

Clim. Past Discuss., referee comment RC2
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Comment on cp-2021-3

Anonymous Referee #2

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General comments

This manuscript combines multi-proxy analyses ($\delta^{13}\text{C}$, $\delta^{44}\text{Ca}$, paired U-Th and ^{14}C ages) and geochemical modelling of vegetation-soil respiration within the soil-karst-cave system in the northern Iberian Peninsula over the last glacial-to-interglacial transition (ca. from 26 ka to 4 ka).

Authors have irrefutable knowledge on the subject and are familiar with the region, data and tools applied. They have performed comprehensive analytical work on three speleothems from two caves [Candela, El Pindal Cave (Moreno et al., 2010; Rudzka et al., 2011; Stoll et al., 2013; this study); Laura, El Pindal Cave (this study); Galia, La Vallina Cave (Stoll et al., 2013; this study)], including three pieces of the overlying bedrock.

The focus is on the $\delta^{13}\text{C}_{\text{speleo}}$ signal (Fohlmeister et al., 2020) to decipher whether it can be a possible paleo-soil respiration proxy ($p\text{CO}_2$). CaveCalc (Owen et al., 2018) and ISTAL (Stoll et al., 2012) are used to account for effects of prior calcite precipitation (PCP), mean soil carbon age - dead carbon fraction (DCF), karst hydrology, bedrock dissolution, seepage zone and drip interval length changes. The referential temperature pattern is taken from Iberian Margin marine sediments (Darfeuille et al., 2016), the radiometric chrono-stratigraphy of which is sufficiently robust, and multiproxy studies have been performed on its strata.

Results point to increasing soil $p\text{CO}_2$ over the last deglaciation (from late glacial ca. 530-1030 ppmv to ca. early interglacial 1155-5780 ppmv) heavily dependent on temperature ($Q_{10} \sim 2.7-7$; i.e., a factor by which soil respiration increases with a 10°C rise in temperature). This is in line with previously documented changes in vegetation cover and substrate from open glacial grassland, steppe taxa and low arboreal percentages to interglacial high arboreal pollen (data compilations in Fletcher et al., 2010 and Moreno et al., 2014; simulations in Scheff et al., 2017). Authors present and discuss other possible processes which are not found to exert a huge impact on the $\delta^{13}\text{C}_{\text{speleo}}$ signal. Their results, interpretations and conclusions are justified by data and are consistent with previous monitoring data that showed seasonal variations in cave $p\text{CO}_2$ driven by external temperature variations (Moreno et al., 2010; Stoll et al., 2012).

The science of the manuscript is excellent (significance and quality) and the overall presentation well structured. The organisation and length of the manuscript are good: 1 Table and 6 Figures in main text and 4 appropriate supplementary Figures (see below for specific comments). The title clearly reflects the contents of the paper and the abstract provides a concise and complete summary. The subject addresses relevant scientific questions within the scope of CP. My opinion is that it merits publication with minor changes once few clarifications are added. Please see below for constructive suggestions.

Specific comments and technical corrections

Tables

Suppl. Table 1, Suppl. Table 2

I was unable to find the 2 supplementary Tables mentioned in the text.

Figures

Current Figure 1 (Speleothem $\delta^{13}\text{C}$ records covering the last deglaciation in temperate Western Europe)

- Remove lines when 'hiatus' or 'no data' in panels A, C
- Change "El Pindal (study site)" to "El Pindal & La Vallina (this study)" in legend of panel A
- Complete figure caption. Something like: "El Pindal Cave – stalagmite Candela (Moreno et al., 2010, this study), – stalagmite Laura (this study) and La Vallina Cave – stalagmite Galia (Stoll et al., 2013; this study)."
- $\delta^{13}\text{C}$ Villars (Genty et al., 2006; Wainer et al., 2011) does not appear to be consistent with El Pindal ca. 19 ka. Issue with resolution of the former? Any comment?
- $\delta^{13}\text{C}$ Buraca Gloriosa (Denniston et al., 2018) appears opposite to El Pindal (Moreno et al., 2010) ca. 13 ka and around 19 ka. Dating issues? Other reasons?

- $\delta^{13}\text{C}$ Cova da Arcoia (Railsback et al., 2011, PPP 305) is not included. It seems to have quite different absolute values during the time span Galia grows (ca. 9 ka). Any comment? Could this be relevant to the comparison with temperatures derived from the marine record located further south? Atlantic versus Mediterranean climatic zones? (see for instance Fig. 1 in Denniston et al., 2018)

- In this regard, what about adding $\delta^{13}\text{C}$ La Mine (Genty et al., 2006) as a contrasting environment?

This is important to highlight the "regional" extent of the exercise submitted in the present study.

Additional Figure (new Figure 1? Current numbers would change accordingly up to 7 Figures)

A non-specialist reader would very much appreciate being able to recognise all the variables measured and modelled under discussion. Thus, I earnestly request that authors include an illustrative scheme with the processes and reactions in question. As far as possible, the text must be self-explanatory: labelling the parameters as in the Figures, i.e. $\delta^{13}\text{C}$, $p\text{CO}_2$, specifying sources and including the notion of dead carbon (modern-to-fossil reservoir effect) so the reader can follow the reasoning step by step: (i) atmospheric CO_2 and rainwater (highlight seasonal effects in temperature and rainfall density/amount); (ii) biogenic CO_2 from vegetation-plant litter-microbial activity-soil respiration (emphasize role of moisture availability, vegetation type and cover in soil gas $p\text{CO}_2$); (iii) infiltration through soil water to karst and water flow paths, bedrock dissolution, drip water, CO_2 degassing-calcite precipitation to form the speleothem (link to cave air $p\text{CO}_2$, cave ventilation dynamics, etc). Perhaps two panels are needed: one for a 'summer' scenario (assimilated to interglacial situation?) versus a 'winter' one (for the glacial conditions?). Ideally, this must lead the reader through the diverse situations deduced from the results, without digging too much in dispersed literature, which is somewhat scarce for $\delta^{13}\text{C}$ specifically (indeed this is a strong point of the present manuscript value). Consider adding the seasonality of the caves in question with the series of instrumental measures available for the area (see below for additional comments on that).

Figure 3

- Complete figure caption to highlight the paired U-Th and 14C ages shown at the bottom of the figure. Refer to the Suppl. Table 2 (if it exists?)

Figure 6

This figure seems too compacted. Try to uniform the criteria for all Figures, so the period of interest (from 26 ka to 4 ka?) and the relevant events of the study are clear.

Main text

References are made to the text by giving [line numbers: "text quotes"].

[Line 15 "underwent dramatic climatic and environmental change"]

Please remove "dramatic". If qualifying the change is needed, any alternatives? "profound" is used for the Introduction, what about "significant" here?

[Lines 17-19 "global carbon cycle" ... "on local soil respiration"]

My recommendation is that neither the word "global" nor the point to "local" fit in here or at least may add confusion. The present work may have "regional" application (and unvaluable as such!) for similar temperate environments of Western Europe when results are properly reproduced in subsequent studies.

[Lines 21, 73, 88, 92, 325, 336, 337, 349 ... "Northern Spain", "NW Spain", "northern Spain" ...]

Check for consistency and consider changing to geological terms such as "NW Iberian Peninsula".

[Lines 34-35 "Between 22 and 10 ka BP (ka: thousands of years, BP: "before present", with the present referring to 1950 CE),"

[Lines 103-104 "Minimum average temperatures are reconstructed for Heinrich event 1 (H1; 18-15 ka BP) and are ~8°C cooler than those of the Holocene Thermal Maximum (~8 ka BP; Darfeuil et al., 2016)."]

[Lines 246, 252, 256, 260, 298, 301, 333, 427 ... "LGM (26.8 ka BP)" "the LGM (24 ka BP)" "(LGM, H1, and YD)" "during the LGM and YD" "~530-1030 ppmv during the LGM, and ~1155-5780 ppmv during the EH"]

[Line 316 "for the Early Holocene (EH, post 10 ka BP) and the Late Glacial (LG, pre 10 ka BP and including deglacial)"]

These excerpts use terms and chronostratigraphic units that must be clarified.

For instance, "last glacial maximum" (LGM) is used, though I am afraid I do not find the complete acronym meaning anywhere in the manuscript. In any case, both characterisation and timing of the LGM are complex enough for including the term here (see different approaches and stratigraphy ranging from ca. 33 ka to 26.5 or 23 ka to 19 ka, depending on literature e.g., Peltier & Fairbanks, 2006, QUAT. SCI. REV. 25; Clark et al., 2009, SCIENCE 325; Batchelor et al., 2019, NATURE COMM. 10; Gowan et al., 2021, NATURE COMM. 12; and references therein). Decoupling between temperatures and ice volume is specifically pronounced during deglaciations. Temperature estimations at the Iberian Margin suggest that the LGM was not a real stadial but a kind of weak interstadial. Although undoubtedly cold, it was not the coldest interval. The coldest intervals are observed during Heinrich events. Following the reference used in the manuscript (Lambeck et al., 2014), the main phase of deglaciation occurred from ca. 16.5 ka to 8.2

ka. My advice would be to delete any reference to “the LGM” and stick to two phases Late Glacial (LG) and Early Holocene (EH). Similarly, avoid the reference to a “Holocene Thermal Maximum”, which is an even more diffuse designation. The “Holocene temperature conundrum” debate will likely remain highly contentious over many years to come (Liu et al., 2014, PNAS 111; Bader et al., 2020, NATURE COMM. 11; Martin et al., 2020, QUAT. SCI. REV. 228; and references therein).

Additionally, the base of the Holocene must be placed ca. 11.7 ka, not 10 ka (Walker et al., 2009, J. QUAT. SCI. 24) and the EH spans from 11.7 ka to 8.2 ka (Greenlandian; Walker et al., 2019, J. QUAT. SCI. 34), though technically speaking the present study shows results up to 4 ka in Fig. 3, i.e. the Mid-Holocene (Northgrippian; Walker et al., 2019, J. QUAT. SCI. 34). This does not alter the results of the manuscript but respects the formal definition and dating established, in line with the useful INTIMATE event stratigraphy of Greenland interstadials and stadials (GI and GS, respectively; Lowe et al., 2008, QUAT. SCI. REV. 27; Rasmussen et al., 2014, QUAT. SCI. REV. 106; Mojtabavi et al., 2020, CP 16, 2359). For the LG events, please consider this nomenclature (i.e., use GS-1, not YD; and GS-2.1a, not H1), which implies showing a Greenland d18O profile in the Figures where these intervals are discussed. These are aspects of relevance to the subject because, the manuscript works on and paves the way to well dated speleothem material, with chronologies specifically reviewed within the SISAL database, version 2 (Comas-Bru et al., 2020a,b).

[Lines 96-99 “(AEMET meteorological stations at Santander and Oviedo, period 1973-2010; AEMET, 2020)” “(AEMET meteorological station at Santander, period 1987-2000; AEMET, 2020)”]

[Lines 469-470 “AEMET, 2020. State Meteorological Agency (AEMET) [WWW Document]. URL <http://www.aemet.es/en/portada> (accessed 470 10.8.20)”]

Not sure I understand the data source used here. Are the time intervals 1973-2010, 1987-2000 chosen for a particular reason? Is there a gap between 2010 and 2020? Can the series be shown for instance in the new Figure? Something that illustrates the seasonality in the region and explains more clearly the assumptions for the parameters involved in the present study (cave-monitored CO₂, d13C, etc).

[Lines 98, 101-102, 106-107, 197-198, 407-409 “winter months (December-February)”, “summer months (June-September)” “estimate of the deglacial temperature change in caves on the coastal plain, as the region’s modern seasonal cycle displays similar

amplitude to sea surface temperatures (Stoll et al., 2015).” “caves are well ventilated in the cold season with close to atmospheric pCO₂ values, but feature elevated CO₂ concentrations during the warm summer season (Stoll et al., 2012).” · “Cave monitoring data from winter months (December-March) were excluded from the regression analysis”; “Two model scenarios mimic full glacial and Holocene conditions, including changes in temperature, cave pCO₂, and soil pCO₂ for “winter” (i.e., atmospheric) and “summer” (i.e., elevated) cave pCO₂ (Suppl. Fig. 4).”]]

Please correct “mimick” to ‘mimic’; or better still, change the word to “simulate”?

Seasonal changes, both in CO₂ and temperature, appear crucial for interpretation of the results. Please clarify as much as possible throughout the manuscript. This would improve if illustrated with the new Figure. The reader would appreciate a clearly understandable and comprehensive discussion on that. For calibration purposes, I wonder if databases considering non global atmospheric CO₂ values but continuous seasonal CO₂ measurements from the ground-based network ICOS may be of some assistance here (Integrated Carbon Observation System, ICOS; Ramonet et al., 2020, Phil. Trans. R. Soc. B 375). Any comments?

[Lines 103-104, 343 “Heinrich event 1 (H1; 18-15 ka BP) and are ~8°C cooler than those of the Holocene Thermal Maximum (~8 ka BP; Darfeuil et al., 2016).” “Assuming a temperature change of roughly 8°C between the LGM and EH (Darfeuil et al., 2016)”]

Please clarify. It seems the 8°C value accounts for the increase of temperatures between GS-2.1a (H1; ca. 18-15 ka) and the EH (before 8.2 ka). Other alternatives, i.e., from LGM to values after 8.2 ka seem closer to 6°C, though perhaps I am missing something here. I understand the selection criteria of the site used as a reference for temperature (Iberian Margin site MD95-2042; Darfeuil et al., 2016) is based on its chrono-stratigraphy? I’d suggest authors also highlight the fact that multiproxy studies have been performed on its strata. In Darfeuil et al., 2016, two complementary paleo-thermometers are discussed, the TEX86 and Uk’37 (annual mean sea surface temperatures, a potential shift towards summer production that may occur for glacial times?). Authors refer to the former only and the profile is shown in Fig 3. Any comment here considering seasonality? Please include considerations of the analytical and calibration errors of the estimates. What about alternative documentation provided by pollen transfer functions? Perhaps it would be preferable to have a sediment core further north, closer to the caves, though to my knowledge this is not available.

[Line 129 "Stoll et al., in review"].

If the paper is not publicly available at the time the present manuscript is published, I would suggest that the authors remove the reference in review and point to a different reference already peer-reviewed or add the information in this study.

[Lines 142.145 "Reimer, 2013"; "Reimer et al., 2013"]

It may be advisable to work on the updated calibration curves, i.e. IntCal20 and Marine20; Reimer et al., 2020, Radiocarbon, 62; Heaton et al., 2020a,b, Radiocarbon, 62. For Marine20, marine reservoir ages are modelled as time-varying, though for IntCal20, speleothem dead carbon fractions are approximately constant over time but with an unknown level. Any comment here?

[Line 360 "vegetation cove"]

Change to "vegetation cover".

[Line 433 " $\delta^{13}\text{C}_{\text{spel}}$ over the last deglaciation in Western Europe is best explained by c"]

Please complete the sentence.