identification of spatially and temporally consistent patterns across a number of different types of proxies gives us confidence that our interpretations are based on large-scale patterns rather than local feedbacks arising from anthropogenic activity.

28) P2368, Line 4 onwards: A discussion of an east-west seesaw pattern of aridity/humidity is made. Could this be included in figure 5? Perhaps overlain as a line over the main figure?

See general response, paragraph b.

29) P2368, Line 16: ‘1500vyr’ should be ‘1500 yr’

Changed to 1500 yr BP in the revised manuscript.

30) P2368, Line 19: Would an aridity shift be shown if only the most significant changes were shown, rather than the shifts either side of the mean?

See the general response.

31) P2368, Line 22-26: If the intention is to suggest a change in NAO, then it would be more appropriate to calculate the change in NAO index. Also the season should be stated; is this winter, summer or annual data/NAO?

This is winter (DJFM). The intention is not to force an NAO signal but to examine the response of climate in the Mediterranean to the prescribed SSTs based on an hypothesis derived from the Rimbu et al. (2003) paper. Further, see reviewer 2, comment 14 and reviewer 2, comment 25.

4. Discussion

32) P2369, Line 18: Again, I do not really understand the relevance of change in ET. Precipitation or P-E would be more appropriate.

See reviewer 2, comment 3a.

33) P2371, Line 5 onwards: A number of examples are cited that appear to undermine the argument made earlier that the archaeological evidence can be interpreted in terms of climate change. The earlier arguments should therefore be caged to reflect the complexity of interpreting this evidence (as should the reference to historical literature)

We do not agree that this evidence undermines the previous statements. Each region discussed has different climatic and historical characteristics which we investigate in detail before making our interpretations. We agree with the reviewer that the interpretation of historical evidence is not straightforward as we demonstrate in our introduction with examples of misinterpretations. In the revised manuscript we have phrased this evidence in a more cautious tone as we accept it retains a high degree of qualitative interpretation, albeit based on convincing evidence.

34) P2372, Line 10: This acknow

We don't understand this comment

35) P2373, Line 1-3: Rimbu et al do not call this a CNAO index

Accepted. We have reduced the number of references to the Rimbu et al. (2003) dataset in the revised manuscript so this sentence does not remain.

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36) P2373, Line 6-10: I think that more evidence is needed to support the conclusion that the changes observed in the Mediterranean are due to the NAO. For instance, change in summer precipitation could be responsible, perhaps linked to a weakening of the sub-tropical high pressure (and related to the African/Indian Monsoon systems). The relationship to the Indian Monsoon is also known to invoke a east-west split. A logical place to look for supporting evidence would be Northern Europe, since NAO changes over Southern Europe should be linked. For instance a positive NAO would result in cooler drier winters over the S Europe, and warmer wetter winters over N Europe. See also for instance Holocene NAO reconstructions by Nesje et al 2000, 2001 Holocene glacier fluctuations of Flatbreen: : :).

See general response.

37) P2375, Line 7: This study does not look at the Holocene, only the late Holocene, so to say that ‘Holocene deforestation had little impact on the climate of the Mediterranean’ is inappropriate.

We agree and changed Holocene to late Holocene in the revised manuscript.

38) P2375, Line 11-13 and onwards: These statements should be more guarded. No conclusive evidence has been provided to invoke the role of the NAO. Palaeo SST’s were not used; this is a sensitivity experiment using a low resolution EMIC.

Agreed, this should be more guarded. As mentioned we have dropped the CNAO experiments in favour of a more detailed discussion of published literature so this sentence is removed from the revised manuscript.

Figures

39) Table 1: Study 4 is a lake-level study, not pollen based

Study 4 derives lake levels from mineralogy, diatoms and ostracods and compares this with pollen data from the same core. So, the lake level is not based on pollen but

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is validated using pollen. This record is not used in the EOF analysis in the revised manuscript.

40) Fig 1. Sites 3 and 4 should be in Morocco Atlas Mnts, not Tunisia. Site 5 should be in the Jura, not Alps. Sites 16/17 also look wrong.

Sites 3 and 4 are misplaced. Site 5 is slightly south of where it should be. Sites 16/17 are probably 50 km too far south. Greater care will be taken in indicating the location of sites in the revised manuscript.

41) Fig 2. The use of symbols rather than color would be better.

We considered this but the representation of c. 2500 points with symbols is difficult. We will strive to make Fig. 2 as clear as possible in the revised manuscript.

42) Fig 3. Again, why evapotranspiration? All the proxies cited in table 1 are for precipitation; what was the change in precipitation?

See reviewer 2, comment 3a.

43) Fig 5. Contrasting symbols would be better than color for wetter/drier conditions. The + symbol is often used to denote wetter conditions in eg lake level studies. See comments on Fig 1 about the location of some studies in the maps. The lower figure is misleading (see earlier comments where this figure is mentioned in the text) since data has been interpolated across the 100 year time slices. It would be useful to show chronological control (eg dates with errors).

See general response, paragraph b.

44) Fig 6. The caption is misleading since this is not a +/- CNAO (or at least you need to demonstrate this elsewhere). This would be better described as +/-SST.

Fig. 6 has been excluded from the revised manuscript.

Supplementary Information

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45) Please put the different proxy sources into a single table with a single age axis, and not randomly distributed across the worksheet.

The supplementary information will be expanded to include the proxy datasets used in the EOF along with the running mean values. The proxy records will be compiled in a table with a single age axis. For each record used in the EOF a plot of the running mean overlaid on the original proxy record is also supplied so that the relation between the running mean and original record is clearly displayed.

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