

Dear Editor,

First, I would like to explain the delay in our reply. Due to severe medical issues in my family, as a senior author and PI of the project where this research was carried out, I could not work as regularly, and this situation prevented me from an earlier answer on this work. We hope you and the reviewers can understand this exceptional situation.

Kind regards

Ana B. Marín-Arroyo

Dear reviewers,

As authors, we deeply value your perspective and the time you have invested in improving the quality of our contribution to the Biogeosciences journal. We sincerely apologize if any misunderstandings led you to believe we did not appreciate your suggestions. Upon thoroughly reviewing the manuscript, we have diligently followed the reviewer's instructions. In instances where we have not, we have provided an adequate justification. As the reviewer suggested, we noted inconsistencies in our justifications and have now addressed them accordingly. We regret our mistake, and we hope everything is now satisfactory.

Our detailed reply involves a coloured file (made in Excel) where we have 1) enumerated all changes made in response to the reviewers' suggestions, 2) summarized our reasons for some suggestions that were not fully implemented, and 3) described some partially implemented changes and their subsequent enhancements. Moreover, we have specified changes not initially addressed in the reviewer responses but included in the latest draft submitted. The coloured document will facilitate this new reviewer process, where answers are provided to each individual suggestion provided by reviewer 2. In summary, from the 35 individualized suggestions from reviewer 2, we have accepted and justified 30 of them; 2 were not implemented, and 3 were partly implemented and justified to the reviewer and within the paper.

Regarding the comments provided by the editor to reviewer 2 comments, we precise the following issues:

- Title (R2-34): We acknowledge that we were unclear in our decision regarding the title, with changes from the online response to reviewers. Initially, we agreed to change the title, but upon further review, we realised that the suggested title was inappropriate in English. We believe the current proposal aligns with the reviewer's advice and provides a closer idea to the original.
- Chronologies adjustment (R2-3): The chronologies were subsequently adjusted by discussion among authors through the reviewing process. We agree that we didn't justify this change adequately in our previous response to reviewers. We have reviewed the chronological methods, which are explained in a specific section in Methods (3.1. Dating methods). We hope this explanation is more precise now.
- Northeastern Iberia (R2-21): Regarding the use of "northeastern Iberian Peninsula" instead of "Mediterranean," we found relevant this suggestion, and it was implemented throughout the document, except in cases where we referred to the Mediterranean area in general and not specifically to the Canyars site. We acknowledge that the reviewer was correct.

Regarding the 30 accepted changes from reviewer 2, we identified seven cases in which changes were already implemented but are now being improved in the draft submitted today (R2-1, R2-

3, R2-4, R2-10, R2-21, R2-23, R2-31). In our view, these are minor changes, but we believe they now better fit the reviewer's expectations. Expect the site chronologies (R2-3) explained in detail above. Please refer to the attached document for further details.

Only two suggestions were not accepted: the new title proposal (previously explained; R2-34) and a suggested figure for climatic estimation evolutions (R2-33). We justified the second case in our previous response to the editor. In short, we chose not to include the figure because the estimation of paleotemperatures was approached tentatively, and we preferred not to focus on this discussion in this paper.

Afterwards, there are three suggestions from reviewer 2 that were only partly implemented (R2-8, R2-9, R2-11). All three are related to temperature or precipitation estimations, probably the most complex part of this manuscript. Our primary focus for this paper was not to delve deeply into these aspects, as explained. These decisions were extensively explained to the reviewer and justified considering the reviewer's argumentation within subsection 3.4. Specifically, it was suggested to introduce some corrections regarding temporal isotopic composition and age-specific correlations for  $\delta^{13}\text{C}$  (R2-8) and  $\delta^{18}\text{O}$  (R2-9), considering fluctuations experienced in these elements throughout the Pleistocene. In both cases, we justified our decision not to apply age-specific correlations based on the uncertainty of the dates, which was also pointed out by the reviewer. Nonetheless, we applied a general correction for both  $\delta^{18}\text{O}$  and  $\delta^{13}\text{C}$ . Furthermore, the reviewer suggested correlations for temperature estimations, differentiating between the Atlantic and Mediterranean regions and between cold and warm seasons in R2-11. We chose to maintain a wide-geographic correlation considering unknown past atmospheric circulation patterns and the limited data derived from IAEA stations. However, as suggested, we decided to include different equations for summer, winter, and mean annual temperatures, and we opted to apply the linear regression models proposed by Pederzani et al. (2021). This last aspect was changed from the initial online response and modified after the reviews were implemented in the text, as we noticed that this change substantially improved the quality of the data provided.

Finally, we detected an error in climatic estimations when implementing reviewer suggestions related to error calculations, which led us to explore alternative solutions and necessitated a significant investment of time. As explained in our last draft, responding to the editor: "During the calculation of errors, it was identified that the correlations utilized for the conversion from  $\delta^{18}\text{O}_{\text{phosp}}$  to  $\delta^{18}\text{O}_{\text{mw}}$  do not correspond to the most updated version. The equations now chosen are the same as those employed in the Axlor site study (Pederzani et al., 2023), which includes a larger number of specimens and is more comprehensive. This, however, implies the modification of Figures 4 and 5. Numbers have also been updated in Tables 3 and 4, in the text and the Supplementary Information (SI). No significant implications have been detected, and the general interpretation aligns with the previous findings."

We believe that these explanations will help the editor and reviewer appreciate our time carefully implementing their suggestions to improve the quality of our manuscript.

Kind regards,

The authors

REVIEW CODE	IMPLEMENTED?	Reviewer comment complete	Summary of reviewer comment	How was implemented?	Examples	Changes from online reviews?	How is implemented in the new draft (05/2024)?
R2-1	yes, and now improved	Authors suggested that this type of paleoenvironmental studies is key to understanding past climate and human interactions. See for example abstract lines 18-19 or the introduction. Authors must keep in mind that the paleoenvironmental reconstructions that have been performed in these archaeological sites are "discontinuous points" in the paleoclimatic record of the Iberian Peninsula (see for example the chronologies in Fig.5).	Highlight that the paleoenvironmental reconstruction provided represent discontinuous points in the paleoclimatic record	In the introduction, we reflected on this idea and insisted that our primary focus is climate reconstruction linked to human presence at the sites. (e.g. "These analyses provide high-resolution snapshots of ecological information from animals accumulated during human occupations at the caves")	In lines 88-90		The implementation of this perspective has been improved through the modification of some sentences in the introduction. Even if some sentences were already corrected to reflect this idea, we have included this perspective with new changes along the introduction (lines 32-34, 56-57, 77, 105).
R2-2	Yes	(...) the most accurate climatic records for the studied periods are marine, lacustrine and speleothem records, where one can observe the "obedient" fluctuations of past climate. Authors should acknowledge this point in the manuscript as well as comparative with these records (there are many for the IP, for example Martini et al., 2004, 2007; Pérez Mejías et al., 2019; Moreno et al., 2012; González-Sampériz et al., 2020; Camuero et al., 2019, 2022, among others). Some of them also discuss about vegetation changes in NE Iberia, close to the study areas, which would help authors contextualise and discuss their interpretations about the animal diets. In fig. 2 authors included the d18O record of Greenland, but they did not discuss their data according to this record. As I mentioned before, they should compare and discuss the Iberian records instead, since there can be some temporal offsets between some of the events of the NGRIP curve and the Iberian/Mediterranean records. Taking this into account, objective 3 (line 96) is not totally achieved. Similarly, the speleothem records from the north of Iberia would help constrain temperature/precipitation patterns; there are many of these records in the Vasco-Cantabrian area. Other records from the Iberian Peninsula, the Mediterranean coast or the Iberian margin would show the general climatic patterns for this period. Authors should discuss their data (agreement or disagreement) according to these continuous records in order to have a big picture of the paleoclimate and paleoenvironments in the studied period.	Discuss out data (agreement/disagreement) with other local regional climatic records (some references provided) and also with continuous records (marine, NGRIP) in terms of vegetation/animal diet and climate/environment changes.	All these references have been incorporated, mentioned in the introduction and discussed in the discussion section. Indeed, this suggestion has notably improved the Discussion	Section 1 (lines 81-82) and Section 5.4		
R2-3	yes, and now improved	Chronology. This is a very important part of the study and should be presented in a subsection in section 2 or in the methodology (section 3). Please, explain deeply the absolute chronology of the levels where the unglazed remains were collected. (...) Please, specify the dating method: ESR, OSL, or 14C in Fig. 2. What are the grey bands in Fig. 2, studies? Please explain (...) What is the meaning of the green colour in one date at around 40 ka in Fig. 2 (Canyars)? What is the meaning of the dates (dots and bars) in Fig. 2. Do they represent a single dating event, or a sum of distribution of various dates? Please explain this in the caption and in the main text. (...) Please explain the dates of the sites where the statistical approach to obtain it (such as the ones that can be obtained from OxCal software)? Do they represent the ages of the whole archaeological sites, or the ages of the sites where the unglazed teeth were taken? This is very important to specify in the main text since the age of the remains could vary. Therefore, the caption of Fig. 2 should explain these details and there might be a subsection in the main text explaining the chronology of the levels where the remains were taken. This is crucial to validate the discussion of the manuscript.	Explain the absolute chronology methods and calibration methods (radiocarbon curve IntCal2020) in a subsection within the manuscript and include details in figure 2: dating methods, green colour and grey bands meaning, dots and bars meaning, statistical approach and software. Review inconsistencies between explanation provide to reviewer and manuscript implementation in Fig. 2, section 2 and Appendix B.	We have specified the methods of calibration and date origin for each level in section "2. Archaeological sites and sampled material." Appendix B includes all the original ESR, OSL, and 14C dates for each level and 14C calibration, as well as an explanation of average estimation by levels. All formal changes indicated in Figure 2 are included. (*) This review changed from the initial online response.	Appendix B (B1_Dates) + New Appendix C		This issue has been largely improved in this last draft from the previous one because we detected some inconsistent results derived from the calibration method during the reviewing process (*). This includes new changes in the Figure 2 caption, Section 2, and Appendix B. A new section 3.1 and a new Appendix C, providing details on calibration dates, have also been created.
R2-4	yes, and now improved	Takin into account the confidence intervals of the ages (I suppose 2 sigma for 14C)->Line 98-99. "The chronological resolution in the study areas for this period allows us to correlate regional paleoenvironmental changes with global records"; this would be only true for the two sites younger than 30 ka, since the dates of the other sites might overlap axial and interstadial (and their probability distributions are very large). (...) In any case the chronology of the samples	The chronological resolution do not allow to correlate our levels to global climatic changes	We have modified this sentence: Considering the error chronological margins and the limitation into a straightforward correlation with a single climatic stadial (GS) or interstadial (GI) we have modified the sentence "The chronological resolution in the study areas for this period allows us to correlate regional paleoenvironmental changes with global records".	Lines 111-113		In this last draft, we finally decided to remove this sentence
R2-5	Yes	Authors mentioned that they did not carry out any pre-treatment to remove secondary carbonates, but did authors check the potential presence of secondary carbonates or the preservation of carbonates? This is very important since all the results are based on these values (there is no data of d18O in phosphates). The physical cleaning that was carried out would not remove all the potential secondary carbonates. Secondary carbonates are very common in archaeological contexts such as karstic caves (like the ones studied here), and would modify the isotopic composition of carbonates if they are not eliminated. This must be double checked before stating the sample preparation. Authors did not mention methods to double checked that the isotopic signal was the pristine one. They only mentioned in line 503-504: "The carbonate content in our samples, ranging from 3.3% to 6.3%, is similar to the proportion found in modern tooth enamel, suggesting no immediate indication of diagenetic alteration". However, there is no explanation about the methodology used to calculate this percentage of carbonate. Authors should explain this in the methodology section and add the % of carbonate in a table (e.g. Table 2).	Explain if some methods are use to double-checked absence of secondary carbonates, explain the method of carbonate content calculation and include it in Table 2	We answered the reviewer with all the methods employed (calcium carbonate content, d18O in phosphates from Axlor) and explained that any method or pretreatment can totally assure this issue. We include carbonate content in Table 2 and the explanation in Appendix B.	Table 2, Appendix B (B1_Samples Raw), Section 3.3, and lines 603-604		
R2-6	Yes	Regarding the potential treatment to remove the organic matter, authors said: Lines 145-151: "For this reason, in this work, most of the samples were not pretreated, except for the equid samples from Labeito-Koba and Alzaitzate III, and the cervids and equids from El Otero that were sampled and pretreated in the context of the initial project. Pretreatment followed was established by Estalase et al. (2002), where around 7 mg of powdered enamel was prepared and pretreated with 3% of sodium hypochlorite (NaOCl) at room temperature for 24 h (0.1 ml/mg sample), and thoroughly rinsed with deionised water, before a reaction with 0.1M acetic acid for 4 h (0.1 ml/mg sample) equivalent protocol in Jones et al., (2019)". And afterwards in lines 453-456: "In the case of equid samples from the Vasco-Cantabrian region, it should be considered that they have been pretreated with a combination of NaOCl and acetic acid, which could potentially affect the isotopic values. Samples after organic removal pretreatment can potentially show either higher or lower $\delta^{13}C$ values and higher $\delta^{18}O$ values based on previous experiments (Pellegrini and Snoeck, 2016; Snoeck and Pellegrini, 2015)". So, my doubt is, why did authors treat whole batches of samples to evaluate these "side effect" instead of applying both protocols (with pretreatment and without pretreatment) to aliquots of the same samples? Although they finally ended up that "the influence of the pretreatment on our samples is deemed to be limited.", this was a risk, and now they cannot be 100% sure about this potential influence. Was there any reason to measure the samples where the organic matter was removed in a different IRMS. If yes, please, explain.	Justification on pretreated some samples and non-pretreated others	We have explained that the cause is related to different research phases of the project within the Evocadapta group.	Lines 182-183		
R2-7	Yes	Line 162-166 and throughout the calculations and the manuscript. Authors referred to 'E' as the fractionation factor. The symbol E is traditionally the enrichment factor, not the fractionation factor (alpha). E= (alpha-1)x1000‰. So, what factor have authors applied eventually: fractionation or enrichment factors? Are these factors mixed in the text and in the calculations? This has to be clear, and if the factors are wrongly applied (enrichment instead of fractionation factor), correct the calculations. Although both factor are related (E=(alpha-1)x1000‰) the obtained results would differ, and thus, the derived potential interpretations.	Review if E is the fractionation factor or enrichment factor was applied.	We detected it was a terminological confusion: fractionation was used instead of enrichment. This was reviewed in the paper	Different parts of the paper		
R2-8	Partly implemented	Lecuyer et al. (2021) performed the calculations to correct the effect of atmospheric CO2 (difference of 1‰ and CO2 concentration) for the LGM, so, these specific CO2 corrections can only be applied for the LGM, but in the present manuscript there are no samples for the LGM (23-19 ka). In addition, the correction for the isotopic composition of atmospheric CO2 should be done specifically for each age of the studied samples, instead of using a general average 7‰, as authors mentioned a variation of up to ca. 1‰, would imply a change in 150mm of precipitation. Check for example the isotopic composition of atmospheric CO2 reconstructed from ice records for the Late Pleistocene that ranges from -7 to -6.5 ‰ (Eggleston et al., 2016; Paleoclimatology, among others). So, please, apply age-specific CO2 corrections. I mean, when you are quantifying climatic variables, you should reduce the potential error sources. These errors increase by applying a general unspecific correction for all the data (the same affirmation could be applied to all corrections of the isotopic data in the manuscript).	Considering variation on isotopic composition of CO2 during the Pleistocene, apply age specific CO2 corrections for all 13C and not a generic correction to avoid errors	We agree that, ideally, corrections should be age-specific. However, considering the chronological uncertainties of some of the older levels included in this work, we believe this could complicate the final interpretation. We, therefore, decided not to implement it, but we explained these CO2 variations; we mentioned the identity of age-specific corrections (as well as provided references) and the uncertainties related to MAP estimations.	Section 3.4 (lines 251-253, 260-263)		

REVIEW CODE	IMPLEMENTED?	Reviewer comment complete	Summary of reviewer comment	How was implemented?	Examples	Changes from online reviews?	How is implemented in the new draft (05/2024)?
R2-9	Partly implemented	Authors proposed the above-mentioned (oversimplified) correction for the change in the isotopic composition atmospheric CO <sub>2</sub> , but they did not apply any correction related to the change of the isotopic composition of the sea-water during the Late Pleistocene, which is the main moisture source for rainfall (I would not mention the moisture and precipitation due to inland evaporation/recycling in the Iberian Peninsula, even during cold periods (Kirkle and Dominguez-Villar, 2014), in order to simplify this interpretation). The global isotopic composition of the rain during colder/warmer periods (glacial/interglacial, stadial/interstadial) differs, not only due to the isotopic fractionation caused by temperatures, but also due to the accumulation/release of the lighter water isotopes in the ice sheets/glaciers during cold/warmer periods (Dansgaard, 1964), among other factors affecting the global isotopic composition of sea waters. Therefore, in order to obtain a reliable isotopic data related to precipitation, the obtained δO <sub>18</sub> values has to be corrected to remove this effect. See for example, Niedermeyer et al. (2010) or Garcia-Alix et al. (2021) approach to correct past hydrogen isotopes from vegetation, or even Fernandez-Garcia et al. (2020) for fossil mammals in the studied period of this paper.	It is proposed to apply a δ18O <sub>sw</sub> correction considering δ18O oscillations in sea-water. Preferentially, an age-specific correction, considering δ18O glacial-interglacial fluctuations.	Considering the chronological uncertainties, we find it more consistent to apply a general correction in δ18O <sub>sw</sub> (for the MIS3 period). It is explained in section 3.5. This proposed changes in temperatures estimations along the text and in some tables and figures.	Table 3, table 4, figure 4, figure 5, section 3.5		
R2-10	yes, and now improved	Where did these obligate drinkers drink (water source)? Directly from the rain? Rivers? Lakes? Unless they directly drink precipitation waters (oversimplification), this would imply more isotopic fractionation and would also mask the temperature signal. This is even more important in the studied glacial period, and especially in the stadials? Apart from the potential enhanced rain evaporation due to low atmospheric moisture in glacial times (Dansgaard, 1964), no significant evaporation in lakes, ponds (and even in vegetation-enhanced evapotranspiration) during dry periods have been demonstrated by different isotopic studies (based on freshwater gastropods, bivalves, or ostracods and even from leaf wax isotopes of freshwater and terrestrial plants. Please clarify this issue, and explain this constrain in the methodology since it would affect the reconstructed temperatures.	Water sources of the animals studied and implications in temperatures estimation. Justify this in the text	In response to the reviewer, we explain possible water sources and implications for δ18O interpretation. In short, evaporation and aridity do not seem to impact our samples, and for some individuals, we justify a seasonal pattern reflecting seasonal rainfall. We included, however, explanations of the impact of the non-temperature effect in the manuscript.	Section 5.2		We have reconsidered this response and added some explanations in the current subsection 3.5 (lines 284-289).
R2-11	Partly implemented	Precipitation source (from Atlantic Oceanic modes and Mediterranean dynamics). In the Iberian Peninsula, the isotopic composition of precipitation is highly affected by the moisture source in the present - and in the past - (Atgüés y Diaz Tejedor, 2005; Calle-Jaentón et al., 2001; Dominguez-Villar et al., 2013; Kirkle and Dominguez-Villar, 2014; Moreno et al., 2010, 2012, 2014, 2021; Toney et al., 2020; Garcia-Alix et al., 2021; Schirmacher et al., 2020, among others). Therefore, temperature-isotope fractionation equations would not work that well to reconstruct past temperatures. When proper analysis of the isotopic signal of precipitation are performed in Iberia, there are sampling stations (precipitation, temperatures, isotopes, and moisture sources) where the isotopes from precipitation do not correlate well with temperatures (Moreno et al., 2021). This thoroughly study of the isotopic composition of precipitation in northern Spain ended up with "although important, air temperature only affects the δ18O variability and is therefore not the only control". This issue is even more important in coastal areas, as the ones studied in this paper, and even more in the Mediterranean coast (at present there is some influence of amount effect in the Mediterranean areas of Catalonia; Moreno et al., 2021). Therefore, in the best oversimplified case-scenario (not considering previous comments 5 and 6, and admitting large errors due to these potential source and amount effects) if we would want to calculate temperatures from the isotopic values of precipitation we would need an equation for the Vasco-Cantabrian region, and another one for NE-Mediterranean (with different precipitation patterns and forcing). And even more, since atmospheric patterns, and therefore, moisture sources, are not the same during the warm and cold seasons (see the above-mentioned studies), specific equations for cold and warm seasons should be applied to reconstruct "summer" and "winter" temperatures.	In temperatures estimations based on δ18O, consider moisture sources and other effects different from temperature-effect is dominant. Develop specific equations for Atlantic and Mediterranean and for cold and warm seasons.	Considering this unknown past atmospheric circulation patterns and the limited data derived from IAEA stations, we preferred a wide-geographic correlation. In the final review, we decided to include different equations for summer, winter and MAT finally. Considering the reviewers' argumentation in section 3.5, these decisions are largely explained and justified. (*) This review changed from the initial online response.	Section 3.5		(*) During the online review, we did not follow the advice to adjust the correlation to cold and warm seasons, but during the reviewer's implementation in the text, we noticed that this change significantly improved the quality of the data provided.
R2-12	Yes	"MAT was calculated from the δ18O mean value between summer and winter in each tooth before modeling to reduce associated error". However in caption Table 4: "For some profiles with an unclear seasonal shape, MATs were deduced from the original average (teeth without a seasonal profile)". So, what is the correct methodology? In any case, according to the methodology section, MAT was calculated before modeling to reduce associated error, but summer and winter temperatures after the inverse modeling? This reasoning is not clear to me. Is there no associated error in the transformation for summer and winter temperatures? This is why there are some odd values, for example, sample AXL60 MAT 12°C, ST 20.4°C and WT 10.0°C (only 1.2°C colder than the MAT).	Explain how MAT is estimated (summer-winter or original teeth average) and why summer and winter after modelling -	MAT was estimated from summer-winter unmodelled data to reduce errors, whereas summer and winter can only be deduced after modelling because seasonal amplitude is otherwise attenuated. To maximize data, in non-sinusoidal teeth profiles, MAT was deduced from teeth δ18O average, but it is less reliable. We detected that these explanations were not clearly explained, and we improved them in section 3.5.	Section 3.6		
R2-13	Yes	The reconstructed meteoric waters are different depending on the species, even in the same level, and therefore, reconstructed temperatures also differ. I'm aware of the different ecological behaviors of the different species, but the MAT should be, at least close. There are also some discrepancies in specimens of the same species in the same archaeological levels.	Reasons on differences in δ18O <sub>sw</sub> between species in the same level	We believe interspecific variability is not higher than intraspecific variability, and we argue multiple reasons that can explain this (ecological behaviour, physiological factors, levels as palimpsests) both in the reviewer response and within the manuscript in section 5.2.	Section 5.2 (lines 637-641)		
R2-14	Yes (justified)	The general comparison between the isotopic composition of the faunas of the archaeological sites of both areas is not objective since in the Vasco-Cantabrian area there are remains from 80 ka to 18 ka approx. (14 sections from 5 archaeological sites), but in the Catalanian area there is only one site at around 40 ka (it could have been covered to the HSA, a period especially cold and dry). Thus, the general comparisons that appeared in some parts of the manuscript between two areas (not the ones specific at ca. 40 ka) are not balanced.	Comparison between NW and NE samples is not balanced	We find this site interesting enough by the period it represents to be included, even if we agree with the reviewer	No changes required		
R2-15	Yes	What is the error associated with the different equations Eq. 1-9? Authors have to take into account the different errors (not only the standard deviations) that are being accumulated in each equation and also plot and mention them in the tables and in the text. For example, a MAT of 15°C +/- 0.5 °C would be accurate, but if the error was +/- 4°C the interpretation would be more open. This is even more important in this work since the oxygen isotopes were measured in carbonates, not in phosphates, and two more equations were used to convert the δ18O data of the carbonates to δ18O of phosphates. Authors said: lines 277-278: "In these estimations, the associated error from converting δ18O <sub>carb</sub> to MAT is enlarged by the uncertainty derived from the transformation of δ18O <sub>carb</sub> (VPDB) to δ18O <sub>phos</sub> (VSMOW)". Therefore, all the errors should be calculated (for both O and C), included in the plots, tables and when describing the results.	Provide error accumulated associated to equations	Associated errors were calculated following Pryor et al. (2014). Considering the advice provided by Federzani et al. (2021), it has been argued that the uncertain replications are associated with each conversion step. This is mentioned in section 3.4. Errors are included in Appendix B (B-Temperature-estimations) and mentioned in table 4 caption. Moreover, spreadsheets were provided in SI.	Section 3.5, Appendix B (B-Temperature-estimations), Table 4 caption, Spreadsheets in SI		
R2-16	Yes	Inverse Modeling. I think that the correct reference for the inverse modeling (main text and appendix D) is Passey et al. (2005) but this one "Passey, B.H., Cerling, T.E., Schuster, G.T., Robinson, T.F., Roeder, B.L., Kruiger, S.K., 2005. Inverse methods for estimating primary input signals from time-averaged isotope profiles. Geochim. Cosmochim. Acta 69, 4101-4116. Reviewing this paper, I noted that the reconstructed profiles showed mostly the same trends/changes as the original isotopic data. However, in the case of the reviewed manuscript sometimes these reconstructed profiles exhibited opposite patterns/trends from the original isotopic values. I'm not familiar with these kinds of transformations, but is this common? This is an extra transformation for the data that would be used to calculate absolute climatic parameters, and thus, and extra potential error source. Could authors double check these calculations? In addition, in the original paper from Passey et al. (2005), this inverse modeling was also applied to the carbon isotopes. Why authors did not apply this correction also to the carbon isotopes of the sequential sampling?	Doubts on modelling: 1) why some modelled teeth show opposite pattern from original; 2) why is not apply to δ13C profiles. Error in the reference.	We provide different reasons that can explain the editor's feeling about "opposite patterns" after modelling, derived from a lag in the x-axis respecting the original signal and non-sinusoidal profiles. The reference was corrected. The absence of seasonal change does not allow model application. Details in Section 3.6 and Appendix E.	Section 3.6 (lines 334-339) Appendix E (lines 1567-1569, 1587-1590)		

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R2-17	Yes	The first paragraph in the introduction and the first lines in the abstract deal with the importance of these kinds of studies to understand the human evolution in this region, but eventually, this is not discussed in the manuscript according to the obtained data.	The introduction explains the importance of the article for human evolution but this is not discussed	We mean the changes in human dynamics related to environmental conditions. We have clarified this in section 1	Section 1		
R2-18	Yes	First sentences of the introduction (lines 38-43): please add references. There are interesting papers dealing with this issue in the Iberian Peninsula and in Europe: Neanderthal-AMH-climate change.	Include references on Neanderthal-AMH-climate changes	References were added in section 1	Section 1 (lines 44-45, 52-53)		
R2-19	Yes	Lines 49-50. It is ok, but authors are not including some information (and significant climate-related references) in the area dealing with continuous paleoclimatic records, and they only focused on the data obtained from archeological sites (whose paleoenvironmental record is not that continuous); so, line 75 is not summarizing the multiproxy studies in this area.	Introduce information climatic related references	References were added in section 1 (link with comment R2-2)	Section 1 (lines 79-85)		
R2-20	Yes	Lines 80-87: add references	Add references	References were added in section 1	Section 1 (lines 90-91, 93-94)		
R2-21	yes, and now improved	Regarding the fossil sites that authors call "Mediterranean". Since the rest of the fossil sites are in "northeastern Iberia", the term "Mediterranean area" is very open and do not specifically identify the studied site. I would say NE Iberia?	Change Mediterranean by northeastern Iberia	We decided it to change this all along the text.	Appendix A. Different parts of the paper		We detected an error that sometimes prevented completion during the reviewer process. Now, it is corrected everywhere, including in Appendix A.
R2-22	Yes	There are some issues with the chronology/dates. For example, line 21 abstract: 80 to 15,000 cal BP. Do you mean 80 ka or ky. right? Taking into account the study period, and the accuracy of the dates, I would not use "yr". I would use ky or ka. In addition, authors should round the dates in the text to the nearest hundreds (eg. line 501: 41.136 to 38.570 cal yr BP: 41.1 ka to 38.6 ka). This accuracy does not make sense in the studied period due to the uncertainty of the measurements.	Correct dates format to ka/ky	All dates throughout the paper were revised and expressed in as ka BP or ka cal BP.	Different parts of the paper		
R2-23	yes, and now improved	Results please, add the ages (in ka) and the different technocomplexes to the subsection headings of the different archaeological sites, otherwise one has to check Fig. 2 for each site.	Include dates and technocomplexes from archaeological sites in subsection headings	We included dates in the mention headings (*) This review changed from the initial online response.	Headings from subsections 4.1 to 4.6	(*) Final dates are different from those indicated initially in online reviews due to the already explained change in calibration criteria (More details on R2-3)	In this last review, we noticed that it was suggested also to include technocomplexes. Now, it is implemented.
R2-24	Yes (justified)	I understand the structure of the result description, but the mixed description of the isotopes from different levels of the same archaeological sites, which sometimes have 10 ka of difference between them, is raw to me.	In results, different levels are explain together	We believe that this is the most efficient way, considering the specific characteristics of each individual site and baseline isotopic values.	No changes required		
R2-25	Yes	Line 325: MATAs=1.8-2.1°C? do you mean 1.8?	Error on MAT (line 325)	Corrected and all temperatures reviewed along the paper	Section 4	(*) All temperature estimations were greatly modified and derived from new correlation adjustments for temperature estimations	
R2-26	Yes	Eq 10: P value <?	Error on p-value	It was an error, but we finally Fernandez-Garcia et al. (2019) was removed from the text. (*) This review changed from the initial online response.	Subsection 3.5	(*) We propose to correct this in the initial answer, but the p-value was associated with a correlation no longer used in the text (more details in R2-11).	
R2-27	Yes	All figures with the data plotted according to the chronology. E1 Castillo 21A appears after Andor III, but according to the chronology this site is previous (Fig. 2). The same would happen between some levels of Labeko Koba and Carihars-I. Is this correct? In this case, arrange the sites with the real chronological order even though they belong to different archaeological sections.	Arrange the sites diachronically	We interpreted reviewer referred to Figure 5. We have rearranged the levels in chronological order. (*) This review changed from the initial online response.	Figure 5	(*) Initially, we considered was more easy to understand arranging the figure by sites, but we reconsidered this decision during reviews modifications in the manuscript and finally on organizing levels diachronically.	
R2-28	Yes	When speaking about range of values (temperatures, isotopes, precipitation), sometimes the lowest values are mentioned before and other times the other way around. Be consistent and cite the lowest values first (Eq. line 404 MATAs).	Put the lowest value first in data	Corrected and review along the paper	Sections 4 and 5		
R2-29	Yes	Table 3. I suppose that the different groups of data (rows) are related to the three groups of specimens. Right? Please, specify the taxon groups in the tables.	Taxa missing in table 3	Titles of the table were corrected	Table 3		
R2-30	Yes	I do not understand this sentence: Line 510-511: "Based on these arguments, it is suggested that the non-sinusoidal $\delta^{18}O$ signal observed in some individuals is likely attributed to the preservation of the original isotopic signature from water input." This sentence would suggest a bad preservation of the original water composition when a sinusoidal pattern is present? However, authors explained afterwards some reasons related to the ecology of the specimens. Please, clarify this.	You suggested that sinusoidal profiles indicate bad preservation or ethological factors?	These non-sinusoidal profiles do not indicate poor preservation and may be linked to the individuals' ethological factors (more details on R2-10 and R2-13). We clarified this in the text.	Subsection 5.2 and lines 607-620		
R2-31	yes, and now improved	Section 5.4. Regarding the $\delta^{18}O$ of meteoric waters. Authors compare (as a whole) their reconstructed $\delta^{18}O_{mw}$ with the current values in the area and they ended up with similar ranges. Keep in mind that the reconstructed values correspond to the last 80 ka under glacial conditions: different temperatures, precipitation amount, and in some cases, moisture source	When comparing $\delta^{18}O_{mw}$ with current values consider all factors that can be different in the past	We agree with this consideration and we were less determinant in our explanation.	Subsection 5.4 (first paragraph)		We have included an extra explanation on this point
R2-32	Yes	Sometimes the different $\delta^{13}C$ and $\delta^{18}O$ have subscripts with the meaning of the isotopic values (eg. $\delta^{13}C_{carb}$ , $\delta^{18}O_{carb}$ ), but another times this is not indicated. This is confusing. Please, add always the subscripts explaining the meaning of the isotopic values.	Add always subscripts to $\delta^{13}C$ and $\delta^{18}O$	We corrected the text to ensure that " $\delta^{13}C$ " and " $\delta^{18}O$ " always include subscripts, except when referred to general explanations.	Different parts of the paper		
R2-33	No implemented	I miss a figure summarizing the chronological evolution of reconstructed temperatures and precipitation (with the associated accumulated errors) and comparing them with other continuous records of precipitation or temperatures or vegetation from the Iberian Peninsula, Mediterranean coast or Iberian margin.	Include a figure summarizing temperatures and precipitations estimations	We chose not to include it because the estimation of paleotemperatures was approached tentatively, and we preferred not to focus on this discussion in this paper	None		
R2-34	No implemented	Title: ecological evolution of what? This is very general: I would say ecological evolution of ungulate fauna....	Change the title	We agreed with the title initially in the online reviews, but after discussing it with coauthors we considered it not the most appropriated	None		We provide a new proposal in this reviewed version
R2-35	Yes	Please, use always the symbol delta instead of d, there are some "ds" throughout the manuscript.	Use delta symbol (not "d")	Corrected	Different parts of the paper		