***Interactive comment on* "Climatic subdivision of Heinrich Stadial 1 based on centennial-scale paleoenvironmental changes observed in the western Mediterranean area" *by* Jon Camuera et al.**

Anonymous Referee #3

Received and published: 6 February 2020

In this manuscript, Dr Jon Camuera and co-authors propose to divide the Heinrich Stadial 1 (HS1), one of the coldest and driest phases of the last glacial, on the basis of the changes in moisture availability and temperature inferred from the high-resolution pollen sequence of the Padul wetland (southern Spain). The authors also use sedimentological data to support their argumentation. The chronological framework is based on a number of radiocarbon dates from bulk sediment and classical age-depth modelling. The chronology of their record led the authors to suggest the HS1 in the western Mediterranean region to have an offset of ca. 1 ka with respect to the Greenland ice cores and most (if not all) of the European well-dated sedimentary records currently available. The research questions addressed are timely and of great relevance for the palaeoclimatic community, the methods used are mostly appropriate, and I acknowledge the effort the authors have made to discuss their results in the context of previously published regional palaeoclimatic evidence. However, I have noticed several important issues that the authors should address to make their arguments more convincing. At the moment, after reading carefully the manuscript, I am afraid that the main conclusions of the research are not fully supported by the data presented. In the following paragraphs, I will elaborate on why I got this impression.

GENERAL COMMENTS

1. A high number of radiocarbon dates does not necessarily mean that the chronology is robust. In this case, as shown in Figure 2 and Table S2, the number of radiocarbon dates available is truly impressive but all the accepted ones are on organic bulk sediment and bulk carbonate. The risk of hard-water effect is notable when dating bulk sediment from lakes whose catchments are mostly on calcareous bedrock. This holds particularly true for sections of the sedimentary sequence with higher shares of inorganic matter. The bedrock in the Padul-Niguelas basin is mostly calcareous (see Ramos-Roman et al., 2018), which is relevant also for this work and should be indicated in the main text or at least in the SI. This would contribute to explain the offset of ca. 1000 years observed in the Padul record at the beginning and the end of the HS1 compared with most of the well-dated records from Greenland and elsewhere in Europe. To have a sound support for their conclusions in this regard, the authors should provide a chronology based on short-lived terrestrial plant macrofossils or try to find out what the reservoir age for this period is. In any case, I consider that it is important that the authors discuss the limitations of their chronology.

We thank the reviewer for these constructive comments and suggestions.

In this new version of the manuscript we have clarified the possible issues concerning the influence of the dissolved carbonate from bedrock in the radiocarbon dating that could have affected the chronology. In addition, in the previous version of the manuscript there was a mistake in the dated "Material" column from Table 1 (Table 2 in the previous version of the manuscript) that has been corrected. The "bulk carbonate" and "bulk peat" material from the six new radiocarbon samples were not bulk sediments. These six

samples were taken as bulk sediment material from the core, but there were pretreated with HCl and HF in the Stratigraphy and Paleontology department from the University of Granada before submission to the BETA analytic laboratory for ^{14}C measuring. Therefore, the reservoir effect from these organic plant residues from both peat and carbonate lithologies does not take into account inorganic carbonate and the reservoir effect should be low(er). Nevertheless, we also understand that our age-depth model can present inaccuracies and the high number of dates does not assure us a perfect chronological control for the age delimitation of HS1. Therefore, in the new version of the manuscript, we have included that even if the age-depth model can present uncertainties, the early environmental record of HS1 in our latitude cannot be ruled out, and several factors and causes have been provided for supporting our hypothesis of an early record of HS1 in the study area.

In addition, a new Bayesian age-depth model has been carried out for the last 30 kyr BP, including the two previously rejected dates (from the six new samples analyzed for the period concerning the HS1) in order to reduce error uncertainties and improve the chronology. As explained above, these six new samples were pretreated and the dated material was organic plant residue, and therefore, there are no objective reasons to exclude any of these samples (new Figure 2, *Chronology* section from the Supplementary Information and Tables S1 and S2).

2. Although I also see some patterns in the pollen data and the synthetic pollen-based indices suggestive of changes in the local climate, the record is quite “noisy”. I was wondering whether the authors have tried to check and validate their visual delimitation of the phases and sub-phases within the HS1 using numerical tools such as zonation. Divisive (e.g. optimal splitting) or agglomerative (e.g. CONISS) methods to delimit groups of samples with similar pollen assemblages in an objective manner (pollen zones) would be quite appropriate in this case because otherwise the position of the boundaries for the different periods seems to be somehow arbitrary.

Thanks for the suggestion. In this new version of the manuscript we added a plot with the detailed pollen diagram including the most characteristic pollen taxa from Padul (percentages of samples above 1%) for the age period between 20 and 11 kyr BP (new Figure S1). In addition, we ran a cluster analysis (CONISS) on the most important pollen taxa (i.e., *Quercus* total, *Olea*, *Pistacia*, Cupressaceae, *Artemisia* and Amaranthaceae) to group environmentally- and climatically-similar samples in the HS1 record using an objective statistical method (with the Tilia software). This is shown in new Figure S1. Note that the agglomerative CONISS methodology shows the same variability than visually, identifying 7 sub-phases within HS1. See Supplementary Information (*Palynological analysis* section) for more information.

3. I acknowledge the interest of using indices to summarize pollen data to facilitate their interpretation, but it is necessary to see the raw data (in the SI if not in the main text) because the abundances of specific taxa may be relevant for the palaeoclimatic interpretation of the results. I would kindly ask the authors to supply a pollen diagram for the relevant period under investigation so the readers can fully assess the significance of the dataset.

Thank you for the comments. We agree and in this new version of the manuscript we included the pollen diagram of the most characteristic pollen taxa (as explained above) (Figure S1). In addition, we also uploaded the complete pollen data to PANGAEA data repository (https://doi.pangaea.de/10.1594/PANGAEA.904053, *dataset in review*).

4. According to the authors, the HS1 lasted 2.8 kyr in Padul, which is also in agreement with other continental, marine and ice records. Then, do periodicities of 2000 or even 800 years make sense at all? To me, on due respect, perhaps not too much…

Sorry for the misunderstanding, but we did not want to say that the 2000- and 800-yr cycles only occurred during HS1. As the reviewer says, it would make no sense because HS1 only lasted 2800 years. We have clarified in the text that the spectral analysis was run on the xerophytes times series for the age period between 20 and 11 kyr BP. Therefore, the spectral analysis is showing the periodicities of these proxies for an age range lasting 9000 years. In addition, we have also removed the spectral analysis done on xerophytes only for HS1 (Fig. 4b in the previous version) and the spectral analysis of *Ip* data (Fig. 4c in the previous version) as they were not showing any additional information with respect to the Figure 4a (Fig. 5a in the new version) and can also produce misunderstandings.

SPECIFIC COMMENTS

Several words are repeated in the title and the keywords, i.e. Climate, Heinrich Stadial 1 and western Mediterranean. Perhaps the authors might consider adding some different keywords to make their paper easy to be found in scientific databases, e.g. southern Iberia, palaeoclimate or pollen analysis.

We totally agree. Thank you so much for the suggestions. We have modified some keywords.

L18 Replace “generating” with “characterized by”?

Changed.

L39-40 The authors might consider adding here a very relevant reference about last glacial rapid climatic variability in the North Atlantic context: Sanchez Goni et al. (2008) Quat. Sci. Rev. 27, 1136-1151.

We agree. We have included this reference.

L46 Replace “focus” with “have focused”?

Changed.

L47 Replace “short-scale” with “short-term”? I think it would be more adequate…

Ok. Replaced.

L49-53 At least part of the content of the SI should be moved here so the reader gets a better idea of the background that justifies this research.

Thank you for the suggestion. We think that including the environmental conditions of every phase of the HS1 (early, middle and late) from six different studies (including Padul) is too much information for the *Introduction* section. However, we have moved the Table (Table S1 in the previous version of the manuscript) to the main text (Table 1 in the new version), as we also think that this information is important to understand the background of the paper (as the reviewer suggests). We think that including this Table 1 in the main text could be enough, as it summarizes the text from the Supplementary Information.

L54 “Sedimentological” instead of “sedimentation”?

Yes, we agree. Changed.

L58 There is a mismatch between the time resolution indicated here, i.e. “61-yr”, and the values shown in the SI, i.e. “77-yr” and “131-yr”. Please, check and be consistent.

The 61-yr resolution was for the period between 18.4 and 15.6 kyr BP. In the SI the 77-yr resolution was for the period between 20 and 15.6 kyr BP and the 131-yr resolution for the period between 15.6 and 11 kyr BP. Accordingly, we have modified and simplified the main text in the *Introduction* and *Materials and methods* sections for a better understanding.

L81-83 Low percentages of xerophytic pollen during the LGM are quite unexpected. I have been checking the pollen diagram in Camuera et al. (2019) Quat. Sci. Rev. and pines were particularly abundant by that time around Padul. I was wondering whether the authors could provide a plausible explanation for these values because I consider this point would be worth to be discussed.

Thank you for the interest. Pons and Reille (1988) suggested that the decline in trees except *Pinus* could be triggered by very arid climate but under not extremely cold temperatures. In additions, the high *Pinus* occurrence could suggest less marked ocean advection and higher continental climate conditions. As explained in Camuera et al. (2019), *Pinus* abundance in this record seems to be related with treeline movements during transitions between warm/humid interglacial/interstadials and the coldest/most arid periods (such as the Heinrich Stadials) (see also Figure 9 from Camuera et al., 2019). Therefore, we can suggest that *Pinus* acted as transitional taxa between relatively warm and humid periods and the coldest and most arid climate phases.

As I already said in my general comments, I see the patterns that the authors point out but it is also true that there is a significant overlap in the values, especially around the boundaries. The authors could seek statistical support for their inferences using a zonation procedure. HS1a.1-HS1a.3 and HS1c.1-HS1c.3 are hard to accept. Perhaps increasing the number of samples would bring support to this proposal, but with the current record it is not possible to assess whether peaks in a single sample are palaeoclimatically meaningful or just outliers.

As explained above, we have carried out a cluster analysis using CONISS (Tilia software) on the pollen data from the studied time period (new Figure S1). This agglomerative statistical analysis supports the 7 sub-phases subdividing HS1 in the Padul record, validating our previous subdivision of the SH1 with a statistical method. We thank the reviewer for the suggestion, as it has been really useful to have a better and more robust statistical methodology for the validation of the subdivision of the HS1.

L129-153 The authors should discuss here the potential hard-water effect in their chronology.

Ok. In this new version we have added more discussion about the influence of the hard-water and reservoir effect on radiocarbon datings from lacustrine environments, and therefore, the possible limitation of our chronological control.

L146-148 References supporting this statement are needed.

References are in the following sentence, when showing the examples from the CAN speleothem (N Spain), lake Prespa (Macedonia, Albania, Greece) and MD99-2331 (NW Iberian margin) records.

L150 “Conditions” could be deleted.

Deleted.

L159 How the authors would explain the ca. 1000-yr offset between the onset of the Lateglacial Interstadial at Padul and most well-dated European records, including those from southern Europe, e.g. Monticchio, Trifoglietti?

We have included new paragraphs pointing into several causes that could have influenced the early record of HS1 in our mid-latitude region with respect to high-latitude areas. As explained above and clarified in the main text, the high-resolution dating methodology from Padul does not assure a perfect chronological control for the HS1. However, possible asynchronicities and early record of HS1 in this mid-latitude Mediterranean region as result of the provided/explained causes should not be ruled out.

SUPPLEMENTARY INFORMATION

L21 Please delete “one of”, because it is certainly the most recent HS.

Ok. We agree.

Regional and local settings. It is highly relevant that the authors inform about the bedrock in the catchment.

Done. We have included information about the catchment.

Chronology. Please, indicate which dates are new and which were published in previous studies.

We have clarified with letters ([a] and [b]) in the Table S1 (Table S2 in the previous version of the manuscript) the new samples analyzed for this study and samples analyzed and published in the previous study (Camuera et al., 2018).

L93 For the future, it would probably be worth to discuss the presence of Carpinus.

We agree. In the future, we would like to discuss the presence of both *Carpinus* and *Abies* during the last two glacial-interglacial cycles in Padul, as they are really interesting taxa that disappeared in the Padul record during the last interglacial period and the last glaciation, respectively.

Spectral analysis and filtering. At least part of this text should be moved to the main text.

Thanks for the suggestion. We have moved an important part of the spectral analysis to the main text, including the age range of the xerophyte data, periodicities, confidence intervals (and frequencies) and the software used.

REFERENCES

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