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.**

Response to comments by Ola Kwiecien: "Comment on the manuscript"

This is an impressive set of data; I have seen it at conferences and I was looking forward to seeing it published, but this work leaves my professional curiosity unsatisfied and disappointed. The authors aim to shed light on the centennial climate variability in the Western Mediterranean region during the late glacial and document a three-phased nature of HS1. In fact, they fail. The new high-resolution data are exciting and have great potential, but the interpretation is somewhat careless. E.g.: I do not see how the HS1 in Spain could lead the hemispheric (?) signal by ca. 1 ka? This is a bald and provocative statement and the authors suggest "different environmental responses" (lines 144) to remedy this problem. This argument is not convincing, in particular, with-out explaining and elaborating on the nature of the purported responses. Perhaps the answer lies in the uncritical approach to the Padul sequence 14C-based chronology? Independent of how good the age model is – it is just a model. A critical discussion of potential flaws of the Padul chronology and their implications might solve the problem.

We thank the reviewer for her excellent comments, and we have endeavored to incorporate them into our thoughts and into the manuscript, as well as provide explanations where we either were unclear, or where we disagree. It is important to remember that, while one outcome of this analysis is that our age model suggests an early beginning to HS1 from the Padul data, the primary goal of this analysis was *to investigate the characteristics of potential subdivisions of the HS1 as expressed in the Padul record*. With this in mind, we address the comments of the reviewer directly below:

We agree that there are limitations in radiocarbon dating. However, in the Padul-15-05 sedimentary record we tried to reduce age uncertainties for the last 20,000 years by providing a total of 27 radiocarbon samples (including 2 radiocarbon samples on specific compounds) (Camuera et al., 2018). More specifically for the HS1 interval, we have 10 AMS radiocarbon dates, ranging over an ~2400 year interval, between 17,980 and 15,658 cal yr BP (see Figure 2a and Table S2). Even though we believe that this is a very good spread of ages for this time period, one potential flaw in our age model is that two short gaps of less than 500 years appear in the probability distributions of this sequence (Table S2). However, combined with the fact that 9 of the 10 ages in this interval are in stratigraphic order suggests that this age model has considerable power for the entire interval.

Further, in order to strengthen our model and prevent circularity, we have not tuned our model to any other paleoclimate records, which would have eliminated the capacity to describe succession of events and pre-conditioning triggers. We were surprised by the results we obtained – that of an earlier record of HS1 from the Padul sediments – when compared to the other paleoclimate records. Our results then became the impetus for the current manuscript.

Nowhere in the paper did we mention that the HS1 in Spain led the hemispheric signal. We apologize if our explanation was not clear enough. Here we wanted to show that in southern Iberian Peninsula (where the Padul record is located) environmental signals associated with the HS1 (e.g., high aridity) preceded the age established for this event in other records located at higher latitudes. This is an outcome of our age model, which we believe, but it is not the primary object of this analysis. Even so, several previous studies support an earlier record of deglaciation and asynchronicity of these events. For example, an early tropical warming has been described in recent studies related to (1) an early deglaciation of the Antarctic Peninsula (Weber et al., 2014) along with (2) an early rise in global sea level (Lambeck et al., 2014), leading to (3) early responses of the north hemispheric mid-latitude terrestrial and oceanic records (Jackson et al., 2019). (4) Early warming in tropical SST is also described before any considerable ice volume change took place in high latitudes (Romero et al., 2015). Therefore, we suggest that environmental responses to HS1 in low- and mid-latitudes could have preceded the climatic response of high-latitude regions.

With respect to the causes producing these “different environmental responses” triggering the early environmental response in our region, we could suggest different hypotheses. In a new version of the manuscript, we have included that the early European ice-sheet retreat and alpine glaciers melting from the Alps and Apennines at ca. 20,000 years BP suggest an early meltwater influx into the Western Mediterranean through the rivers from the northern Mediterranean borderlands (Bonneau et al., 2014). This conditions affected the Mediterranean Sea overturning circulation (Fink et al., 2015) and could have produced the early beginning of cold and arid conditions in this region, as it is the main factor controlling the climate in this area (Rohling et al., 2015). On the other hand, the enhanced MOW towards the Atlantic occurred ~15,500 years BP (Rogerson et al., 2010), several hundred years before the usually assumed end of HS1 (or the onset of the B-A) at ~14,700 yr BP. According to this, it could be hypothesized that the early enhanced MOW and the own Mediterranean Sea thermohaline reactivation could have previously affected climate at the lower latitude Mediterranean area, and could have caused an earlier environmental response of the warm and humid B-A interstadial in this region.

Nevertheless, the main goal of this manuscript is to show, describe and discuss the internal climatic variability of HS1 with its subdivision in 3 main phases and 7 sub-phases in the Padul-15-05 record, and not so much on the age discrepancies of HS1 with other paleoclimatic records.

Also, while the authors document the three-phased nature of HS1 they discuss neither the causes nor regional implications of the described features. Why does the division of HS1 matter at all?

As detailed in our Supplemental material, it has been known for some time that the HS1 was complex, perhaps with 3 parts. Here we show that it may have been even more complex than previously thought, and can be subdivided into 7 sub-phases based on the paleoenvironmental signal of the Padul record. In addition, we compared our record with other paleoclimate records from the studied area, showing a similar climatic pattern during HS1 (see Figure 3) and pointing to a regional feature. The possible causes of this variability are discussed in lines 202-219, mostly related with millennial- and centennial-scale climate variability forced by solar output variations and the influence of the North Atlantic polar front displacements in the land-ocean temperature contrast and in moisture advection between both environments. Thus, it might be important to look at other HSs in greater detail to see if those events are more complex as well.

In the new version of the manuscript we have also included that these relatively short-scale periods (such as Heinrich Stadials or Dansgaard/Oeschger events) and their smaller-scale internal oscillations observed with high-resolution data are important in order to understand how the environment reacts to fast climate changes and the causes affecting them (such as fast changes in the Mediterranean thermohaline circulation).

Further, the authors call for solar activity as a driver of changes in the Padul proxies. Can they elaborate on the exact mechanism? How solar activity translates to floral assemblage changes?

Solar activity has been proven to significantly affect past climate variability, which in turn affected vegetation dynamics. Solar activity changes could affect temperature, air and water masses and thus precipitation on land affecting plants (Tinner and Kaltenrieder, 2005; Lozano-García et al., 2007; Ramos-Román et al., 2016). In addition, Kofler et al. (2005) also suggested that solar output caused decreases in pollen production and treeline shifts in the Alps, which could also be affecting vegetation in the Sierra Nevada and, therefore, in the Padul area.

Last but not least, the spectral analyses results seem a bit at odds with common sense. 800 yr cyclicity during HS1 event of less than 3 ka duration is already suspicious. Periodicity of 2000 yr within HS1, BA and YD (line 232-233) is simply absurd! The YD itself only lasted for ca. 1000 yr.

Sorry for the misunderstanding. We did not want to say that the ~2000-yr cycle occurred during the HS1 itself (or YD itself) - we know that this would make no sense. We meant that this cycle can be identified from 20 to 11 cal ky BP, which includes the HS1, B-A and YD (Figs. 4a and 4c and Figs 5a-c). We have clarified this in the new version of the manuscript:

Lines 172-173: "The spectral analysis on xerophytes and *Ip* presented ~2000, 800 and 500-yr cycles from 20 to 11 cal ky BP (Figs. 4a and 4c)". We have also removed the 200-yr cycle (also from the Figure 4) as it is not representative due to the sampling resolution.

Lines 232-233: "The main periodicities obtained for climatic oscillations of ~2000 and 800 yrs between 20 and 11 cal kyr BP seem to be related to solar forcing."

We have also changed lines 183-185, clarifying that the maximum values of xerophytes during the 2000-yr cycle at ~18 and ~16 cal kyr BP would mark the HS1c and HS1a, whereas the minimum value at ~17 cal kyr BP would mark the HS1b (Figs. 5a and 5b).

Lines 191-192 have also been modified. We have clarified that the maximum values of the 800-yr cycle based on xerophytes at 18.2, 17.3, 16.5 and 15.8 cal kyr BP would mark HS1a.1, HS1a.3, HS1c.1 and HS1c.3, respectively. On the contrary, the minimum values of the 800-yr cycle at 17.7, 17 and 16.1 cal kyr BP suggest the HS1a.2, HS1b and HS1c.2, respectively (Figs. 5a and 5c). Coincident minima obtained in the 2000 and 800-yr cycles at ~17 cal kyr BP define the HS1b interval, a period of moderate humid conditions within HS1.

This 2000-yr periodicity is pervasive for the last 20 kyr BP, at least in Southern Iberia, associated to solar activity and/or monsoon activity (Rodrigo-Gámiz et al., 2014). Subsequent studies demonstrate that this "monsoon-like" signal is linked to Nile input (Bahr et al., 2015), transmitted by Mediterranean thermohaline circulation to western Europe (Kaboth-Bahr et al., 2018) and associated with winter precipitation for the last 1.3 Myr in Southern European lacustrine records (Lake Ohrid) (Wagner et al., 2019). The absence of this pervasive 2000-yr signal at the Padul-15-05 record (very similar to Lake Ohrid record) would be surprising, since both areas are very sensitive to winter precipitation synchronized with African monsoon.

We have removed the Figure 4b as it is only showing the spectral analysis for the HS1, it is confusing and it is not providing additional information with respect to the Figure 4a.

Paleoclimate research is so much more than tuning wiggly curves and finding prescribed periodicities in proxy records! Valid, original observations call for careful and thorough and original interpretations rather than preaching to the choir using empty but catchy phrases and unsubstantiated claims.

We totally agree with this comment and that is exactly what we are trying to do in this study, avoiding tuning to other well-known records (such as those from Greenland), trying not to find prescribed periodicities as result of the tuning effect and avoiding circular reasoning.

I wish the authors will address mentioned points in the revised version of their manuscript.

Best regards,

Ola Kwiecien

We hope that our new manuscript version will be more convincing for Dr. Kwiecien. In any case we are thankful for the comments.

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