

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

### **Anonymous Referee #2**

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Dermody et al “Revisiting the humid Roman hypothesis: novel analyses depict oscillating patterns”

This paper investigates wet/dry oscillations shown in proxy records from the Mediterranean region during the Roman period, and the possible role of deforestation and the NAO.

I enjoyed reading the paper, and I especially like the integration of climate modeling with large-scale data synthesis.

The paper is well written, but while I find myself agreeing with the authors in many ways including the possible importance of the NAO, I find a number of problems with the interpretation and methodological basis of the paper. These make it difficult to

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support some of the main conclusions in the paper.

General points:

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 intercomparison) 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

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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 ( $\pm 1.5\text{C}$ ) 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.

2) 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. 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. This could be obtained from pollen data

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(<http://www.europeanpollendatabase.net/data/>) for instance.

3) 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. 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.

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. 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.

More specifically:

Abstract:

P2356, Line 11-12: Change 'since' to 'after'.

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

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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.

## 1. Introduction

P2357, Line 2: 1500 yr not 500 yr?

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.

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.

P2359, Line 14: The Kaplan et al is a model simulation so to call it 'data' is a little misleading since it is not measured land cover in the same sense as the word is used elsewhere in the paper.

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.

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

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

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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.

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.

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.

## 2 Methods

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.

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.

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).

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.

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 forcing's and not just internal model variability.

P2365, Line 16: There are many proxy reconstructions of Mediterranean climate not used by the authors (see for instance: [http://www.dsm.unisalento.it/webutenti/piero.lionello/public/book/mb\\_2\\_JL/old\\_version/Chapter2\\_2010\\_Final.pdf](http://www.dsm.unisalento.it/webutenti/piero.lionello/public/book/mb_2_JL/old_version/Chapter2_2010_Final.pdf)).

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

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

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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.

### 3. Results

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

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

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?

P2367, Line 21-24: How well can the model used by the authors simulate Mediterranean vegetation? I know that more sophisticated models have a great deal of problems with the representation of soil moisture, since this aspect is often poorly modeled. The role of soil water storage is very important in the Mediterranean since the storage of winter rainfall helps mitigate the effects of the summer drought.

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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?

P2368, Line 16: '1500vyr' should be '1500 yr'

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?

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?

#### 4. Discussion

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

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)

P2372, Line 10: This acknow

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

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, changes 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

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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. . .).

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.

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.

## Figures

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

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.

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

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

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).

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.

Supplementary Information

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

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