

Interactive comment on “Terrestrial climate variability and seasonality changes in the Mediterranean region between 15 000 and 4000 years BP deduced from marine pollen records” by I. Dormoy et al.

Anonymous Referee #2

Received and published: 7 April 2009

General remarks

This paper discusses climate reconstructions for the Mediterranean region for the period 15 to 4 kyr BP, based on three different methods and using pollen records from two marine cores. In my view, this could potentially be an important contribution to the literature, mainly for three reasons. First, the Mediterranean region is a key area for our understanding of climate response to external forcings (see for instance my comment F). Second, in the considered period many climate fluctuations have been recorded in the North Atlantic region at different scales, and it would be very interest-

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ing to see how/if these fluctuations are expressed in the Mediterranean region. Third, three different methods are applied that all use the same pollen data to reconstruct past climate, providing the possibility to critically compare the different methods and provide the reader an overview of the possible outcomes.

Although I see the potential of this study, there are several problems that have to be resolved before it is acceptable for publication in CP. Please find a detailed explanation below.

Major comments

A) Throughout the document, confusing and inconsistent terminology is used to designate the different phases seen in the records. A few examples:

In Table 1, Figures 3 and 4, the period before 14,700 yr BP is named 'Heinrich event 1', in Figure 2 'Oldest Dryas'. Heinrich events are defined as IRD-events recognized in North Atlantic Ocean cores. Using the term 'Heinrich event 1' is confusing here, since the cores discussed in this paper do not contain IRD. Consequently, it is more correct to use 'Oldest Dryas' here, as this has been defined originally as a pollen zone.

Throughout the paper, also terminology for periods defined in Greenland Ice cores (e.g. GI-1e2 in Table 1) is used alongside with bio-zones. I suggest using consistent naming for climatic events.

According to Table 1, the Older Dryas lasts from 13,500 to 13,400 years BP. This timing is inconsistent with the cooling phase labeled as Older Dryas in Figure 3 and 4. In Figure 3, the Oldest Dryas is placed around 14 kyr BP, in Figure 4 it is positioned between 14.0 and 13.5 kyr BP. It is essential for a manuscript dealing with many different climatic phases to use a consistent naming and timing of these phases. Please revise.

B) Related to the previous point: In Figure 3, 4 and 5, different climatic phases are designated by grey bands. These grey bands do not follow defined time limits based on for instance the Greenland Ice cores. Instead, the start and ending of climatic

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phases is based on the interpretation of the local climate reconstructions. The authors suppose a correlation with the reference records in which the climatic phases were originally defined. As a consequence, the timing of a climatic phase differs between the two cores in this study. If we take the example of the 8.2 ka event: according to the grey banding in Figure 5, in the Aegean Sea it is expressed by changes in climate from 8.2 to 8.0 kyr BP, while in the Alboran Sea the start is at 8.5 kyr BP and lasts until 8.15 kyr BP. This raises several questions. Are these differences in timing real? Do they reflect leads/lags, or are these due to uncertainties in the age models? And how are the start and end times of the different climatic phases actually defined? Furthermore, Figure 5 shows clearly that the different reconstruction methods suggest quite different climate anomalies around the timing of the 8.2 ka event. What reconstructed curve is used for the definition of the local '8.2 ka event expression' (i.e. the grey bands), or is the average reconstruction used? And is this definition applied consistently throughout the paper? How do the authors know that it is an expression of the 8.2 ka event if the three methods are not consistent? Could the response also reflect an artifact of the applied method?

All these questions are also relevant for other climatic phases, especially the brief and subtle Holocene climatic cooling episodes. In my view, the authors should be very careful with their claims to have found 'strong climatic links between the North Atlantic and the Mediterranean', given the uncertainties in timing and climate response. I suggest to only make such claims for links when all methods show a consistent response and when the timing is similar (within dating uncertainty) to the timing in the reference record. Consequently, I suggest revising Figures 3, 4, 5 as indicated and adjust the text accordingly.

C) Figure 3, 4, 5. I would suggest to remove the average curves from these figures, as the average has no meaning (i.e., it is not more reliable than the individual curves), and it makes the figures hard to read.

D) Section 2.1. This section should be extended and should summarize in words the

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main phases seen in the pollen diagrams.

E) Three methods have been applied to reconstruct temperature and precipitation. I am not an expert in these methods, and to me it is unclear from the manuscript how these methods deal with uncertainty. It would be useful to discuss this issue. In addition, Figures 3, 4 and 5 do not include uncertainty estimates. In these figures, a lot of wiggles can be seen, but without uncertainty estimates it is unclear if these wiggles represent noise or real climate anomalies. In my view, it is thus essential to provide uncertainty estimates, also to see if the claimed correlation to North Atlantic climate events can be substantiated.

F) I propose including a critical comparison with the MTWA reconstructions of Davis et al. (2003) for the Mediterranean area. In their reconstruction, MTWA shows a long-term warming trend over the course of the Holocene, with the period 8 to 6 kyr BP being significantly cooler than today. This strongly contrasts with the results presented here, showing mostly warmer summer conditions in the early-to-mid Holocene. Davis & Brewer (2008) have recently suggested that their Holocene warming trend is a response to the latitudinal insolation gradient. According to their hypothesis, the latitudinal insolation gradient is a very important climate forcing that has also dominated climate response during the previous interglacial. The results presented here by Dormoy et al. appear to contradict this hypothesis and it would thus be interesting to provide a discussion of this issue.

G) Page 753, lines 18-20. 'From this, we conclude that the models used here perform as well or better than other models used for climate reconstruction at a continental level in Europe'. This statement is surprising to me, since there are huge differences between the different reconstructions shown in Figures 3, 4 and 5. For instance, according to Figure 4, during the Preboreal the MTCO was about -15°C according to the PLS method and around 0°C according to the MAT method. And estimates for PWinter ranged from less to 50 mm to more than 300 mm. If all three methods are performing as well or better than other methods (as the authors state), it implies that the Preboreal

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climate is very uncertain for the Mediterranean region, as it could have been anywhere between much colder/drier than today and quite similar to today. In my view, it would be very helpful if the authors could provide a more critical evaluation of the different methods. Is it possible to provide a kind of ranking of the different methods in terms of suitability for reconstructing climate in the Mediterranean region at this spatial scale?

Minor comments

1. Why are ages expressed in yr BP instead of kyr BP? In my view, using yr BP suggests a degree of accuracy in the age models that cannot be warranted. I suggest using kyr BP. In addition, a consistent notation should be used. For instance, on Page 748, the following notations are used: cal yr BP, yr, years, yrs BP, yr BP.
2. In Figure 1, the average monthly temperature and precipitation curves are shown for the two regions. On what data are these curves based, and for what period do they represent the average? Please also provide references.
3. In Figures 3 and 4, yellow stars presumably indicate the modern conditions for the two cores, based on measurements. Please mention this in the captions.
4. Page 738. It is not clear to me why these two records have been chosen for this study, and not other cores with marine pollen records from the region. It would be helpful if some background information could be provided.
5. Page 738, lines 6-7. 'in the eastern and western.' Insert 'Mediterranean region' before the full stop.
6. Page 738, line 12. 'from climate three quantitative methods'. Delete 'climate'.
7. Page 738, line 13-14. 'A similar approach has been successfully applied to other regions and time intervals'. Please provide a few references here.
8. Page 738, line 14. 'It has been shown to field', should be 'yield'.
9. Page 739, line 9. 'assuming a reservoir ages of 400-600 years'. Delete 'a', and

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please explain the range in reservoir ages. For instance, during what intervals was 400 years used and when 600 years, and based on what arguments? Also, include a reference to Figure 2 in this paragraph, as the 14C dates are shown in this figure.

10. Page 740, line 2. ‘five to ten modern spectra’. How many are used here for the MAT method? Please clarify.

11. Page 740, line 13. ‘long species gradients’. It is not clear to me what is meant by long species gradients. Please explain.

12. Page 741, line 3. Please explain how summer and winter are defined for the reconstruction of PWinter and PSummer.

13. Page 742, line 26. ‘Greeland’ should be Greenland.

14. Page 743, line 3. ‘Bjorck’ should be ‘Björck’. Also on Page 744.

15. Page 743, lines 23-24. Please rephrase, because ‘MTWA anomalies’ do not correspond to winter temperature values.

16. Page 744, line 2. ‘three distinct climatic phases’. I do not see three distinct climatic phases during the YD in Figure 4. Please adjust.

17. Page 745, line 11. ‘negative anomaly than the present day’. Please rephrase.

18. Sub-titles Section 3.4. Please rephrase titles, remove ‘around’.

19. Page 746, line 4: ‘in increased in evaporation’. Remove second ‘in’.

20. Page 747, line 8. ‘reconstructing’ should be ‘reconstructed’.

21. Page 749, line 6. ‘The duration of this event is 200 to 300 yr’. I suggest referring here to Thomas et al., 2007 (QSR 26, 70-81), who show that the event lasted about 150 years according to Greenland ice cores.

22. Page 750, line 23. ‘at 4, 5, 6, 7 and 8 are’. Presumably there is an age notation missing here.

23. Page 751, line 6. 'Comparaison' should be 'Comparison'.
24. Page 751, line 19. 'MAT and NMDS/GAM model appear to have'. I suggest 'The MAT and NMDS/GAM models appear to have'
25. Page 751, lines 23-24. 'to be borne out in reality'. Unclear, please rephrase.
26. Page 752, lines 3-6. The sentence starting 'It is clear that the NMDS/GAM' is confusing. Please rephrase.
27. Page 752, lines 16-17. Since pollen in the Alboran Sea, the pollen'. There is a verb missing here.
28. Page 753, lines 4-5. 'In all cases PLS appears to perform relatively well'. On what argument is this statement based? Please explain.
29. Page 753, line 7. 'This we may be relatively certain'. Unclear to me what is meant here. Please rephrase.
30. Page 753, line 10. 'parameter' should be 'parameters'.
31. Legends Figures 3, 4, 5: 'PSL Method' should be 'PLS Method'.

Additional references

Davis, B.A.S. and Brewer, S., 2008. Orbital forcing and the role of the latitudinal insolation/temperature gradient. *Climate Dynamics*: doi: 10.007/s00382-008-0480-9.

Thomas, E.R. et al., 2007. The 8.2 ka event in Greenland ice cores. *Quaternary Science Reviews*, 26: 70-81.

Interactive comment on *Clim. Past Discuss.*, 5, 735, 2009.

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