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 #2

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This paper presents the details of the recently published new Padul pollen record for the Heinrich Stadial 1 and Lateglacial interval.

The pollen record reveals significant changes over the study interval, presented in the form of pollen-based indices with established use in the study area. The record is also at a high temporal resolution offering new insights into centennial-scale variability during the study interval.

There are fascinating visual parallels between the pollen indices for the Heinrich Stadial 1 interval and SST records from the nearby W Mediterranean (Alboran Sea) which generally support the interest and interpretation of rapid climate variability during this interval.

The main difficulty for the manuscript is the chronology of the record. In essence, the Heinrich Stadial appears "too old" in the Padul record, and this creates difficulties for the analysis and interpretation. The manuscript seems to have a "split personality" – attempting to interpret both (A) the difference in ages between the Padul record and other records as a real and meaningful phenomenon, e.g. with implications for reservoir ages, etc. and (B) propose synchronicity of events between Greenland and S Iberia, e.g. as shown by wiggle-matched records in some figures and direct labelling of pollen changes with Greenland event stratigraphical terminology. It should be noted that conceptually (A) and (B) are mutually exclusive and they sit together very uncomfortably in the manuscript.

Regarding (A), the authors suggest that changes in marine reservoir effects might explain the difference in apparent age of the Heinrich stadial between Padul and the Iberian margin records. However, the logic is reversed here and the apparently older age of the Padul record cannot be explained away by marine reservoir effects which would tend to give older ages in the marine realm, not the terrestrial. Furthermore, the study of coupled land-sea tracers in nearby Alboran records (Comboureiu Nebout et al., 2009; Fletcher et al., 2010) already reveals a synchronous (within age model uncertainty) coupling of climate changes over the W Mediterranean and the high-latitudes, with possible modest enhancements of up to ∼200 years of the marine reservoir effect.

We thank the reviewer for the comments. We agree about the reservoir effect. The explanation about the reservoir effect in marine environments makes no sense and we have removed this sentence from the manuscript. However, we still think that even if the high-resolution chronological dating from Padul does not assure us a perfect age control, the possible asynchronicities between our mid-latitude region and high-latitude areas could have occurred and cannot be ruled out.

However, the reviewer suggest that the studies of coupled land-sea tracers in the nearby Alboran Sea from Combourieu Nebout et al. (2009) (ODP 976 record) and (Fletcher et al., 2010) (MD95-2043 record) reveal synchronous climate changes between the W Mediterranean and the high-latitudes and we partly disagree. If one looks in detail at the high-resolution proxy data from these two above-mentioned records and the high-resolution data from the 293G record from Rodrigo-Gámiz et al. (2011, 2014) (also from the Alboran Sea), one can see that there is not an exact synchronicity between these W Mediterranean records and the high-latitude Greenland records (climatically equivalent GS-2.1a dated at 17,480 – 14,692 yr BP according to Rasmussen et al, 2014) (see also Figure below).

Fletcher et al. (2010) suggested the beginning of this cold and arid HS1 period at 17.5 kyr BP and the end at 14.9 kyr BP in the MD95-2043 record from Alboran Sea. However, if we focus on the alkenone SST reconstruction from the same sedimentary record, it seems that the most drastic temperature decrease related with the beginning of this cold period could have occurred hundred years before, at ~18.1 kyr BP (see Figure below).

The 293G record from Rodrigo-Gámiz et al. (2011, 2014) (Alboran Sea) also displays an early HS1 response in this region, at least for the beginning of HS1, with changing values in both SST and Zr/Al at 17.9 kyr (see Figure below). This suggest that, according to this record, the environmental response during the onset of the HS1 in Alboran Sea was ~400 years earlier than in the high-latitude Greenland records. The end of HS1 is not as clear as the beginning, as the LDI-SST data is showing a transitional increase from 15.8 to 14.7 kyr BP and the change in Zr/Al is not as sharp as during the onset. According to their study, Rodrigo-Gámiz et al. (2011) placed the end of HS1 and beginning of Bølling-Allerød at 14.7 kyr BP, which seems to be approximately concordant with the end of the increasing trend in SST at 14.8 kyr BP and the end of the decreasing trend in Zr/Al (note inverted values) from ~15 to 14.8 kyr BP.

With respect to the ODP-976 record (Alboran Sea), the alkenone SST data show HS1 between 17.1 and 14.7 kyr BP, presenting differences with respect to the MD95-2043, 293G and Greenland records, especially for the beginning of the HS1 (see Figure below).

According to this, we still suggest that the environmental response of HS1 in our region does not seems to be exactly synchronous with respect to high-latitude areas. Several studies suggested early environmental responses in low- and mid-latitude areas during HS1 and deglaciation (early alpine glaciers melting, early Mediterranean Sea overturning circulation… e.g., Bonneau et al., 2014; Fink et al., 2015) (see the new version of the manuscript), but age uncertainties related with radiocarbon dating in marine and continental records cannot be ruled out. Therefore, age offset between mid- and high-latitude records may have been result of 1) the different environmental responses depending on the latitude/location, 2) imprecision in the chronology related to radiocarbon dating, or even 3) the sum of both factors. Further studies should be focused on these age differences between records at different latitudes in order to understand possible asynchronicities related with different environmental responses due to possible specific regional environmental conditions. Nevertheless, the main goal of this study is not to observe asynchronicities between different regions and latitudes but to show the environmental and climate conditions occurring in the southern Iberian Peninsula during the deglaciation and, for the first time, to describe the internal climatic variability of HS1 in seven sub-phases.

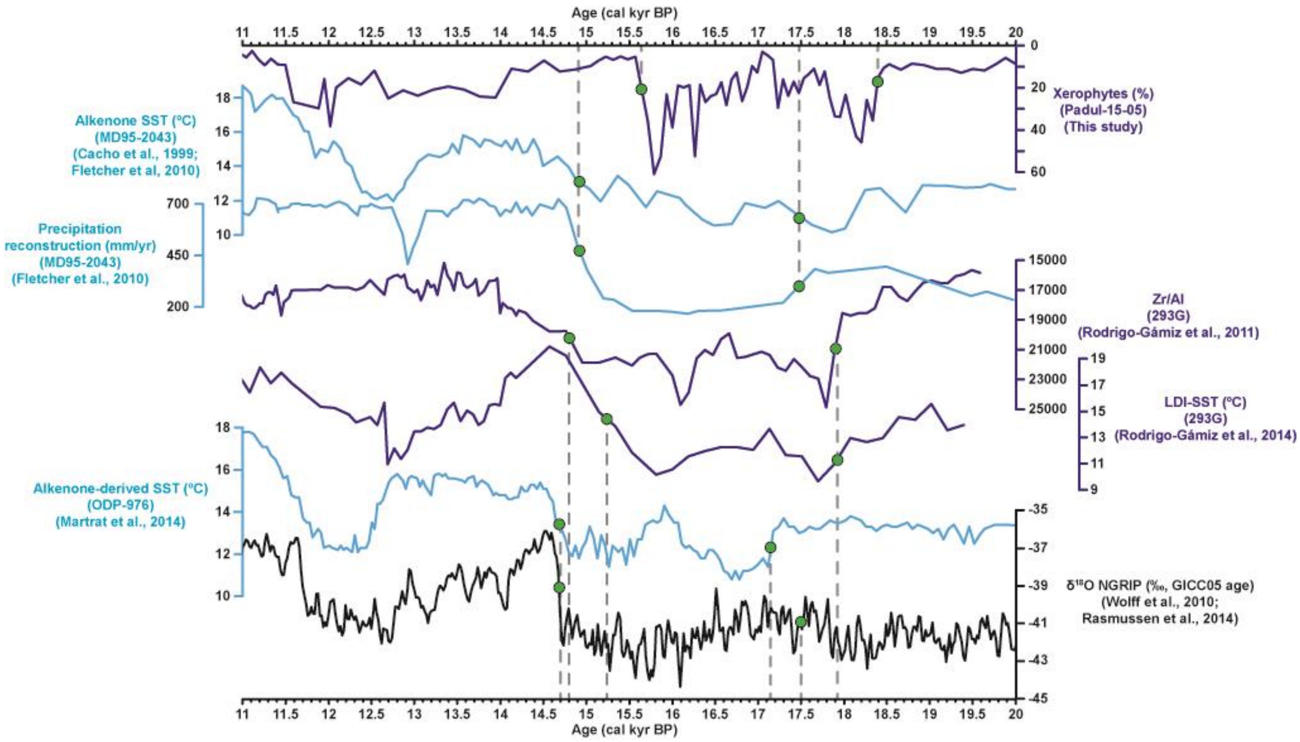

Figure. Different proxy records from the western Mediterranean. From top to bottom: Padul (dark blue), MD95-2043 (light blue), 293G (dark blue) and ODP-976 (light blue). The d18O record from NGRIP is represented in black. Green dots (and vertical dashed grey lines) show the age boundaries of HS1 (climatically correspondent GS-2.1a in Greenland) suggested by the correspondent studies.

Overall, I suspect that there are uncertainties in the site-specific age model which are not dealt with fully in the manuscript. Essential information for the validity of this study about stratigraphy, age control data and rejected dates must be included and discussed in the main manuscript and not placed in the supplementary material. Inspecting the radiocarbon data, it is evident that there are difficulties with reservoir ages or old carbon sources leading the authors to reject several dates obtained on bulk carbonates and gastropods. However, I do not see that it can be excluded that old carbon effects are not impacting also on the included dates made on bulk sediment. The authors need to deal with this more directly in the presentation of the record and ultimately the interpretation of the data. If the uncertainties in the age model are too great to support (A) then this shortcoming should be accepted and the implications of (B) can still be tentatively explored.

Thank you. We really appreciate these comments.

Firstly, we would like to apologize for the mistake about the dated material. In the Table S1 (Table S2 in the previous version of the manuscript), we said that samples were “bulk carbonate” and “bulk peat” and this was wrong. The six new additional radiocarbon samples that we used to improve the age-depth model for HS1 were taken from bulk carbonate and bulk peat lithologies, but were pretreated with HCl and HF in the laboratory of the Stratigraphy and Paleontology department at the University of Granada. Therefore, after removing the inorganic carbonates from both peat and carbonate sediments, the final residue sent to the BETA analytic laboratory was the organic plant residue, and not the bulk sediment itself. The two previously considered “too old” radiocarbon samples that were not used in the previous version of the manuscript have been included in the new Bayesian age-depth model, as all the six new radiocarbon samples analyzed should have similar reservoir ages and we do not have objective reasons to exclude them.

In the new Table S1 (Table S2 in the previous version) we have also included the $\delta^{13}C$ and C/N values from the used radiocarbon samples in order to understand the origin of the organic carbon source. The observed values (for both $\delta^{13}C$ and C/N) seem to be in agreement with vascular C3 land plants (Meyers, 2003; Meyers and Lallier-vergés, 1999), suggesting that the organic carbon source should be principally atmospheric and the reservoir effect related with the organic carbon from plants should not be excessively high. Nevertheless, as explained above, we are aware that the high-resolution radiocarbon dating from the Padul record does not assure a perfect chronological control, and this possible "old carbon" problem has also been taken into account in the new version of the manuscript.

Moreover, we built a new age-depth model based on Bayesian modeling for the last 30 kyr BP in order to have a better control of the age uncertainties, including all the new six AMS radiocarbon samples analyzed for this study and the radiocarbon samples already published in Camuera et al. (2018). The new Bayesian age-depth model used in this new version of the manuscript is based on 40 radiocarbon dates (including three specific compound radiocarbon samples). All this information and related changes have been included in the new version of the manuscript, and Figure and Tables have also been modified (new Figure 2, *Methods* from Supplementary Information and Tables S1 and S2).

Without a more open and direct appraisal of the age control issue, I do not think that the time series analysis can be sustained. Although there do appear to be interesting pseudo-cyclical patterns in the proxies for some time intervals, the authors must be cautious about over-interpreting weak spectral signals (e.g. at 80%, 90% confidence levels) and cautious about identifying spectral peaks at high frequencies occurring close to three times the sampling resolution which may be spurious.

Thank you so much for the suggestions. We totally agree with the reviewer about identifying spectral peaks at high frequency and close to 2-3 times the sampling resolution. Therefore, the spectral peaks at ~200 years have been removed, as they are not statistically significant and do not provide with additional information for the main goal of the manuscript. In addition, we have also removed the spectral analyses of xerophytes for the HS1 and *Ip* data, as they are not show any additional information with respect to the spectral analysis shown by xerophyte pollen data between 20 and 11 kyr BP. Please see these changes in Figure 5 (Fig. 4 in the previous version).

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