Response to reviewer #1

1) References to major works are missing in the introduction. The authors mention a long list of works showing variations in lake level from France, Italy, Spain and even the Dead Sea but they did not cite recent works on Central Mediterranean such as the synthesis of Magny et al. (2013 CP). In addition, excepting one pollen record from southern Spain, only continental pollen records from the Middle Atlas are cited. The authors should have taken into account pollen-based vegetation and climate reconstructions from the nearby marine sites from the Alboran Sea (Combourieu Nebout et al., 2009 CP; Fletcher et al., 2008 QSR; Fletcher et al., 2013 The Holocene). In addition, the synthesis paper of Fletcher et al. (2011 Catena) on rapid climate changes in the western Mediterranean region and impact on landscape should have been considered.

Thank you for this comment. We have remodeled the introduction accordingly and included ad hoc citations.

2) Chronology:

a- Which CALIB software version was really used? CALIB 6.0 is indicated in the text (p.4012, l.16) and CALIB 7.0 in the table caption, with a different reference.

We have re-calibrated the ^{14}C dates using Calib 7.1 and the differences are very minor with the Calib version used previously (6.0). We have made the correction in the ms.

b- Information on the calibration curve used and the material dated (charcoal, shell, bulk sediment. . .?) should be indicated.

The dated material is bulk. It is now mentioned in Table 1.

c- Presentation of the calibration results in Table 1 does not follow recommendations given in the CALIB manual (in particular indication of 2 sigma cal age ranges and relative area under distribution).

Table 1 now follows the Calib recommendations and each column is properly titled.

d- In Table 1, what are SD1 and SD2

These were the standard deviations. We have corrected this in table 1.

e- and does “Calibrated age” stand for median probability?

It corresponds to the median probability provided by Calib. This has been corrected in Table 1

3) Methods:

a- References for all methods used should be given.

References are now provided in this version of the ms.

b- p. 4101:

- l. 16: “stable isotopes of delta 13C” must be replaced with “carbon stable isotopes” or “delta 13C isotopic ratio”

This sentence is now replaced by “delta ^{13}C isotopic ratio”.

- l. 18: “Pollen was” or “Pollen grains were” should be used

This has been corrected.

- l. 23: Justify why Cyperaceae pollen from this site is included in aquatic plants

Cyperaceae pollen percentages are included in the aquatic plants because they originate mainly from the genus *Cyperus* which often grows around the lakes.

- l. 26: replace “the fine grains (< 2 mm) of the sediment” with “the sediment fraction < 2 mm”

This has been corrected.

c- p. 4102:

- l. 3: Sentence meaning is unclear.

We agree. This sentence has been re-written.

- l. 14-15: the method used for delta 13C measurements should be described

We have provided a full description of the method.

- l. 15: “University of Bordeaux”

Done

- l. 21 & 26: “annual precipitation” instead of “annual amount of precipitation”

We agree

d- p. 4103 :

- l. 2: give information on the database (name, reference, area covered. . .).

The database of georefenced plant species has been set up by R. Cheddadi from the following two plant distribution atlases: Hulten and Fries (1986) and *Flora Europaea* (Jalas et al. 1972, 1973, 1976, 1979, 1980, 1983, 1986, 1989, 1991, 1994). These references are

now cited in the manuscript. The database is a local one and its coverage corresponds to the two cited atlases.

4) Pollen data:

*The description of the pollen diagram is very short and uncomplete. In contrast to the author's statement, other taxa than Pinus and Cedrus, such as Poaceae and Quercus, present strong variations which are not described or interpreted. The authors do not give a clear description of the pollen diagram and do not present a comprehensive history of the vegetation changes at the site. Only the replacement of pine by cedar at 3750 cal yr BP is pointed out while for instance, two important forest opening episodes are clearly recorded, the first one preceding the cedar expansion and the second one between 2500 and 2500 cal yr BP marked by a drastic cedar reduction.

Thank you for this remark. In order to make an efficient and useful presentation of our data, we now provide a detailed description in a new table. This makes the main features of the pollen diagram easily accessible to the reader without overwhelming the manuscript with unnecessary details.

*In the discussion, the vegetation changes recorded in the Hachlaf sequence are not compared to the other records from the Middle Atlas (only cited in the introduction) and the Alboran Sea.

Thank you for this comment. We have added a new section that compares the aridity trend after 6ka in both the Mediterranean area and within other records outside the study area as well. This, indeed, provides a wider view of our work.

5) Pollen-based climatic reconstructions:

* The reconstructed climate changes should be discussed in the context of the vegetation variations as recorded by the pollen diagram. Is the increase in precipitation seasonality in the second half of the record supported by the vegetation changes?

Thank you for this point. The precipitation seasonality increase coincides with substantial changes in the ecosystems around the studied site. The main observed changes are the increase of the taxonomic diversity along with an expansion of the cedar populations and the retreat of the pine forest. Mediterranean areas are known for their high species diversity and for their sclerophyllous tree species that may withstand pronounced annual rainfall seasonality. Since both the annual amount of precipitation and the winter temperature have not changed substantially, we believe that the observed vegetation changes in Hachlaf may be explained by increase of the winter-summer contrast in precipitation.

- Is Quercus taxon including evergreen or deciduous oak pollen, or both?

Yes it does because the distinction between the two types was rather random during the pollen analyses. We decided not to use dubious assignments of the pollen identifications.

- And which pine and oak species are taken into account in the database used for the climatic reconstructions?

For the climate reconstruction we have assigned *Pinus* pollen grains to *P. halepensis* and those of *Quercus* to *Q. coccifera*.

- Are cedar ecological requirements supporting higher rainfall seasonality?

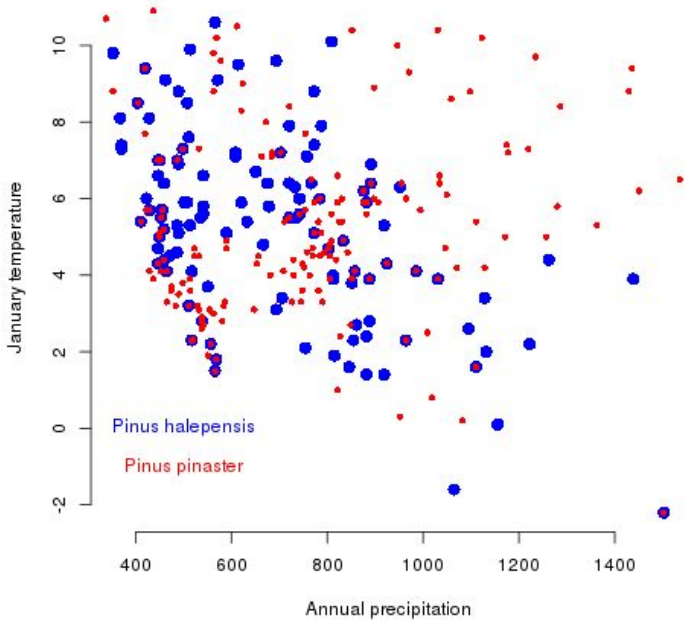
A study of drought thresholds influencing the growth and photosynthesis was performed on different cedar stands and species (*C. atlantica, C. libani, C. brevifolia & C. deodora*) of different origins (Aussenac & Finkelstein, 1983). This study shows that among many conifers, cedar trees may keep a sustained photosynthesis activity even when drought is very high. Thus, strong precipitation contrast between summer and winter may not affect the cedar's overall growth as long as the total amount of rainfall is sufficient (higher than 600 mm/year) and the winter temperature is low enough for the bud burst (Larcher, 2000).

Aussenac, G., & Finkelstein, D. (1983). Influence de la sécheresse sur la croissance et la photosynthèse du cèdre. Annales Des Sciences Forestières, 40(1), 67–77.

Larcher, W. (2000). Temperature stress and survival ability of Mediterranean sclerophyllous plants. Plant Biosystems, 134(3), 279–295.

- Which pine species are developing in the Middle Atlas and to which climatic characteristics are they associated with?

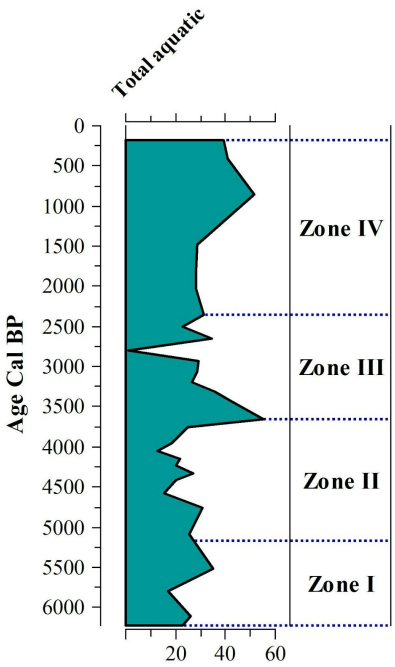
Currently two pine species are developing in the Middle Atlas; *Pinus pinaster* and *Pinus halepensis* up to 2600m asl. Although these two species are rarely associated (Benabid, 2000).



This plot, based on plant species distributions and their related climate variables archived in our database, shows the climatic niche (annual precipitation versus January temperature) of the two species. The median annual precipitation for P. halepensis and P. pinaster is 927 mm/yr and 991 mm/yr, respectively. The median January temperature for these two species is 4.2°C and 4.5°C, respectively. There is a clear overlap of their climatic niches. Therefore, assigning P. halepensis or P. pinaster to Pinus pollen grains will not affect the climatic reconstruction of neither their annual precipitation nor the January temperature.

- p. 4105, l.25: “water persisted in the lake during the summer season which allowed the presence of aquatic plants (which flower during late spring and summer)”: Could the authors relate this assertion to the pollen diagram?

The occurrence of aquatic plants (>20%)in the pollen record (figure below) requires a sustained presence of water. All the identified taxa in the fossil record (Potamogetonaceae, Cyperaceae, *Typha angustifolia,Typha latifolia*, Ranunculaceae, *Myriophyllum*, Juncaceae) flower during the summer season in a lake (or marshy) environment.



- p.4016: the authors state “After 3750 cal BP, Atlas cedar spread noticeably around the site while the pine forest strongly regressed. During this ecosystem transition we do not observe any major change in the reconstructed amount of annual rainfall or in winter temperature.” However, we can see in Fig.3 that Pann become noticeably lower and Tjan more regularly higher when cedar percentages increase.

Actually, the overall change that we observe in the amount of annual precipitation over the last 6000 years is about 50 mm and the amplitude of change in January temperature is about 2°C. These changes may not be considered as high. However, as the precipitation index shows it, if the 50 mm/yr represent an additional difference between summer and winter seasons then that may affect the ecosystems because it increases the seasonal contrast which is already quite pronounced (3 to 4 times more precipitation in winter than in summer, please see figure 4). Thus, both summer and winter amount of precipitation decrease (since Pann decreases) but the decrease affects more the summer than winter. Such pronounced winter-summer contrast may explain the regression of pines at the elevation of our site (1700m asl) and the expansion of Atlas cedars.

*The obtained climatic reconstructions are not discussed in comparison with previous pollen-based climatic reconstructions available in this region (Cheddadi et al., 2009; Combourieu Nebout et al., 2009). This would have been interesting to note that reconstructions based on the MAT from Cheddadi et al. (2009) are not consistent to those presented in this manuscript and try to explain this contrasting pattern. Is it related to the method or to an actual different climate pattern?

Thank for this remark and we will make such necessary comparison in the manuscript.

As a matter of fact, the climatic reconstruction presented in Cheddadi et al. (2009) are based on the pollen records from 3 sites. Two of them are from the Middle Atlas in Morocco (lake Ifrah and lake Tigalmamine) and the third site is from Algeria (Chataigneraie). The record

from lake Ifrah does not cover the last 5000 years which prevents from a direct comparison with our data. The climatic data from lake Tigalmamine have already been published by Cheddadi et al. (1998). These data (with an age model based on uncalibrated ^{14}C dates) which cover the Holocene do not show any major conflict with the data of this manuscript (based on calibrated ^{14}C dates). Indeed, they both show a rather decreasing trend of the annual amount of precipitation (figure 4A in Cheddadi et al., 1998) after 6ka. The main "discrepancy" concerns Tjan (figure 4C in Cheddadi et al., 1998). However, if we focus only on the period of interest (the last 6ka) the reconstructed amplitude of change from both pollen records (lakes Hachlaf and Tigalmamine) is about 2°C and in both cases the reconstructed absolute Tjan is between 3 and 5°C.

- The authors only state p. 4104: “Besides the low range, both Pann and Tjan show consistent trends. Pann decreases progressively since 6000 cal BP which is in line with the aridity trend that has been observed in other records from the Mediterranean borderlands (Risacher and Fritz, 1992; Brooks, 2006; Hastenrath, 1991; Anderson and Leng, 2004; Umbanhowar et al., 2006).” None of these works deals with records of the Mediterranean region but to Bolivia, tropics, Greenland and USA records.

Thank you for this remark and we agree with it. Indeed, there are records that show the aridity trend after 6ka outside the Mediterranean (Risacher and Fritz, 1992; Brooks, 2006; Hastenrath, 1991; Anderson and Leng, 2004; Umbanhowar et al., 2006) and other records in the Mediterranean as well (Pons and Reille, 1988 ; Julià et al., 1994a, b, 1996 ; Davis, 1994 ; Burjachs et al., 1997; Yll et al., 1997 ; Valino et al., 2002) and in Northern Africa (Ben Tiba and Reille, 1982; Ritchie, 1984; Ballouche et al., 1986; Lamb et al., 1989). We have cited all these studies now properly in the manuscript.

*The reconstructed increasing trend in precipitation seasonality is also opposite to conclusions of other papers such as Fletcher et al. (2008), dealing with Western Mediterranean pollen records. It is also counterintuitive regarding the orbital forcing which favors a decrease in seasonality along the Holocene. Can hypotheses be proposed for explaining that contrast?

If we may, the reference proposed above, is probably Fletcher and Sanchez-Goni (Quaternary Research, 2008) rather than Fletcher et al. (2008) as we didn't find any Fletcher et al. (2008). If this is correct then, Fletcher & Sanchez-Goni (2008) have not presented any climate reconstruction in their work and ultimately haven't stated any seasonality trend for the mid-Holocene. In their conclusions they state the following: "Changes in the seasonality of insolation (precession) also had an important pervasive influence on vegetation development over the last 48,000 yr, influencing both the amplitude and composition of forest development in this Mediterranean region."

While we do agree with this overall statement, we still are unable to compare our data for the last 6000 years with those of Fletcher & Sanchez-Goni (2008).

We must add that the relationship between precipitation trends and the orbital forcing is more complex than that of temperature. Reconstructed annual precipitation at 9ka, 6ka and 3ka show major spatial differences in Europe (Guiot et al. 1993) which makes their direct

relation with the insolation, probably difficult. While some areas recorded more than 400 mm/yr, others received 400 mm/yr less than today (please see figure 8 in Guiot et al. 1993). The temperature trends seem to provide more coherent patterns with the long-term insolation changes (Davis et al., 2003).

Having made these statements, we would like to add that concerning our data one should analyse the total amount of annual rainfall (Pann) and the seasonality index (SI) at the same time. Pann shows a decreasing trend (of about 50 mm/yr over the studied period). SI shows that the winter amount is 3 times higher at 6ka and 6 to 7 times higher today than the summer amount. These results tend to indicate that the overall amount of annual rainfall decreases which makes the summer season even drier today than at 6ka.

Davis, B. A. S., Brewer, S., Stevenson, A. C., Guiot, J., & Contributors, D. (2003). The temperature of Europe during the Holocene reconstructed from pollen data. Quaternary Science Reviews, 22, 1701–1716.

Guiot, J., Harrison, S. P., & Prentice, I. C. (1993). Reconstruction of Holocene precipitation patterns in Europe using pollen and lake-level data. Quaternary Research, 40, 139–149.

* Rapid climatic changes are presented in the introduction as a characteristic of the Holocene. Could the event at ~ 5 ka be discussed in this context?

Thank you for this remark. Although the thermal amplitude during the Holocene is lower than 4°C in the temperate latitudes (including the Mediterranean), there are indeed short and abrupt changes between 2 and 4°c which took places in a few decades (or even less). We will discuss these issues in the manuscript.

- Does it correspond to some regional changes?

As stated above, there are climatic events that have been recorded during the Holocene (Rohling and Pälike, 2005; Berger and Guilaine, 2008; Kaniewski et al., 2008). And they have causal relationship with some societal changes in the Mediterranean (Ben Tiba and Reille, 1982; Ritchie, 1984; Ballouche et al., 1986; Pons and Reille, 1988 ; Lamb et al., 1989; Julià et al., 1994a, b, 1996 ; Davis, 1994 ; Burjachs et al., 1997; Yll et al., 1997 ; Valino et al., 2002). We will include mention these regional changes in the manuscript.

- P. 4105, l.11-15: instead of guessing in a complex sentence that summer rainfall is probably decreasing because annual rainfall is decreasing and seasonality index increasing, why not showing the Psum curve in Fig.3?

- p. 4105, l.21-23: Same, why not showing the Pwin curve in Fig.3?

Thank you for this suggestion. We will add the summer and winter precipitation in figure 4.

6) Minor comments:

- The authors repeatedly refer to the “stability” of the Holocene climate (abstract, introduction and discussion). This is somehow clumsy because since more than a decade a large

number of works demonstrated that the Holocene climate is far from stable, at orbital and millennial/centennial timescales. And this is also something well known from marine and terrestrial sequences of the Mediterranean region.

We have stated that the Holocene climate is "stable" in the sense that the overall recorded temperature changes (in different proxies) is estimated between 2 and 4°C, including the little Ice Age, the Medieval Warm Period and other abrupt events such as the 8.2 ka. This "stability" is made in comparison to other time periods and mainly the post-glacial one. However, it is right that the 8.2, for instance, was abrupt and some authors made a remarkable similarity with the Younger Dryas. In our "stability" statement we meant a low amplitude of change. we will make this point more clear in the manuscript and we will mention that the Holocene climate recorded, indeed, some abrupt changes.

- p. 4099, l.6: Holocene climate fluctuations are not only related to “internal variability of the climate system”. Major forcing such as volcanic eruption and solar forcing are external to the climate system.

Thank you for this comment. Indeed there are other major forcings and we will mention them properly in the manuscript.

- p.4099, l.9: “known”

Done.

-p.4099, from l. 13 to l.17: what about the influence of the subtropical high and Westerlies on the seasonal climate of the western Mediterranean region?

The subtropical high and the westerlies are major components for the Mediterranean climate. We will discuss their impact on our reconstructed climate data.

- wrong numbering of the figures, ex. a call to Fig. 4 is missing, and it is should appear in the first paragraph of the “Results” section, i.e. before Fig. 3 first call.

Thank you. This will be corrected in the submitted version.

- p.4100, l. 10: delete “years”

Deleted

- p.4103, l.11: “dominating” – dominant

OK

- p.4015 (p.4105), l.13: sentence unclear, “difference” instead of “amplitude”?

Thank you, yes.

- p. 4106, l.18: delete “was”

"was" will not be deleted because the sentence was truncated; to complete it we must add this: “was important but part of the TOC originated from the terrestrial biomass.”

- p; 4107, l.3-17: First sentences repeated twice.

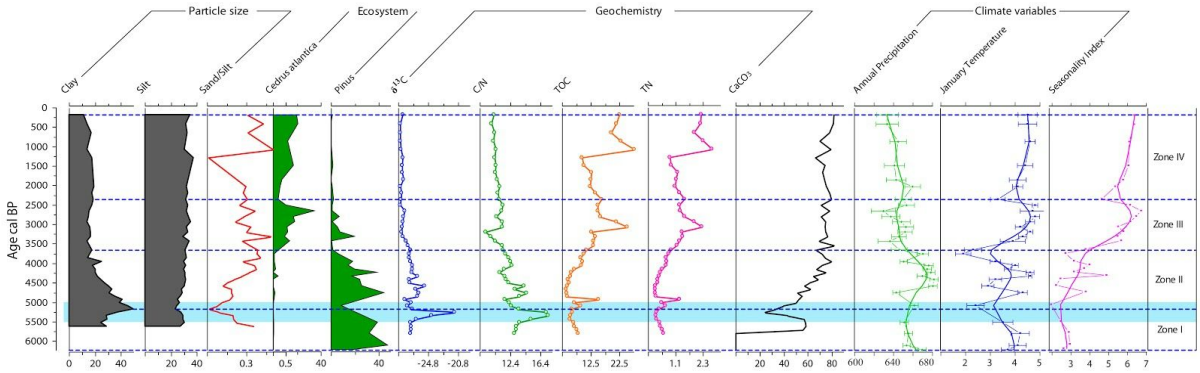
Indeed, the first sentence was repeated twice. We will delete it.

The last paragraph of the discussion has to be rewritten. It is repetitive and unclear.

The last paragraph has been rewritten

- p.4117: TN curve is not shown in Fig. 3 while it is described in the text and mentioned in the figure caption;

TOC and TN are highly correlated (0.99). The shape of the two curves is very similar, that is why we plotted only the TOC curve. We have added it to Fig. 3 (as below) and changed the legend accordingly.



what is the red line?

The red line will be deleted as it concerned a previous interpretation which no longer appears in the manuscript

- p.4105, l.4: “a SI”

“an” will be replaced by “a”

- p.4118: in Figure 4, a curve showing the taxonomic diversity and a hierarchical clustering tree are displayed but not mentioned in the caption. In addition, the method and software used for implementing the hierarchical clustering should be described in the main text or indicated in the figure caption.

Here’s a new caption for Fig. 4: “Diagram showing the percentages of the main pollen taxa identified in the Hach-I core. Cyperaceae and Juncaceae were grouped within aquatic taxa. The dashed black curves show an exaggeration (×7) of the percentages of some taxa. On the right, pollen zones with their boundaries are set up using a constrained hierarchical

clustering (R Development Core Team, 2013). The taxonomic diversity is computed using a rarefaction analysis."

What is the red line?

Here it's the same; the red line will be deleted as it concerned a previous interpretation which no longer appears in the manuscript

Is the taxa Quercus corresponds to deciduous or evergreen oak, or both?

Quercus pollen grains include both deciduous and evergreen oak as their assignment may often be dubious and/or not reproducible by another pollen analyst.

Response to reviewer #2:

-The English needs to be checked and improved by a native English speaker – there are too many mistakes and the grammar could be better.

== Thank you for this comment. The English will be checked and improved by a native English speaker

-There is a very interesting change observed in the pollen at ca. 3750 cal yr BP with the expansion of Cedrus but this vegetation change seems to start earlier on, around 4 ka, with a decrease in Pinus and decreasing estimations of temperature (Fig. 3). Could that also be related with the globally detected 4.2-4ka cold/arid event? This event has been previously identified in the western Mediterranean (i.e., Jiménez-Moreno & Anderson, 2012) so it would be worth adding some discussion about it.

== Actually, in many sites that we have studied in the Middle Atlas we observe an expansion of Atlas cedar populations after ca. 6 ka. The sustained expansion of Atlas cedar after ca. 4500 cal BP in this site is more probably related to its late spread at high altitudes. In other sites (lake Tigalmamine (Lamb et al., 1995), Ras El Ma (Nouelbait et al., 2014); Ait Ichou (Tabel et al., JQS in press)) located at about 100 to 200 meters below lake Hachlaf (ca. 1700m asl), Atlas cedar occur much earlier. The strong expansion of Atlas cedar around lake Hachlaf is very probably related to an upslope migration rather than to an abrupt climatic event.

-It seems reasonable to suggest the hypothesis about an increase in seasonality triggering the Cedrus expansion since 3700 cal yr BP in the area but it looks like the authors are not very sure about it. . .This could be tested checking the environmental factors controlling Cedrus occurrences at Present, which I am sure has previously been done by ecologists in the area.

== Thank you for this remark. Indeed, Atlas cedar is one of the most resistant conifers to summer drought which should explain its expansion at the expense of *Pinus*. Here is some eco-physiological information that we have provided to reviewer 1 (apologies for the copy/paste). "A study of drought thresholds influencing the growth and photosynthesis was performed on different cedar stands and species (*C. atlantica, C. libani, C. brevifolia & C. deodora*) of different origins (Aussenac & Finkelstein, 1983). This study shows that among many conifers, cedar trees may keep a sustained photosynthesis activity even when drought is very high. Thus, strong precipitation contrast between summer and winter may not affect the cedar's overall growth as long as the total amount of rainfall is sufficient (higher than 600 mm/year) and the winter temperature is low enough for the bud burst (Larcher, 2000)."

Aussenac, G., & Finkelstein, D. (1983). Influence de la sécheresse sur la croissance et la photosynthèse du cèdre. Annales Des Sciences Forestières, 40(1), 67–77.

Larcher, W. (2000). Temperature stress and survival ability of Mediterranean sclerophyllous plants. Plant Biosystems, 134(3), 279–295.

Response to reviewer #3

Major comments:

- A detailed review of the available literature must be done. This will strongly improve the present manuscript which doesn't present the most relevant or recent studies from the western Mediterranean, Morocco and Middle Atlas: Reille 1976; Barker et al 1994, Lamb et al 1989, 1991, 1994, 1995, 1999; Cheddadi et al. 1998, 2009; Combourieu- Nebout et al., 2009 ; Detriche et al 2008, 2009, 2013; Rhoujjati et al. 2010; 2012; Amami et al. 2013; Fletcher et al., 2008 and 2013; Zapata et al., 2013; Reddad et al, 2013; Giraudi et al. 2013, Wassenburg et al., 2014; El Bait et al. 2014; Muller et al. 2015; Lebreton et al. 2015). Also see the synthesis presented in Fletcher and Zielhofer, 2013. While the actual review covers the Mediterranean area up to the Anatolian plateau, it should mention synthesis from Fletcher et al., 2013 and Magny et al., 2013 (and Joannin et al., 2014 if the authors still wish to go eastward). This comment is to be addressed both in introduction and in the discussion where one would like to see what the processes behind the observed environmental changes are.

== Thank you for this very constructive comment. We agree that a due review of available studies and a comparison with available data will certainly improve the manuscript. However, we prefer not to extend our discussion to the Anatolian plateau and/or the western part of the Mediterranean. A review restricted to the north-western part of Africa and the western Mediterranean area will be developed in the discussion.

- The MS failed to rigorously discuss the main data trend. Whether the Seasonal index trend is valuable, readers do not see much evidence. For instance, modern values of SI are not related to references

== All Pann values plotted in figure 7 stem from the worldclim data-set (Hijmans et al., 2005, cited in the manuscript) and we have computed the SI values from these modern data. The plotted Pann and SI do not stem from published work.

Hijmans, R. J., Cameron, S. E., Parra, J. L., Jones, P. G., and Jarvis, A.: Very high resolution
interpolated climate surfaces for global land areas, Int. J. Climatol., 25, 1965–1978, 2005.

- fig 7 does not show % as mentioned at p9l3 nor winter precipitation as mentioned p9l8.

== We guess that p913 is p 4105 of the manuscript set up by "Climate of the Past" but we couldn't relate p918 to any page and/or line in the manuscript. Having said this, figure 7 does not show percentages but frequencies.

The whole discussion about SI is related to winter vs. summer precipitation and to the favoured evergreen oaks which are however not shown in the pollen diagram.

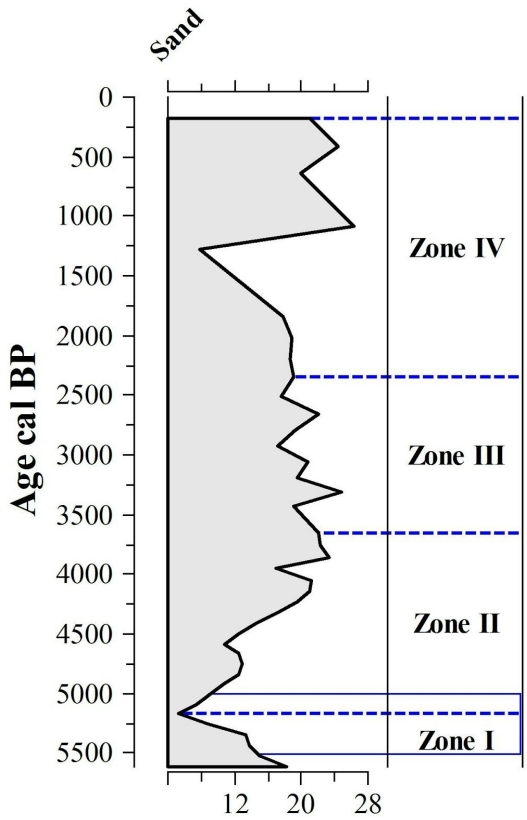
== Apologies for this misunderstanding. Actually, there is an oak curve plotted in the pollen diagram. However, in order to make our discussion about the evergreen oaks more clear, we will specify clearly "Quercus-evergreen" in the pollen diagram.

The first part of the trend is based on a higher water level with an enhanced development of algae recorded in C/N and d13C values. The pollen diagram does not evidence this trend, and on contrary shows higher sub-aquatic plants (Cyperaceae) at first than after where Potamogetonaceae and Typha dominate. Can you please explain these opposite results? This part needs to be thoroughly revisited and discussed at the light of presented data together with other evidences available in the literature.

== Thank you for this remark. Actually, we haven't related any lake level change with the proportion of aquatic plants simply because plants are not directly indicative of lake level changes but rather of availability of water and very often during the summer season because many aquatic plants tend to flower and release their pollen between June and September. We have stated that "water persisted in the lake during the summer season" but one cannot infer any information about the lake level changes from such statement. d13C and C/N ratio provided us with information concerning the origin of the organic matter (in situ production versus input from the catchment area) but not on the lake level changes. Thus, higher d13C and/or C/N ratio with low presence of aquatic plants are not inconsistent in cases where there is a low terrestrial input (low C/N) during a period when aquatic plants may develop around the lake (high pollen production).

- Discussion of the 5.5kyr event: Both Abstract, Introduction and discussion highlight a 5.5kyr event which is insufficiently discussed with no references at all. For example, you should rigorously present all data and highlight that this corresponds to the sand level deposit, which in turn certainly affects the pollen diagram. Clays develop at that time, but do you have any idea of which clay type it is so that you can maybe relate this event with stronger saharian winds (summer?) (Bout-Remazaille et al 2007). This would be worth a try but to read other NA papers showing this event and relate/match your record to their interpretation in terms of climate process. For example, in the Jefara, the second Saharan humid period is divided into two parts by a dry spell that occurred around 5.5–5.4 ka BP (Giraudi et al., 2013).

== thank you for this interesting comment. Indeed, there are references that should be brought up in the discussion and we will develop this topic in the discussion. Concerning the proportion of sandy fraction in the sediment (see figure below), it is rather very low between 5 and 5.5 ka. In effect, the ratio sand/silt in figure 3 shows rather a strong depletion of the sandy fraction. We haven't provided the absolute sand proportion in figure 3 but rather the "sand/silt" ratio because it may provide some information about the erosion strength. Here below are the proportions of the sandy fraction.
Concerning the clay composition, we, unfortunately, analysed only the size fraction of the bulk sediment but not the composition and therefore we cannot locate the origin of clay particles.



Age-model:

- No information on what is dated;

== The material dated is bulk for this coring. We have reported this information in table 1.

- you should use the 2sigma error calibrated with intercal13 and an appropriate program to plot and calculate the ages of samples.

== Thank you for this remark. The 14C ages provided in table 1 were already calibrated using 2sigma error and the calibration data-set used was intercal13. Concerning the age/depth model we have recomputed it using BACON (Blaauw & Christen, 2011). Figure 2 in the manuscript will be replaced accordingly.

Then is artificially added an age at 0cm which is not correct. In the end, data stop at 250 years cal BP without any explanation! Does it suggest a possible water reservoir effect? Can you please detail?

As stated in the section "Materials and methods", the coring length is 250cm that is why the age/depth model covers 250 cm.
We have unconstrained the age of the core top and extrapolated the age using BACON software. Indeed there is a difference of a few hundred years. We compared the obtained age/depth model using Bacon with our age/depth model and we found very minor differences between the two because the four 14C ages show a linear distribution. However, all data will be re-plotted using the new age/depth model.

Methods:

- methods are listed but the reader does not know which process the authors expect them to highlight. For example, C/N threshold is not argued so that the reader has no clue whether it is a good interpretation. It is mentioned a depth resolution of 5 cm for all proxies so that the study should be based on 250cm/5cm=50 samples. This is clearly not the case: 46 samples for d13C, TOC. . . 30 samples for pollen and climate variables. Can you please detail?

== Thank for this remark. Indeed, we have stated that the coring was sub-sampled every 5 cm (p. 4101, l. 15) which is wrong. We will provide in the manuscript the exact number of analysed samples for each proxy.

Results:

- the results coming from the pollen data are not insufficiently presented.

== Thank you for this remark. In order to make an efficient and useful presentation of our data, we now provide a detailed description in a new table. This makes the main features of the pollen diagram easily accessible to the reader without overwhelming the manuscript with unnecessary details.

- We do not know the lower limit of pollen sum counted by samples.

== We have added the pollen sum for each sample in figure 4 (pollen diagram). Just for your information, the minimum and maximum pollen counts are 202 and 1323 pollen grains.

- Results on vegetation are not presented in the text and the figures are not explained. For what use is the pollen zonation?

== We set up pollen zones (using a cluster analysis) to describe periods of time during which the vegetation shows minor changes and to depict more important changes from one period of time to another (or from one zone to another). Indeed, these pollen zones have not been used in the discussion because our aim is to carry a discussion on the overall trends rather than comparing time periods or vegetation zones over the last 6000 years.

- What means the red line in figure 4?

== Apologies for this unnecessary line in figure 3 which has been removed.

- TN is not shown but presented in results. Should be shown or not presented in the text.

== TOC and TN are highly correlated (r2=0.99). However, it will be included in figure 3 of the submitted manuscript.

- Same for sand.

== We have added the sand proportions in figure 3.

Discussion:

- p8 l2-10. Why this paragraph on human and climate relationship when the present study doesn't deal with this? To be deleted.

== Thank for this remark. In fact, our intention is not to deal with the human impact but to state that human populations have settled and expanded around the Mediterranean very probably because climate allowed them to do so. Areas with extreme or unstable climate tend to be less populated and/or with discontinuous human occupation. We prefer to keep this 2 or 3 sentences.

- P6 l1. What is your conclusion about the SI?

== Our conclusions are that (1) it increases steadily after 5 ka, (2) it becomes 2 to 3 times higher today than at 6ka and (3) it corresponds to the observed contrasted Mediterranean climate.

- P11 l15-17. I don't understand on what is based this statement. Please argue. TOC is only mentioned in the discussion but not discussed. Why is it used for?

== Apologies, we couldn't relate this comment to any page of the manuscript. Please could you refer to the pages set up in the PDF generated by CP. Thank you.

Minor comments

- The abstract has to be reshaped and carefully corrected to make it easily readable. The sentence "Holocene climate is known to be rather stable. . ." in spite of not being new is not a good way to introduce the time window of your study. Why the authors focus on the last 6000 yrs? Is it of particular interest?

Thank for this remark. The abstract will be re-written indeed.

1. Introduction: p4 l13-19: this paragraph is of any use? Repetition of the abstract! What is studied, a lake?

== Apologies here again, we couldn't relate this comment to any page of the manuscript. Please could you refer to the pages set up in the PDF generated by CP. Thank you.
In section 2 "Study area" pages 4100 and 4101 we state that "... the studied site, Dayet (lake) Hachlaf. This small water body is located …".

2. Study area:

- where does the climate data come from?

In the study area (we state that "Available meteorological data (1980–2008) at Ifrane station …". The climate data are from Ifrane meteorological station.

- During which season or month the lake level is the higher?

Presently, it is during the late spring.

- Is there any river inflows?

No, there is no river inflow.

- The figure 1 does not show well the lake, nor the scale and nor the coring site.

The scale and the coring site have been added to figure 1 and will be submitted with the manuscript.

- Oaks are differentiated in the text not in figure 1c.
Figure 1c does not provide any detail concerning the plant species but rather the dominating taxa (mainly *Cedrus* and *Quercus*) in the main ecosystem types (steppe, matorral ..). Moreover, we do not use oak species in our interpretations. We now mention "Oak forest" instead of "Oak" in figure 1c.

3. Materials and Methods: a presentation of the coring itself is necessary. Are they twin cores?

A detailed lithological description is provided in figure 2a. No, we haven't made a twin coring.