

Interactive comment on “Late Holocene vegetation changes in relation with climate fluctuations and human activities in Languedoc (Southern France)” by J. Azuara et al.

Anonymous Referee #2

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Reviewer comments to manuscript "Late Holocene vegetation changes in relation with climate fluctuations and human activities in Languedoc (Southern France)" by J. Azuara and colleagues, submitted to Climate of the Past Discussion.

The manuscript presents a new pollen record from a coastal lagoon located in southern France (Palavas) spanning the past 4500 years. Sediments from the same core had already been studied to reconstruct the frequency of storm events and the link between variations of past storm frequency and mechanisms causing climate changes. The lagoon is located in a region that has been settled several millennia ago, and has archeological and environmental-archaeology records that are sufficiently vast to get an, at least, qualitative picture of settlement history and past land-use changes. The

C1948

authors use pollen percentages, pollen-percentage ratios of selected taxa, and storm frequency as inferred from sedimentology to investigate the links between storminess in Palavas and changes in vegetation composition in the surroundings of the lagoon. They also compare the records from the sediment core to glacier advances in central Italy and to ice-rafted debris records from the N-Atlantic (Bond events). A substantial part of the manuscript deals with vegetation history in relation to land-use changes during the Antiquity and Historical times.

The data presented in this manuscript seem interesting but the manuscript itself could be substantially improved. At some points it is sloppy under several aspects (interpretation, structure, figure quality, captions). I therefore invite the authors to revise the manuscript profoundly and invite them to consider the few suggestions here below:

General comments:

1. Writing style should be revised. I found it particularly disturbing to read at times that "Fagus decreased", or "increases in Quercus. . .". The authors should take care of making a clear distinction between the evidence (pollen percentages) and the interpretation (population-size changes, vegetation composition).

2. The hypotheses that are going to be addressed should be clearly declared in the Introduction. The aim of the study, as declared in the Intro, is to "decipher[ing] climatic and human causes of environmental changes. . .". However, when I arrived at the end of the manuscript I could neither figure out if this was actually achieved nor could I understand how it had been done. The authors first discuss (5.1) the effects of the 'long-term aridity trend' on vegetation, which caused the disappearance of Fagus and Abies stands in the warmer lowlands near the lagoon. However, surprisingly they state that variations of Fagus pollen % after 1084 cal BP (AD 866) are independent of climate changes (" . . .[they]. . . cannot be interpreted in term of climate fluctuations because of the strong human influence"). Later (6.1) they discuss the effects of the long-term aridity in relation to evergreen-oak-forest development: they arrive at the conclusion that

C1949

the evergreen oak were favoured by anthropogenic activities already starting from at 4000 cal BP. The reader is therefore confronted with an interpretation envisaging that some trees (*Abies* and *Fagus*) migrated northwards because climate became drier, while other species (*Quercus ilex*) established and expanded because of human activities. I wonder two things here: first, how was the date 1084 cal BP found? There is a dating precision in this age estimate that suggests it has not been determined based on the pollen record; second, how can the authors reject a theoretically possible alternative hypothesis, which envisages that changes in vegetation composition were favoured by the combined effects of both climatic changes and anthropogenic activities? Regardless, I found that the discussion could be improved, particularly concerning the palaeoecological records of evergreen *Quercus*. There's some recent literature that could be useful. One paper to start with: Henne et al. (2015).

Also, concerning the expansion of evergreen oaks, the authors conclude that (last sentence of Conclusion) that "evergreen *Quercus* expansion is related to coppicing and the increase in fire frequency". However, the authors have no data for fire frequency at Palavas and base their conclusion on studies focusing on fire disturbances in eastern Spain (Pinol 1998) and in Europe (Schelhaas et al 2003). The Schelhaas reference does, however, not support the authors' statement because Schelhaas et al. (2003, page 1629) wrote that "Figure 3 shows a fast increase in the number of forest fires over time. This apparent trend is most likely also influenced by the aforementioned increase in forest fire detection and alertness. [...]. However, it is impossible to separate this trend into a real increase and an apparent increase due to better detection methods." Hence the conclusion that "evergreen *Quercus* expansion is related to fire frequency" seems unsupported by data.

In this paper there are also other hypotheses that have been addressed: (i) the potential effect of climatic changes that occurred in correspondence with Bond events on vegetation in Palavas (and thus on the S-French coast); (ii) the vegetation history of the surroundings of Palavas in relation to land-use changes during the Antiquity and

C1950

Historical times. The latter part reads differently than the former one because the text follows a different narrative style, it is more focussed on describing historical facts and their link with vegetation history. I found this part interesting but could not figure out what research question the authors wanted to address. Again, the Introduction failed to introduce the goals and aims of the paper.

In summary, the authors should declare in the Intro the aims of the study, thus also including hypothesis (i) and the more descriptive part (ii). What were the expectations (hypotheses) of the authors? Which hypotheses were rejected? Also, it would be useful to summarize the land-use changes that are discussed in the text in a table, giving age limits for each period.

3. The link between changes in vegetation composition and storminess is unclear. The authors should better declare their hypothesis in the Introduction. Which effects do they expect from storminess on vegetation composition? In section 5.2. one can see that the storminess record was compared to the *Fagus/Quercus* ratio. The authors explain that *Fagus* is at the southern distribution limit in this region and that it therefore may be sensitive to variations in moisture availability (besides, the authors could support this basic assumption with references to studies showing this). Hence decreases of the F/Q ratio are interpreted as indicating "arid events" related to *Fagus* pollen decreases. What is unclear is: why did they use the F/Q ratio rather than just the *Fagus* % record? Also, in which season of the year do storms occur at present? Does the season of occurrence of storms coincide with the drier season for *Fagus* (a deciduous tree)? Further, what is most striking is that the duration of low F/Q-ratio values is rather short compared to the duration of low storminess. One may thus wonder why *Fagus* population would recover after few centuries although the climatic conditions remain dry?

4. The *Fagus* pollen decreases are interpreted as "northward migration of *Fagus* at higher altitudes". I'm rather surprised and puzzled by this interpretation and invite the authors to think better at what "migration of plants" means. For example, this

C1951

interpretation implies high dispersal rates and dispersal distances, and low ages to reach reproductive maturity for *Fagus* trees. Have the authors done such calculations? Are the figures obtained realistic? There are possibly other, more realistic alternatives to this interpretation, and I would invite the authors to explore other interpretations.

2. The authors discuss the changes in storminess and vegetation composition in terms of "NAO-like" climatic patterns. However, to do this consistently they might also discuss, in my humble opinion, the following aspects in order to give the reader a complete picture: the NAO index describes weather patterns in winter (Nov-Mar), and thus a link between *Fagus* and NAO would imply that *Fagus* needs precipitations in winter. Do the authors have any evidence to support this hypothesis? Wouldn't *Fagus* need moisture during the dry summer months (say July-August)? What is the link between NAO and July-August precipitation?

3. The link between Bond events, ^{14}C production, storminess, and changes in vegetation composition is explored in Chapter 5.2. Concerning this chapter it is a pity that the reader hardly gets to know that the comparison between Bond events, ^{14}C production, and storminess had already been discussed and published in a previous paper (Sabatier et al. 2012). This should have been mentioned in the Introduction. Also, the last paragraph of this chapter (dealing with wavelet analyses and solar forcing) seems out of place, or has not been sufficiently introduced.

4. The authors conclude that the variations of the "deciduous *Quercus*/evergreen *Quercus*" pollen % ratio might be the evidence of anthropogenic activities", which therefore caused evergreen oaks to replace deciduous oaks. Here again, as in the case of the F/Q ratio, I wonder if the Q/Q-ratio has been validated and/or calibrated to show that it can be used to infer 'aridification' or 'anthropogenic activities'. Following questions arise: is the ratio not influenced by variations of other pollen taxa as well? Is the ratio representative of which anthropogenic activities? If so, it would make the interpretation stronger. If not, I would suggest to omit the Q/Q-ratio curve. But regardless, it would be useful to plot the simple pollen percentages of *Fagus*, deciduous *Quercus*, evergreen

C1952

Quercus in Figure 3.

Methods are not sufficiently explained:

- There are several issues with the chronology (see detailed comments for Page 4128);
- The use of ratios of pollen percentages is not justified;
- which pollen keys were used to identify pollen grains?
- On what data is the vegetation map based? Please cite the source or explain the method used to draw the Figure 1c (see further comments below)

Figures are sloppy (see detailed comments for Figures below). A Table with ^{14}C datings and other control points (storm events) used to establish the age-depth model should be added.

Detailed comments:

Abstract:

- 2nd sentence: ...separating the effects is not needed to "reconstruct paleoenvironments". Please rephrase sentence
- L10: why are arid events also periods of "climatic instability"? Did all other periods have a stable climate?
- L13-14: few lines above you said that there was a "long-term aridification". Now you say that spread of evergreen taxa and loss of forest cover result from anthropogenic impact. The two statements are not consistent with each other because a shift to drier climate conditions might also have caused loss of forest cover and a spread of evergreen angiosperms. Please note here (and elsewhere in the text) that the term "evergreen taxa" also includes many conifers, such as *Abies*...which in your interpretation decreased due to aridification. Hence, please change the term 'evergreen taxa' unless you want to include most conifers.

C1953

Page4126

- L4-5: Why is this relevant? If Capestang had a well-dated pollen record, why is it necessary to be mentioned here? Is it because you plan to compare your results with the Capestang record? Or are there other reasons?

- L6: I'm not really sure that a sampling resolution of 2-10 cm can be truly termed "high resolution". But I know that many use the term in a generous way.

- L22: in the previous paragraph you mentioned the "lagoons". . . in this sentence there's only one lagoon. . . hence the question: which one?

Page 4127

- Lines 1-19: the description of the distribution of the dominant taxa is very useful. However, the source of the information given in this paragraph and in Figure 1b is unknown to the reader. Is it a result of the current study? If so please explain in the Method section what has been done. If not, please cite the source. Also, Ericaceae are also abundant in some environments. Given that Ericaceae pollen % show an interesting pattern, could the authors also introduce the distributions of species belonging to that family in this paragraph and in Figure 1b? Further, for Figure 1b following questions seem relevant to me: - what do the circles indicate? Vegetation surveys in a plot? A tree? A minimum abundance/density of the species? - sometimes the circles are so well connected to each other that a line appears. What do the lines indicate? Surveys along a track?

-L13-14: . . . distinguished in routine pollen analysis, which limits the interpretation of Pinus pollen variations in terms of. . . ?

Page 4128

L5: here you mention "the last 5 millennia". . . but at the end of the paragraph you mention "the last 7000 years". Please correct one of the them.

C1954

L13: eight, not height in PB06

L22: Cerastoderma glaucum

L23 and following: it is unclear to what extent the present chronology of PB06 coincides with the chronology of the same core as published in previous papers. The authors refer to Sabatier & Dezileau (2010) and Sabatier et al (2012). I looked at those two papers and noted some inconsistencies that would be worth to address in the current manuscript: The authors mention that 14C ages were calibrated with Calib 5.2 as in Sabatier & Dezileau (2010). However, in Sabatier & Dezileau (2010) only the top 300 cm of the core where published, the chronology was shorter than in the present manuscript, and Calib 5.0.2 was used, not Calib 5.2. On the other hand, in Sabatier et al. (2012) one can read that the chronology was obtained using an age-depth model software (Clam) but nothing is said concerning the calibration software or calibration dataset. Please clarify which software, which calibration dataset, which reservoir age, and which storm events were used to build the age-depth model. Please add that as a table as supplementary material because otherwise it will be very difficult to understand in the future what has been done in this study.

Page 4129:

L 7: "average time resolution is around 50 years". 50 years is not a resolution, 50 years tells the duration in time of something (as number of years). Resolution can be defined as years/sample (i.e. years between samples), or as "deposition time" (i.e. years within sample). Please clarify.

L9-10: could you better explain what is meant with "insufficient" pollen concentration? Insufficient is a rather vague term.

L14: to dissolve Lycopodium spores one generally uses HCl first, then HF later. Please bring correct order.

L15: please cite Stockmarr (1971).

C1955

L17: "minimum sum of 300 grains excluding Pteridophyta...and dominant taxa". Which dominant taxa were excluded? And why?

L19: "Proportions were calculated using the total sum of identified pollen grains". Where also the obligate aquatics such as for example Sparganium/Typha included? Also Pteridophytes? If so, please clarify why the pollen sum was not limited to terrestrial plants.

L21: please explain the rationale for using ratios of pollen percentages. Has such a ratio been used previously by other scientists? For the same pollen taxa? What are the modern evidences that validate the use of such ratios as climate proxies?

L24: delete "with distorted pollen proportions".

Page 4130:

L1-12: this paragraph is rather difficult to understand and raises a number of questions:
- the authors say that "pollen analyses from the overwash layers and the samples from immediately underlying sediments with conc. < 5000 grains/g have been discarded from the record to avoid taphonomic perturbations". However, it is unclear if the discarded pollen samples (not pollen analyses!) were deleted before plotting Figures 2 and 3 or if they were discarded only in Figure 3...or if they are plotted in both Figures (probably not, I assume);

- Also, I wonder why storm layers were actually sampled for pollen analysis in the first place: the pollen grains in those layers are almost certainly coming from reworked material;

- Further, it would be important to know how thick (in cm) these storm layers are in the cores. In theory, because the storm layers were deposited in few hours, their thickness should be deleted from the core to obtain a new "corrected depth scale" without the storm layers. The final age-depth model should be based on the new corrected depth scale, not on the original depth scale.

C1956

- L15: Eleven pollen zones in Figure 2. Please correct.

- L15: "...pollen zones based on pollen assemblages describe..." Please change to "pollen assemblage zones were visually determined."

Page 4131:

L3: "Abies disappears". Please find another term for 'disappears'. Also, it is the Abies pollen, not the Abies that disappears. Here and in the following sentences the authors should distinguish the subject of the sentences: the pollen (the evidence), the plants and vegetation (the interpretation).

L5: tree pollen abundance decreases (tree pollen cannot decrease)

L9: What is meant with "Forest taxa"? In figure 3 I see a curve showing Arboreal taxa (pollen %), not Forest taxa? Are Olea, Castanea, Juglans thought to occur as trees in the forests?

L12: Cerealia-type pollen reach...

L14: why 'relatively low'? Relatively compared to which other records? Please clarify.

L16: "while cultivated trees decrease"...please rephrase

Page 4132

L4: Abies and Fagus pollen...

L5: late occurrence compared to which other records? Please clarify

L12: expansion towards the south...or simply to lower elevation?

L19: where might have the shady slopes and valleys have been? From the map in Figure 1a one can see that the topography is rather flat (0-200 m asl) for more than 10 km around the coring site. Please clarify.

L20: but in Mat & Methods we were told that Pinus pollen could not be determined at

C1957

species level. Hence, the hypothesis that the decrease of *Pinus* pollen here occurs because *Pinus sylvestris* shifted northwards is a statement that is skating on thin ice.

L20: also, a shift northwards implies that the species was not present in the north. Have you any evidence for this?

Page 4133

L16: see previous comment concerning the interpretation of a "northward shift".

L22: Pollen % decreases of *Fagus* (e.g. the short-term decrease 4600-4300 cal BP) may be linked to "repetitive northward migrations". This interpretation implies that (1) *Fagus* trees were absent in the hills/mountains north of the Etang before 4600 cal BP, and (2) that they migrated to the north at 4600, stayed there between 4600 and 4300, and migrated back to the south at 4300 cal BP? However, there is no evidence to support hypothesis (1); moreover, hypothesis (2) seems rather striking (and unrealistic) and would imply extremely high dispersal distances and small ages to reach maturity for such trees. Please consider other, potentially more realistic, alternatives.

Page 4134

L1: "Such arid events". . . what is meant with "such events"?

L5-6: "Discrepancies in chronologies are probably due to model uncertainties". Why should arid events in Albania/Montenegro and in southwestern Spain occur simultaneously? Is there any evidence to support this hypothesis? Is this hypothesis actually relevant in the context of this paper?

L1-17: please move this paragraph further below and describe first the relationships between the proxies of your sediment core.

Page 4135

L7-8: "two Bond events might be divided in two phases. . .". I think the authors want to say that the effect of climate changes during Bond events changed over time, with an

C1958

initial phase at the onset of the event leading to more humid climate in Palavas, and a second rather final phase leading to drier climate with less frequent storms.

L27-28.: I cannot truly understand the meaning of this last sentence. Please clarify (1) what evidences, and (2) how & why a better understanding of mechanisms involved in these [which?] climatic oscillations have been brought by your study.

Page 4136

L6-9: The first sentences of this paragraph are not consistent with what had been written at the beginning of Chapter 5.1. In the latter the interpretation given, albeit without much discussion, was that vegetation changed due to the long-term aridification. Here instead the authors set out that the interpretation given previously in Chapter 5.1 is actually debated. It is rather striking to read that now. Please restructure text. And please update the references including also more recent studies.

L13: "increases in evergreen *Quercus* pollen and arid events do not correlate". Which increases in evergreen *Quercus* pollen? No increases were described in the previous sentence. And I can't see a correlation anywhere? What is the correlation coefficient? Please clarify and show the correlation statistics.

L14: delete "classic" from 'classic picture'. . . simply because it is not classic. There are different hypotheses (as described in the first few lines of section 6.1), that's all.

L19: "arid climate, which is usually expressed by the replacement of deciduous *Quercus* by evergreen *Quercus*". Why usually? This is surely not valid for Australia, just to give one example. Also, the argument given by the authors here is not consistent with what had been written on lines 1-5 and with what comes later (lines 24-25 and following); (1: "arid climate is usually expressed by the replacement of deciduous oaks by evergreen oaks"; 2: the deciduous/evergreen oak ratio decreases (thus evergreen oak increases); 3: conclusion: the deciduous/evergreen oak ratio is evidence for anthropogenic activity). please rephrase.

C1959

L20-23: "therefore climate variability alone cannot explain...". Sorry, climate variability from which proxy records? Please specify the subject and objects of the sentences.

Page 4142 This whole paragraph is interesting but needs a more balanced discussion. See for example other palaeoecological and vegetation-modelling studies such as Henne et al. (2015).

Figure 1a: add names of rivers.

Figure 1b:

- what do the circles indicate? Vegetation surveys in a plot? A tree? A minimum abundance/density of the species?
- sometimes the circles are so well connected to each other that a line appears. What do the lines indicate? Surveys along a track?

Figure 2:

- Why is there a gap in the pollen diagram between 200 and 600 cal BP (i.e. AD1750 to AD1350)?
- Y-axis label: Age yr BP...cal BP or not cal BP? Please correct
- Archeo periods: why are there no horizontal lines marking the limits between the cultural periods? Does the 'Modern period' go back to 800 cal BP (i.e. about AD 1200)?
- What is meant with "Antiquity"? I can't find it in an english dictionary (e.g. <http://dictionary.cambridge.org/dictionary/english/antiquity>). Or do you mean "Classical Antiquity"?
- x-axis label: "Deciduous trees"...all deciduous trees? Only some deciduous trees? Please clarify
- Other Riparian trees...not Riparian. Besides, what does this category include?

C1960

Please clarify all categories in the Mat & Methods

- What is "Sclerophyllous Quercus"? They are never mentioned in the text. Do you mean "Evergreen Quercus"? Please homogenize terminology.
- Pollen concentration: correct dimension. should be grains per gram

Figure 3:

- what do the green and red histograms indicate?
- what do the different vertical shadings indicate?

References cited Stockmarr, J. 1971. Tablets with spores used in absolute Pollen Analysis. *Pollen et Spores* 13:615–621. Henne, P. D., C. Elkin, J. Franke, D. Colombaroli, C. Calò, T. La Mantia, S. Pasta, M. Conedera, O. Dermody, and W. Tinner. 2015. Reviving extinct Mediterranean forest communities may improve ecosystem potential in a warmer future. *Frontiers in Ecology and the Environment* 13:356–362.

Interactive comment on *Clim. Past Discuss.*, 11, 4123, 2015.

C1961