

Dear Editor,

We would like to thank the three referees for their valuable comments on our article. The comments have been taken into account to revise the manuscript which has been modified accordingly. Please find below our point-by-point replies to the reviewer comments (in italics)

Answer to Anonymous Referee#1

-My main concern with this paper is about the lack of discussion of the estimates obtained by the pollen data. This is a very common behavior: the authors estimate climate parameters with their pollen data but do not explain what pollen changes triggered those changes in the climatic parameters and if they make sense. For example, in these estimations the summer precipitation maxima occurred during the Holocene climatic optimum. Following this study, precipitations were higher than 200 mm at ca. 7000 cal yr BP (Fig. 3). This was estimated based on what pollen species?

We used the Modern Analogues Technique to infer the precipitation estimates. In this method, the reconstructions are not based on only one taxon but are rather based on pollen assemblages (with the whole dataset) and on their comparison to present-day pollen assemblages (modern samples) and related climate. Therefore it is difficult to link climate changes to a specific pollen taxon. However we state that the increase in summer precipitation is correlated to the extension of the deciduous *Quercus* forest expansion on the Adriatic borderlands. The modern analogs selected around 7000 cal BP are samples collected in temperate deciduous forests from Italy, Bulgaria and Germany). According to the relevant comments from the referees, and because our concern is to provide a robust reconstruction, we have applied to the dataset a second method: the WAPLS which is also commonly used in paleoclimatology and which is a real transfer function (in contrast to the MAT). The resulting new figure now includes results based on both methods with errors bars (for the MAT); the figure shows unambiguously very similar results obtained with the MAT and the WAPLS. A short paragraph on the WAPLS description and on the method comparison has been added in the text.

This is quite a lot of summer precipitation. On the other hand, winter precipitation was lower (around 150 mm)? However, the area was then characterized by a Mediterranean climate (Fig. 1)? Could this be due to the river pollen transport from the Alps? Or are you assuming a summer monsoon? This is a very controversial topic reviewed by Tzedakis (2007).

Yes we reconstruct an important increase in summer precipitation which is corroborated by independent data collected in south Italy: we note at the same time high lake-levels (see Magny et al., 2012, Magny et al. this issue) which has been interpreted by M. Magny as an increase in summer precipitation.

In a new figure, the results based on the MAT and the WAPLS show unambiguously an increase of summer precipitation during the mid-Holocene. The climatic trend is very similar; however the range is quite different: for example, with the WAPLS the area was still characterized by a Mediterranean climate during the mid-Holocene.

The MAT summer precipitations may thus be slightly overestimated while the winter precipitations being underestimated. We think that the most important findings is that the climate trends inferred from both methods are very comparable, both evidencing an increase in summer precipitation at ca 7000 BP even if the reconstructed range is different.

We know the Tzedakis hypothesis. However, in this study we are not able to validate or not the Tzedakis assumption because it will be very ambitious to correlate the summer precipitation increase to a possible summer monsoon from our single record. What is probably correct is to state is that the Adriatic marine core is located at the junction of North-South and west-east climate influences, which may have caused the complex pattern characterizing the mid-Holocene. The core was then possibly influenced indirectly by the monsoon but our data alone do not allow to validate/refute this hypothesis so far. Further investigations are needed.

Concerning the river regime, the clay mineral record indeed evidences enhanced supply from the Po River (high I/K ratio) and/ or from local Rivers (high S/K; CPD submitted paper, figs 7 and 8) during the mid-Holocene. Such riverine supplies result either directly from increased precipitations over the drainage basin and/or from seasonal snowmelt flooding – both ultimately reflecting enhanced precipitations over the studied area.

Were temperatures (TANN) higher during the YD than during the Holocene? The lowest TANN of the past 13 ka was reached at ca. 7.5 ka? Please explain.

The new figure shows that the temperatures using the WAPLS were lower during the YD than during the Holocene, which makes sense. It suggests that the MAT might not to be relevant for reconstructing climate changes during the Younger Dryas and Preboreal (Ortu et al, 2009). However, the results obtained for the Holocene period are very consistent with both methods. Furthermore, the lowest temperature value reached at ca 7500 also makes sense. The anomaly pattern depicted here follows the “classic” pattern of the Holocene climate as shown by Davis et al (2003) (see the comparison between our curve and the reference ones from this work in old Figure 6 and new Figure 7) in which the lowest temperature anomalies occurred during the mid Holocene

Section 6.2. Precipitation

-What pollen species are giving us information about summer and winter precipitation?

Pollen based reconstruction are not based on only one or two species but on the whole assemblages. The composition of the assemblages and the associated climate is detailed in the table 2.

-The maximum in precipitation (PANN) is reached during the Holocene climate optimum. That makes sense. However, how do you know that the precipitations did not occur during winter, as indicated by the speleothem records (lines 21-23)? How do you explain climatically such high summer precipitations, higher than during the winter? –

Comparing reconstructed temperatures obtained using the MAT/WAPLS methods may help puzzling out such contrasted observations. Indeed, summer precipitations as reconstructed by the WAPLS are high but still lower than winter precipitations (see new Figure). Moreover, both methods indicate that precipitation occurred all year-long during the mid-Holocene: the amount of winter precipitation shows a regular increasing trend during the whole Holocene, while maximum precipitation that characterized the mid-Holocene seems to result mainly from summer precipitation increase (Fig. 3), as evidenced by the development of the altitudinal forest (Abies for example) associated with deciduous forest taxa (Fagus, Corylus, Quercus, ...). The contribution of summer precipitation thus appears to be most important during this specific time slice. The climate explanation for such high summer precipitations is thoroughly discussed in a paper based on the southern core studied in the LAMA project (off Tunisia, Desprat et al, 2013) and in the synthesis paper (Magny et al., 2013) of this special issue. Then it will have been redundant to include the same discussion in our paper and we thus refer to these two papers for an extensive and detailed debate on the subject.

It seems we have here expression of the regional climate. Perhaps it corresponds to an indirect influence of the monsoon given that the core is located at the junction of the North/south and West/east climate influences. In that frame, the discrepancies between our data and the speleothem records may thus reflect regional versus local climatic patterns.

Lines 11-13: If climate was driven by insolation changes the estimated temperatures would not record minima at ca. 7.5 ka.... they would record maximum values. -It is interesting to see how the authors explain the observed vegetation changes mostly triggered by changes in summer precipitation. However, this would imply important atmospheric reorganization during the Holocene, as summer precipitation is basically zero in the Mediterranean area Today and what really controls the humidity and thus vegetation is winter precipitation (and the North Atlantic climate dynamics).

It has already been shown that the temperature were lower at the mid Holocene in the south Europe and our record fit well with the general pattern developed by Davis and Brewer which is based on more than 500 pollen data. The observed vegetation at this time is not really a strict Mediterranean forest as lot of deciduous trees (Fagus, Corylus, Quercus, ...) and conifers (Abies) were more developed than the classic Mediterranean taxa (Q. ilex, Pistacia, Olea.), suggesting summers with more precipitation, associated with higher (up to 900 mm) than today PANN (annual precipitation).

Other minor comments:

Minor comments are taken into account in the revised version of the paper and I address below to specific questions.

Abstract: Change "to" for "look at" . Pollen data is plural: correct "allow us" Remove the second "vegetation" from line 10 Change "southern-western" for "southwestern".

Corrections have been done in the revised manuscript

Introduction: Change “heat” for “warming” (line 4). The last sentence needs to be rewritten: the authors analyzed the clay fraction, not just “dust” so I would change that sentence for: “By examining the clay fraction we will be able to discuss...”.

Corrections have been done in the revised manuscript

Text (section 2) and Table 1: AMS 13C ages? Radiocarbon dating is based on 14C decay...not 13C! Correct in page 14 (line 20) and also the reference by Siani et al., 2004!!!

The mistake between 13C and 14C was a typology error that has been repeated again and again in the text. We apologize for that.

Section 3.3. Line 17: what does “ecological significance” mean? How does it control the vegetal organization? Do you mean elevation?

Ecological significance means requirements. We will change this term in the revised version.

Section 3.4. Line 21: Italian “coast”.

OK.

Section 4.1. The lack of pollen in the upper 80 cm of the core is very interesting. The authors believe is due to poor pollen preservation, but related to what process? More oxygenation? Is this somehow related with climate change?

We have no specific explanation about this decrease in pollen preservation. We observed less pollen and the rare grains are badly preserved. May be it is related to oxygenation but the other proxies performed on the same samples do not bring any information that allows to explain that. So it remains uneasy to link this pollen lack to climate influences.

Section 5. Sentence starting in line 7: Please change: “This supports the paleoecological inferences coming from the MD 90-1917 core, revealing regional vegetation changes due to climatic events during the last 13000 cal yr BP in the central Mediterranean area.”

We have changed the sentence in the revised manuscript

-Line 18: Change “prevailing” for “prevailed”. Correct: “in the Adriatic basin”.

OK

-Paragraph starting on line 20: Add some discussion about why the Preboreal oscillation occurred earlier in this record than globally. For example, is it due to age control uncertainties?

We are not sure that this apparent time lag is linked to age uncertainties: it is a working hypothesis. Nevertheless it is really surprising that our pollen record is

consistent with continental vegetation data whereas pollen data displays a time lag when compared to ice core record and though marine data appear rather in accordance with the ice core record. If we have a look of the age error bars in the studied core, the time lag during the Preboreal when compared with the ice core record may be less significant as we have only two dates in this part of the core. In addition, Foramifer study experiences a low resolution during this time-slice and then could be less detailed to show the PB event.

-Line 23: I don't think Quercus, Carpinus, Corylus or Abies are thermophilous taxa...maybe "more" thermophilous taxa than the steppe plants but they are mostly temperate. I would then add "more thermophilous" taxa there.

We will change the term of thermophilous taxa in temperate.

-Page 14, line 20: 14C!!!

The mistake between ^{13}C and ^{14}C was a typology error that has been repeated again and again in the text. We apologize on that.

-In page 14 the authors discuss about the inferred SST records obtained by foraminifera and alkenones from the same core. Why are these plots not shown here? I think they would be very useful for comparison and interpretation of the pollen data.:

SST plots are included in another paper already published in the same special volume and we refer to this paper for convenience. However, we have added the foraminifer SSTs in the revised version of the paper to make the comparison easier.

-Line 23: Please explain why. Are planktonic foraminifera only affected by temperature? With respect to the vegetation, what kind of precipitation changes? An increase? Please specify.

Planctonic foraminifers are also sensitive to salinity changes that may be due to river inputs.

In fact, the PB – PBO events succession is marked first by increase in precipitation followed by a slight decrease before the general improvement of climate.

-Line 25: rewrite the sentence: "...dominated by Quercus with regular occurrences of Corylus, Carpinus..."

Corrections have been done in the revised manuscript

Page 16: Please use past tense when talking about the past. For example in line 2: "Quercus became less abundant while...increased"

Corrections have been done in the revised manuscript

Answer to Anonymous Referee#2

This paper presents a detailed pollen record from the Adriatic Sea, covering most of the last 13,000 years (the last 2000 years are missing). Reconstructions of vegetation and climate are discussed and compared with regional and global climatic records. Overall, the data presented here are sound and the interpretations of the findings are convincing. However, there are a few issues in the data presentation and the format that need to be addressed.

Age model: We are told that the age model is based on 21 AMS 14C dates (and not 13C, please correct throughout the manuscript), and INTCAL04 is used. That is puzzling as dates are derived from marine plankton, so Marine09 (a more recent database) should be used. Furthermore, there is no information about the reservoir correction applied here. If possible, add a column for R (or deltaR) and ideally, using CLAM, an age-depth graph with the probability envelop. Because we cannot verify here the data, one can question one of the concluding points, with regards to the time lag with the ice core record.

The age model was previously published by Siani et al. (2001, 2010) and was based on precise independent past and pre-bomb (see Siani et al. 2000) reservoir 14C age estimations. This means that we establish our age model independently without using a database as Marine 09 that is essentially based on the Cariaco record (Hughen et al., 2004; Reimer et al. 2009) which has been clearly questioned during the last Congress of Radiocarbon held in Paris in July 2012.

The next calibration curve INTCAL12 (special Radiocarbon issue in press) will not involve the Cariaco record and will match better with INTCAL04. For this reason we believe that our age model, also supported by a detailed tephrochronology study (Siani et al. 2004), is certainly the most robust that we could get.

As this corresponds to the age model used in the paper published in the same volume we think that it is better to use it in order to keep the homogeneity of age model used for the core 90-917 records presented in the different papers published in the special issue.

The mistake between 13C and 14C was a typology error that has been unfortunately repeated again and again in the text. We apologize on that and we have corrected it.

In section 5, page 1982, lines 11 to 15, SSTs are mentioned but not shown. It would strengthen the comparison if you could add it in your Fig 5 for instance.

Plots are in a paper already published in the special volume and I refer to this paper that is in the same volume

We have added the foraminifer SST in the revised version of the paper to simplify the comparison..

In section 6.1, page 1985, lines 10-15, I do not agree with the description of MTCO. In fact, they remain constant until 7700 y BP and then, you see fluctuations followed by an increase. MTWA show a strong decline until 7700 y BP. So, what we see here

is a decrease of the seasonality between the beginning of the Holocene and 7700 y BP. I suggest that you add a horizontal line to indicate modern values for each of the variables.

Yes it will be helpful to point the modern values and we will add it in the figures. We have done it in the revised manuscript. The general trend displays a slight but significant decrease in the first part of the Holocene which is more obvious in the Fig. 6 when using temperatures anomalies instead of temperatures.

Fig. 5: How were the anomalies calculated? Caption should be more accurate and say "temperature anomalies"

Anomalies are calculated as follows:

Temperature anomalies = reconstructed value – modern value

Structure of the paper some sections should be moved in order to have a more coherent manuscript. - Section 2 should be included in the material section. - Section 3.4 (pollen inputs) belongs to the discussion. - Section 3.5 (Clay mineral origin) possibly in the discussion. - Section 4 should be called Material and methods

According to the remark of the reviewer, we have changed the structure of the manuscript: these two sections (3.4 and 3.5) are now in a new item of the text. However, section 2 remains a separate part of the manuscript as it concerns only the age model and is not part of the new data, thus we prefer to separate this part from the analytical methods.

Some minor corrections were also annotated in the text.

Minor corrections have been taken into account in the revised version.

Concerning the mention of the Poaceae in the additional document, this taxon is not characteristic of the semi-desert/steppe vegetation. However, its increase is mentioned in the table 2 and we have now noted its presence as well as those of Asteraceae in the revised text.

Answer to Anonymous Referee#3

I think this is generally a good manuscript and important contribution to the understanding of Mediterranean ecosystem- and climate development during the Holocene. Palynological results from core MD90-917 have already been published by some of the authors in 1998, but I always hoped that these would be complemented with a better age model and higher resolution, and this is what this manuscript, together with interesting sedimentological results, delivers. There is no doubt from my side that these datasets should be published, and I also think that the different aspects of the discussion are worth publishing.

However, it seems to me that parts of the text and the figures were done quite in a hurry, and there are some mistakes, which can be seen even when only quickly scanning the manuscript.

E.g., like reviewers 1 and 2, I was a little puzzled that "AMS 13C ages" are mentioned at several places in the manuscript. The manuscript delivers four different wrong spellings of the name "Schmiedl" (related to Schmiedl et al. 2010). There are many mistakes in the figures and figure captions

The mistake between ^{13}C and ^{14}C was a typology error that has been repeated again and again in the text. A similar error has been done on the reference Schmiedl
We apologize on that. This has been corrected.

The English, while not overall bad, seems to contain several "frenchisms" (see below). Since I am not a native speaker myself, I cannot tell in some cases if the grammar is correct or not, but I would definitely suggest to have a native speaker carefully check the complete manuscript!

We took this comment in account and Simon Goring, who is a native speaker co-author, will check the manuscript.

The abstract is a particularly serious example. I will discuss this in detail below. The authors have already submitted a revised version of one figure, but there are several mistakes and editing problems in others. In the following, some issues are mentioned more detailed. I also mention misspellings where I found any.

Some points concerning the content/interpretation have already been discussed by reviewers 1 and 2, I only mention additional points I found.

Abstract

1971, 4: "pollen data... allows us" change to plural: "pollen data... allow us" The first paragraph of the abstract is something I would put in an introduction, but not in an abstract. But even for an introduction, the first statement would be too imprecise. Of course, the past can be key to the future, but this sentence sound like future ecology in the Mediterranean will only return to earlier states. Generally, the abstract does not give any precise information. Shifts are mentioned (from what to what?), changes in precipitation are mentioned, but not quantified - although absolute values are one of the strong points of this publication!

OK, the abstract has been rewritten.

1971, 21: Is it really necessary to state that multi-proxy-approaches are a good thing? I suggest to completely rewrite the abstract. Leave out unnecessary points, and give more precise statements of what you have found!

We have changed the abstract in the revised version and add quantified values for summer precipitation.

Introduction

The first two paragraphs appear quite complicated to me. I am sure it can be shortened significantly. Furthermore, some sentences are strange, e.g.:

1972, 11: "... past shifts in precipitation may help to envisage..." This sounds like the shifts themselves are doing interpretations. Analyses of past shifts may help... 1972, 18: "Its central location.... should be highly sensitive..." The location itself is certainly not sensitive, but the regional climate and the ecosystems. Of course, everybody will understand what you are meaning, but still, you should avoid such sentences. There are more examples throughout the text. The second half of the introduction is okay in my opinion.

The introduction has been modified in the revised version. Nevertheless, some general sentences – modified – are necessary to underline the interest of the Adriatic basin in illustrating climate connections as it is located right at the junction of conflicting influences.

2 Lithology and age model

The point with 13C vs 14C dates was already mentioned by the other reviewers.

The mistake between 13C and 14C was a typology error that has been repeated again and again in the text. We apologize on that.

3. 1 Climate and atmospheric circulation patterns

1974, 7: Why not "Azores High"?

OK

5 Vegetation and climate for core MD 90-917

1981. 16 "Combourieu Nebout" instead of "Combourieu Nebout" for the 2009 paper (see p1986, l12, although it is probably a strong spelling? Occurs several times in the text.

There are two ways to write my name but for bibliographic purpose and citations overview the Combourieu-Nebout spelling is preferred.

1981, 16: The Younger Dryas is only indirectly mentioned in Kotthoff et al. (2008, QSR), but discussed in more detail in Kotthoff et al. 2011 (JQS) which is already mentioned in the references, and the vegetation during the YD in the Eastern Mediterranean is discussed in Kotthoff et al. (2008, The Holocene),

We have changed the references

1982, 11-1983, 5: You seem to avoid discussing the second decrease in temperate forest pollen after the PBO (around 10 900 yr BP according to your age model). Interestingly, such a second decrease can also be found in Italy (Monticchio, Allen et al. 2002, Quaternary International) and in the Aegean region (Kotthoff et al., 2008, The Holocene). Does the question mark in Fig. 4 imply that you are not sure about the correlation with the ice core data, or not sure about the data point? Compare also comments to section 6.1!

I am sure of the PBO oscillation (increase in PB event and a following decrease just after (recorded by three samples) but the second event around 10 900 yr BP - underlined with a question mark – is more questionable because it only corresponds to a single point

6.1 Temperature pattern

I may misunderstand something here. You write: 1985, 10: "The lowest MTCO in the record occurs during the Preboreal anomaly, before 12 000 cal yr BP." I can see two significant declines in the MTCO, one at around 13 000 yr BP, at the onset of the YD according to your own interpretation, (fits well with other records) and a second around 10 900 yr BP, which is related to the decline in temperate forest pollen at the same time (see comments to section 5!). The PBO, according to your Fig. 4, is at around 11 800 yr BP (I agree with you, in spite of the slightly too old age that this may correlate with the decrease visible in NGRIP at 11 400 yr BP...). Your data does not reveal a significant decrease in the MTCO during the PBO, but in the MTWA. In the following:

1985, 24: "Temperature reconstructions indicate several cold..." you again do not refer to this event after the PBO. I think, however, this should be discussed in more detail.

We have changed the discussion to make it clearer and to delete some mistakes in the revised version. For instance:

- "before 12 kyr" which indeed refers to the onset of the YD is thus replaced by 'YD'.
- The PBO anomaly is indeed characterized by a significant decrease in the MTWA and not MTCO (CPD submitted paper, Figure 4), and by a slight but significant temperature MTWA anomaly (CPD submitted paper, Figure 6)
- Moreover, the figure 6 displays anomalies for the last 12 000 yrs and thus does not show the onset of YD.

As mentioned above, the decrease at 10900 correspond to the event pointed in the figure 4 by a question mark. It seems that the slight decrease in temperate trees recorded by only one sample severely alters the climate reconstruction and temperature anomalies. We have changed the text accordingly to moderate the interpretation of this event.

In your Fig. 3, it looks like the event around 10 900 (I assume, it can be correlated to events around 10 500 yr BP in other records) is not just reflected in one sample, but in three subsequent samples...

We can hardly interpret the short event at 10.9 (recorded in the pollen record by one point) without more precise analyses. Nevertheless, although temperature decrease correspond to a single point, the associated temperature anomalies correspond to three points during this period, which may support the significance of the observed climatic event

1986, 3, 5: "Schmiedl" instead of "Schmiel"

OK.

7 Conclusions

1990, 14-17: This sentence is confusing, avoid "provides... and provides", "provides the... signal... to... fluctuations" sounds strange.

The three points are fine, but, similarly to what I mentioned concerning the abstract, I wonder why you do not give some quantitative results here, e.g. concerning the precipitation peaks.

Conclusion paragraph have been corrected and now include quantitative estimates

References

I have not checked the references in detail, but since there are some references related errors in the text and the figures please check if there are more mistakes in the references!

References have been checked another time to avoid errors

1992, 23 "Combourieu Nebout" instead of "Combourieu-Nebout" (see above...)

1999, 11: "Schmiedl" instead of "Schmielldl"

OK. (see above for my name)

Tables

Table 1: 14C...

Table 2:

Is it on purpose that family names are written in italics?

"Q. ilex" instead of "Q. Ilex" (several times)

This have been corrected

Figures

The figures are generally well-organized, but there are so many mistakes...

All figures have been checked and corrected.

Fig. 2: Remove the points over the "i" in Cichorioideae and Asteroideae.

In my version, "Plantago" is bold instead of in italics, same with "Ephedra" and "Artemisia". Texts are overlapping in my version.

"YD" touches the line next to it.

Corrections have been done in the revised manuscript

Fig. 3: You mention "Pann, Twin, Tann, Tsum" in the text, in figure, I see PANN, MTCO, TANN, MTWA. What do you show, coldest month or winter?

"Corylus" and "Quercus" in italics, please.

Corrections have been done in the revised manuscript. We only show MTCO and MTWA, seasonal parameters are only provided for the precipitation.

Fig. 4: The color of the green and red text is not the same as that of the graph.

Are you sure the lines are of the same thickness?

Why are the numbers very close to the scales on the left, but far away on the right side?

The color of the green scale is now the same as that of the graph. Corrections have been done in the revised manuscript

Fig. 5: Change Age (kyrs) to Age (yr)! Remove the unnecessary "c"!

OK.

Fig. 6: Compare to comments to section 6.1: I can generally not follow some of the arrows you show in this figure. They look rather arbitrary. Why not use something like a running average?

We have removed the arrows as the trends are obvious. Nevertheless applying a polynomial test on our record provides the pattern underlined by arrows for MATW and MTCO with the same minima at around 7700 yr.

Fig. 7: On the right side, Age (yr) almost touches the numbers. It is "Schmiedl et al." (see above!). Replace both "Schmiedt" (figure) and "Schieldt" (caption)! Fletcher et al. 2013 (instead of 2012 in the figure)

This have been corrected.

Fig. 8: Consider using a real per-thousand symbol, not 0/00. Why upper case for " Precipitation", but lower case for "discharge"?

This have been corrected.

Final note

Just to state it again, I am convinced this has the potential to be a good publication. The new data fill a gap concerning marine palynomorph records. One additional thing I wondered: in Combourieu-Nebout et al. (1998), there were also dinocyst data – was the dinocyst dataset not improved the same way as pollen dataset? It would have been a helpful addition to this manuscript...

The dinocyst data were not completed so far, and it was not appropriated to show these low-resolution data in front of the new high-resolution pollen dataset.

Some sections only need minor edits, but some sections, particularly the abstract and the first half of the introduction, need a careful revision. A more detailed discussion of particularly the PBO and early Holocene would be fine. The figures and references should be carefully checked for additional mistakes.

This have been done.