Response to anonymous referee #1

This paper presents a useful modeling approach for reconstructing soil respiration using stalagmite carbon isotopes and proxy constraints on prior calcite precipitation and bedrock dissolution effects. Carbon isotopes have been a particularly messy avenue in speleothem science due to the complex interplay of these effects (and others) and the paper represents an exciting effort toward rigorously disentangling this mess. The model is an important step toward understanding how soil respiration changes with climate and, given the breadth of data available in the SISAL database, could be quickly applied on a large scale (assuming it can be appropriately constrained). But the modeling approach carries some critical (and likely invalid) assumptions that need to be addressed. **This paper can be of sufficient interest for Climate of the Past and I think my points can be addressed with major revisions**. I commend the authors for their coupled proxy-model approach and hope my feedback is useful as they refine their work.

My expertise most closely aligns with the modeling work, so I focus my feedback on this part of the manuscript. I can't speak much to the analytical methods. Below, I've divided my feedback into points about the main modeling approach (outlining my own confusion) and line-by-line items. My biggest concerns are that the modern calibration of the soil respired end member seems invalid (or at least should use modern CO2 levels), and that it is not a safe assumption that the mixing end members are time-invariant.

Response: We thank the reviewer for their thorough and fair assessment of our manuscript. We address the comments in detail below.

Main modeling points

My understanding of the modeling approach involves three steps: (1) use modern data to derive a relationship between atmospheric CO2 and soil-respired end members; (2) assuming this mixing relationship holds through time prescribe the full range of soil CO2 and $\delta^{13}C$ possibilities to solve for proxy data with CaveCalc; (3) using the model output, select and analyze the combination of input parameters that yield results closely in line with the measured data. Below, I dive into my concerns on steps 1 and 2 in more detail and include one note on step 3.

Step 1: calibrating a soil respiration end member

This calibration exercise (outlined in Figure 2) carries four big assumptions that I think need to be addressed. The biggest has to do with using modern data to calibrate a pre-industrial mixing curve (see point 2).

1. First, cave-monitored CO2 and $\delta^{13}C$ are used to calibrate a soil CO2 mixing line. This assumes that the mixing of atmosphere and respired CO2 in the cave falls the exact functional form of mixing in the soil (put otherwise, it assumes that the bedrock contribution to cave carbon is the same as soil carbon and that there are no other carbon fluxes distinguishing cave from soil). The authors concede this does not hold true in the winter, but could more be said about this assumption in the months of April-November (when (I think) the monitoring data are used)? Could changes in hydrology, or cave vs soil temperatures, or other things violate this assumption? Is soil CO2 assumed to reflect the soil-column integrated conditions? Is it a problem that this assumption breaks down seasonally if calcite deposition is seasonally biased? **I think, at the least, it must be written that this assumption is made** (right now the link between cave conditions and soil conditions is a bit vague to me).

Response: We agree that the assumptions made when calibrating the soil gas end member would benefit from clarification in the text. Here we summarize our reasoning on using cave air measurements to constrain soil gas and estimate the respired end member. We now show the actual monitored data with modern atmospheric composition and post industrial mixing line, as well as our suggested pre-industrial mixing line corrected for the Suess effect. We will add relevant clarifications to the manuscript.

Estimation of soil respired end member from cave CO_2 measurements: In the absence of a full soil monitoring campaign, samples collected from the cave in summer months represent a reasonable approach to estimate the isotopic value of the respired end member contributing to soils/epikarst (Figure 1). This is because the cave, like the soil, is defined by a two-end member mixing system, which is driven by the physical ventilation of the cave. The main fluxes of carbon in a system like El Pindal and La Vallina caves are from soil gas (mainly seeping through the host rock and into the cave) and atmospheric air (through ventilation). Like many mid-and high latitude cave systems, there is a seasonal reversal in the airflow direction in La Vallina cave (Stoll et al., 2012). In the summer, when cave air is colder than exterior air, cave air flows out the entrance, and is replaced by inflow and diffusion of soil/epikarst air. In this season, the cave has the highest CO_2 concentrations and points which fall closer to the soil respired end member on the Keeling plot. In the winter, when cave air is warmer than exterior air, exterior air flows in through the cave entrance, bringing the cave closer to the atmospheric end member.

The data from the monitoring of the cave reflects primarily CO_2 from the soil that is drawn through the karst network into the cave. It is therefore likely reflecting soil column-integrated conditions and the full contribution of respired CO_2 in the soil and epikarst unsaturated zone (below the soil, "ground air").

Any contribution of C flux from bedrock dissolution does not significantly affect our estimation of the respired end member, because the intercept defining the respired end member is most influenced by the summer season cave pCO_2 data. During the summer season drip flow rates are more than an order of magnitude lower than in the winter and degassing from this drip is suppressed by the high cave pCO_2. In winter, when drip rates are higher, and cave pCO_2 is lower, then degassing may contribute to C in cave air, as seen in other systems (Waring et al., 2017). However, winter data corresponding to ventilated periods are near to the atmospheric composition, and we explicitly define our atmospheric end member from global measurements, not the local cave measurements. Hence, the ventilated season measurements, which in theory may have contribution from degassing of dissolved limestone, would have an insignificant impact on our calculated mixing line. Furthermore we do not find evidence for a different intercept in winter and summer end members, unlike monitoring studies which infer a strong effect of degassing of a carbon source from limestone dissolution (Waring et al., 2017). (We also note that contribution of C to stalagmites from bedrock dissolution is explicitly accounted for by modeling of the dead carbon fraction, DCF, in stalagmite data in CaveCalc.)

We therefore argue that summer cave air can be used to estimate the isotopic composition of the respired end member of soil CO_2, when the competing fluxes are minimized: ventilation is reduced during the summer months (Stoll et al., 2012) and the higher cave pCO_2 levels reduce the amount of degassing that can occur from dripwaters entering the cave (reducing the contribution from host rock carbon).

An estimation of the modern respired end member, defined along a mixing line which includes the modern global atmospheric end member, is -26.9 +/- 0.8 ‰.
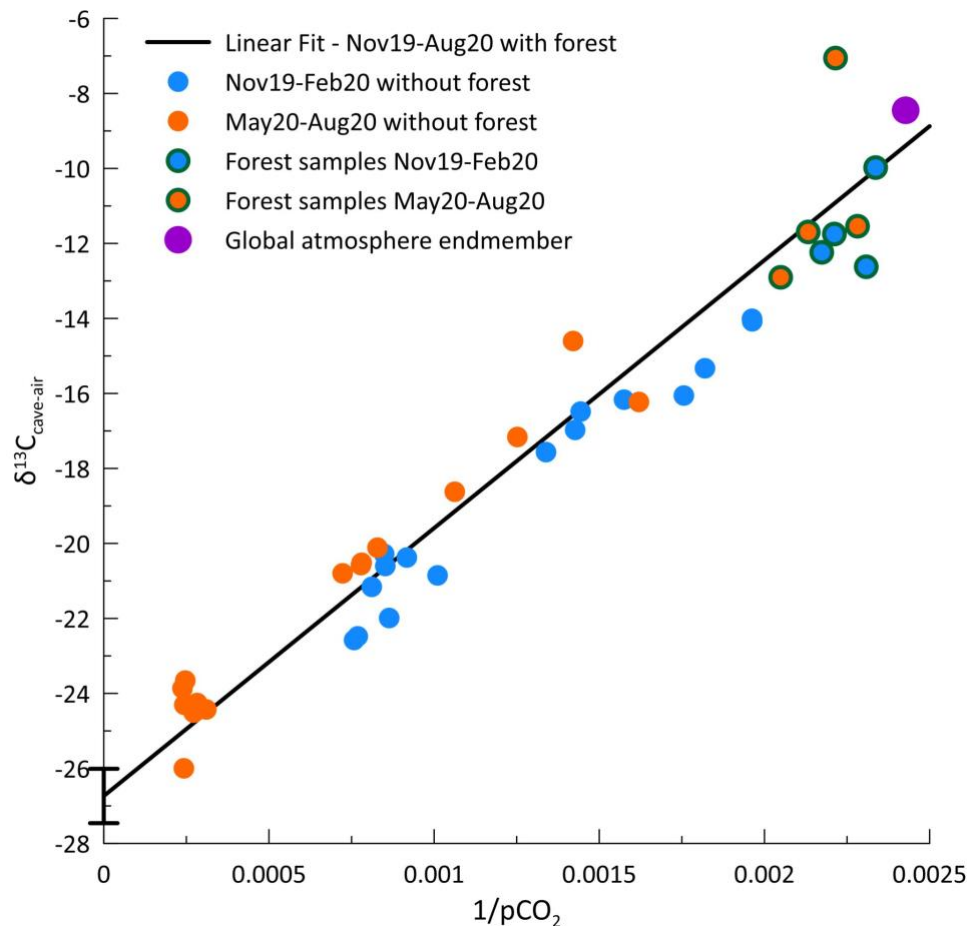
Figure 1: Keeling plot of cave monitoring data from La Vallina Cave. The regression was calculated using the full dataset and the global atmosphere (purple dot). No clear seasonal bias in sample composition stands out.

Seasonally biased calcite deposition
The seasonality of the modern cave air has the advantage of helping to define the modern respired end member. From our monitoring, we do not find evidence for a different isotopic value of the respired end member in different seasons. Thus, exploiting the seasonal ventilation of the cave to define the mixing line and respired end member does not preclude using this respired end member to interpret records from speleothems in which deposition is dominant in one season. Monthly monitoring of drip rate, dripwater chemistry, and cave air composition indicates that currently in the cave, some stalagmites grow at similar rates throughout the year, whereas in other sectors of the cave, growth occurs exclusively in winter, driven by cave ventilation.
We will discuss this in the next version of the manuscript.

2. Second, the model is calibrated to pre-industrial CO2 levels even though the data are taken in modern conditions (when CO2 is very well-constrained and much higher!) I think this is done so the same calibration end members can be applied throughout the Holocene (more on this in the "Step 2" section). The forest data are ignored because they might be influenced by "turbulence and advection effects" (Line 196),ut they would probably fit really nicely on a mixing line that reaches to a modern CO2 end member (with higher CO2)! In fact, all of the data would likely fit better on such a line (given that the monitoring data residuals to mixing line 1 are mostly above the line when CO2 > 1,000 ppm). This makes sense, because the monitored data are mixing with the modern atmosphere, not the pre-industrial

atmosphere. (Correcting for the Suess effect only corrects for pre-industrial $\delta^{13}C$, it does not account for the difference in CO2 between then and now). **I highly encourage the authors to use a calibration to a modern end member**. Otherwise a much more rigorous justification for the pre-industrial end member is needed.

Response: As suggested by the reviewer, we agree that it is useful to illustrate the full derivation of the modern, post-industrial respired end member also accounting for the modern atmospheric contribution, as a first step (Figure 1). As described in our response to point 1, including the modern atmospheric end member leads to an estimated respired end member of -26.9 ‰. This end member may be more negative than the preindustrial end member (which characterized the Early and mid Holocene growth periods of the stalagmites in this study), because atmospheric $\delta^{13}C$ has decreased by 2 ‰ over the last century due to anthropogenic activities (Suess effect). If the currently respired end member is composed of modern respiration and respiration of young (decadal age) soil carbon pools, the pre-industrial respired end member may have been as much as 2 ‰ heavier, that is closer to -25 ‰. If a significant fraction of the respired pool is older, then the preindustrial respired end member may fall between -27 and -25 ‰. We favour the less negative estimate because a young age of respired carbon is suggested by the rapid post-bomb spike decrease in ^{14}C in actively growing stalagmites in the cave.

Therefore, we define an updated reference mixing line for the Early Holocene as having a respired end member of -25 ‰ and an atmospheric end member of 260 ppmv and -6.3 ‰, consistent with ice core records. We use a pre-industrial atmospheric composition in the model since this more closely reflects the end member at the time of stalagmite growth.

3. Third, I don't know how the soil-respired end member is defined as 7800ppm and -22.9‰. **I imagine that it's an extrapolation of mixing line 1, but why not extrapolate some number other than 7800?** Is there some assumption that I'm missing?

Response: We have clarified that we define the isotopic value of the respired end member on the basis of Keeling plots, however by definition the Keeling intercept is not associated with a particular soil CO_2 concentration. We clarify that 7800 ppm CO_2 (now updated to 8000 ppmv, see step 2) is the soil concentration which best simulates the observed speleothem growth rates and isotopic ratios and we note it is consistent with soil pCO_2 above other caves in comparable settings (e.g. Borsato et al., 2015).

4. Fourth, **I think this modeling approach assumes that boundary layer CO2 concentration and $\delta^{13}C$ (the stuff that diffuses into the soil) also falls on the same mixing curve**. This should be stated since at least two things relevant to this study might violate this assumption. First, a shift from no canopy in the last glacial to a canopy when forests appear might lead to a "canopy effect" whereby $\delta^{13}C$ gets lower than expected for a given pCO2 due to recycling. Second, "turbulence and advection effects" (line 196) that appear to matter during the daytime (probably when photosynthesis is happening) can overprint the simple mixing relationship and propagate down to the soil respired end member. These effects might well be small, but I think the assumption should at least be recognized.

Response: This is a good point, and we will add this to the discussion of the uncertainties in the model.

Step 2: assuming this holds through time
This analysis assumes that the mixing slope between soil respired $\delta^{13}C$ and atmospheric $\delta^{13}C$ is constant through time. This is not a good assumption because a lot of factors that matter on decadal or longer timescales (i.e. factors that are not captured by the short calibration) violate it by changing end member CO2 but not $\delta^{13}C$ (or vice versa). For example, if CO2 increases (like it did from LG to EH), then the isotopic composition of CO2 must decrease to keep the end member on the curve, but we know very well that this assumption is violated on paleoclimate timescales (e.g. Schmitt et al., 2012; Science; Figure 1). Similarly, if soil respiration decreases (thus decreasing soil-respired CO2) then the $\delta^{13}C$ of soil respiration must increase to stay on the mixing curve. I'm not sure if there's a defensible mechanism for this, although it might occur by coincidence if water stress increases vegetation $\delta^{13}C$ (thus soil-respired $\delta^{13}C$) while decreasing soil respiration. Either way, I am not aware of any mechanistic reason why the end members of the mixing relationship should, themselves, vary along a mixing curve. The end-members, just like the average soil CO2 values that reflect their mixing, should vary over time.

Response: Two factors may contribute to variation in the mixing line over time. The first and most certain factor is due to changes in the concentration of atmospheric CO_2. The isotopic composition of the atmospheric CO_2 remains within a few tenths of a permil of the Holocene value (Schmitt et al., 2012). Therefore, assuming a constant respired end member, the slope of the mixing line is reduced during periods of lower atmospheric pCO_2 (Figure 2). As a consequence, for a given soil pCO_2 value, the isotopic composition of soil pCO_2 is more negative during the glacial than during the interglacial. This effect is very small (<0.4 ‰) for soil pCO_2 4000 ppm or higher, but for soil ppmv of 2000 ppm corresponds to a 0.9 ‰ difference and at 1000 ppm corresponds to a -1.8 ppmv difference. Nonetheless, this is still a small effect compared to the range of $\delta^{13}C$ resulting from changes in the soil pCO_2. If anything, this change in the mixing line would attenuate the difference in $\delta^{13}C$ between a low CO_2 glacial soil and a high CO_2 interglacial soil.
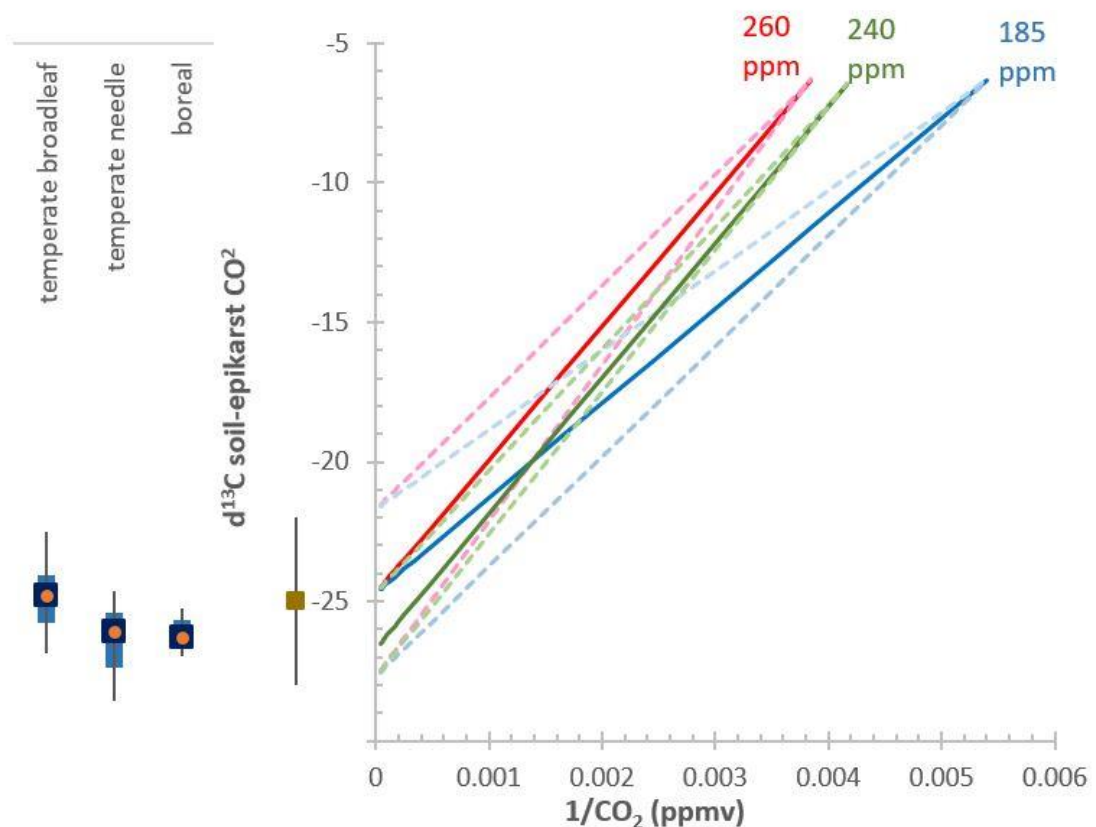
Figure 2: Left panel: Mean respired $\delta^{13}C$ of soil CO_2 across different biomes, adapted from Pataki et al., 2003. The respired end member used in this study is shown by the brown square (with uncertainty of +/- 3 ‰). Right panel: Mixing lines for soil gas illustrating expected changes in the atmospheric composition over glacial-interglacial transitions. While the isotopic composition remains virtually the same, the pCO_2 of the atmosphere varies more strongly, resulting in changes in the slope of the mixing line.

The mixing line may also vary if the respired end member changes. However, there are few constraints on potential changes in the end member. Although there is variation in the respired end member both within and among biomes, the mean respired end member for the potential biomes which may have characterized this site over the last 25 ka - temperate broadleaf, temperate conifer, and boreal - feature mean $\delta^{13}C$ of respired end members which differ by only 1 ‰ (Figure 2; Pataki et al., 2003). This suggests that we cannot predict a systematic change in the $\delta^{13}C$ of the respired end member with changes in the biome. Moreover, the fact that speleothems across Western Europe show very similar trends in $\delta^{13}C$ over the deglaciation also indicates that highly localised factors that may lead to a strong change in respired $\delta^{13}C$ without a biome change are unlikely. Consequently, we address the potential for variation in the respired end member by completing a sensitivity analysis of mixing lines which encompass 3 ‰ heavier and lighter respired end members.

I think the modeling can still be performed if some significant changes are made (these are just suggestions and other options can be valid too!).
1. Consider using the actual paleoclimate constraints on pCO2 and $\delta^{13}C$ of CO2 to parameterize the atmospheric end member.
Response: We have now updated the atmospheric end member for multiple time windows including the EH, LGM, and one period during the deglaciation, in accordance with Schmitt et al. (2012). These do not change the conclusions of the study because

they lie largely within the sensitivity range of the different mixing lines and are therefore taken into account (Figure 3).
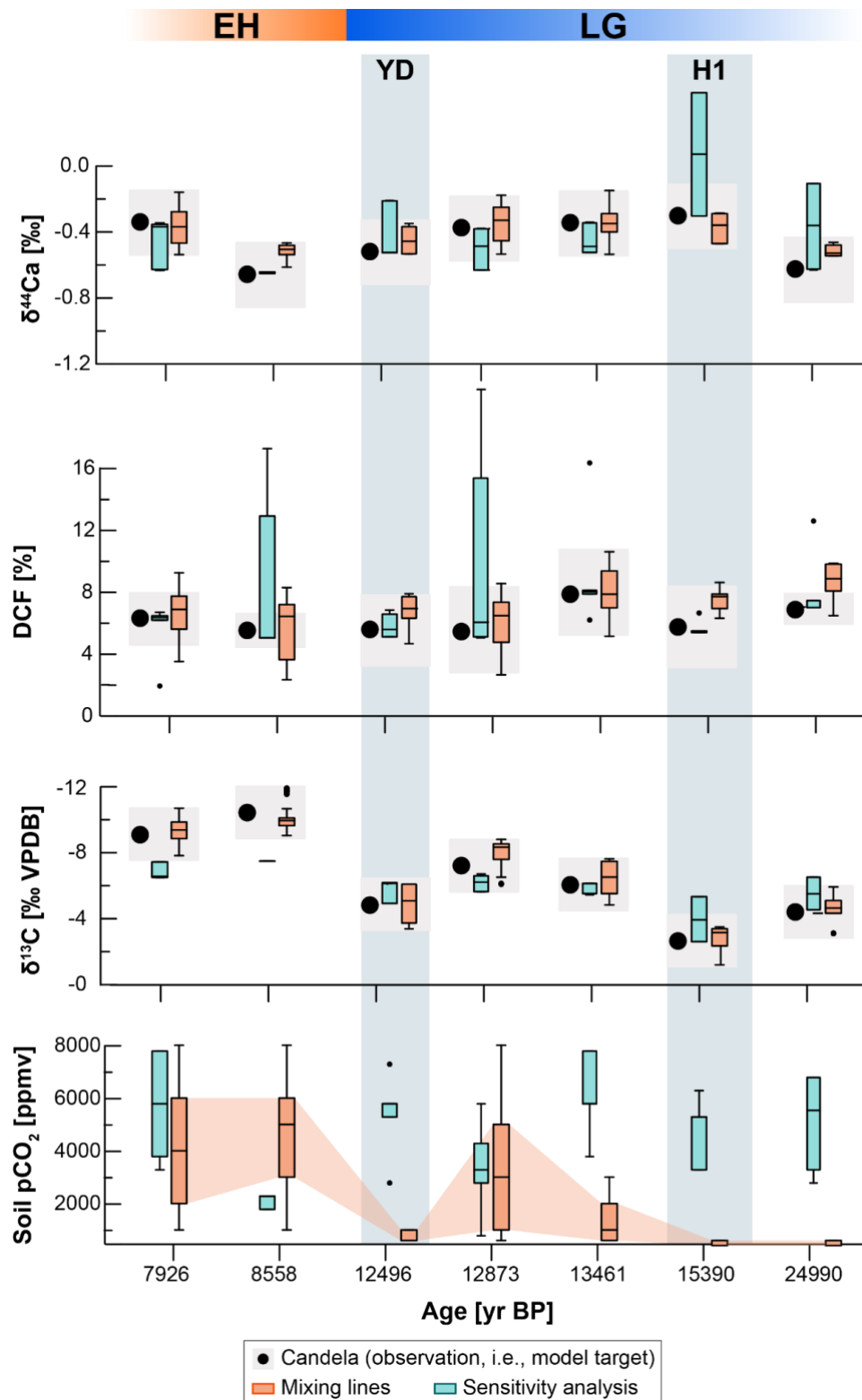

Figure 3: Updated modelling results for stalagmite Candela using newly calculated mixing lines that consider changes in the atmospheric end member (EH, deglacial, and LGM), and in the respired end member δ13C. All mixing lines were grouped together and compared to the sensitivity analysis keeping soil gas δ13C constant.
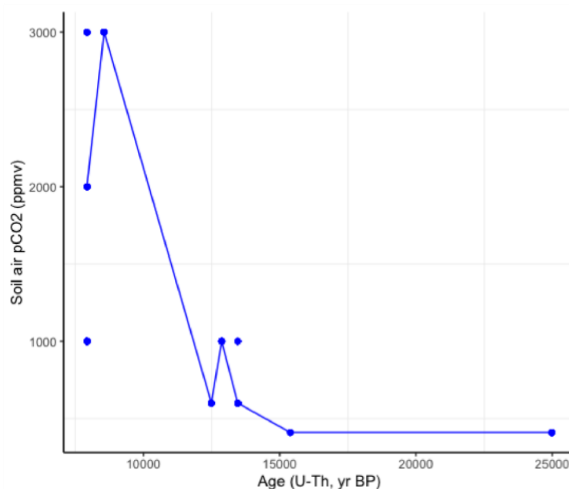
2. Instead of calibrating the soil-respired end member with modern data, be clear that constraints on this term are not great but define reasonable ranges and run sensitivity tests. Allow the soil respired $\delta^{13}C$ and CO2 to vary with time. Or consider forcing the model with different scenarios as a sensitivity test (i.e. low vegetation $\delta^{13}C$, high vegetation $\delta^{13}C$ and variable, or decreasing, or increasing $\delta^{13}C$). Given the strong evidence for substantial changes in vegetation, it is helpful (maybe necessary) to rule this out as the main factor affecting $\delta^{13}C$_spel. Consider holding soil respiration constant while letting $\delta^{13}C$ -respired vary; one might find that the variability would have to be too high to be explained by changes in C3 vegetation or water stress alone (see Kohn, 2010; PNAS).

Response: We have explored a wide range of sensitivity in the composition of the respired end member, which we infer to be the suggestion of the reviewer (holding soil respiration constant while letting $\delta^{13}C$-respired vary) since the mixing line with the atmosphere is an ubiquitous feature of soil-epikarst system.

To test the sensitivity of the model to changes in the soil pCO_2, we have performed two additional analyses:
1) We have extended the new mixing line 1 (respired $\delta^{13}C$ -25 ‰) up to pCO_2 of 15,000 ppmv (Figure 3). This allows us to test how the system reacts to coupled changes in pCO_2 and $\delta^{13}C$ of the soil gas. Since at high pCO_2, the soil gas $\delta^{13}C$ becomes insensitive to changes (hyperbole), this extrapolation mostly affects the pCO_2 of the initial solution, while changes in initial $\delta^{13}C$ are minimal. We find that the trend in increasing pCO_2 over the deglaciation remains robust, as higher initial soil pCO_2 does not lead to solutions matching the stalagmite data.

Mixing line 1 with max initial soil pCO2 8000 ppmv   **Mixing line 1 with max initial soil pCO2 15000 ppmv**
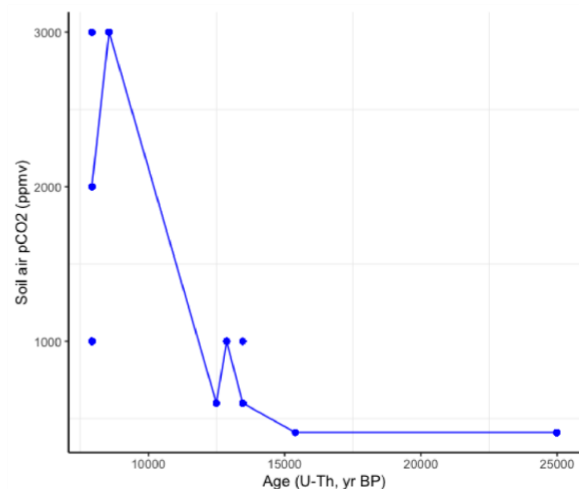

Figure 3: Model results for new mixing line 1 with maximum initial soil pCO_2 of 8000 ppmv (as used in the study, left), and increasing the maximum pCO_2 to 15000 ppmv. Higher pCO_2 does not lead to more solutions matching the stalagmite data.

2) We calculated two more mixing lines with different pCO_2 of the respired end member (10,000 and 4,000 ppmv; Figures 4 and 5) while keeping the respired $\delta^{13}C$ constant. Again, these simulations show a robust increasing trend in soil pCO_2 over the deglaciation, while the absolute values of the median pCO_2 change.
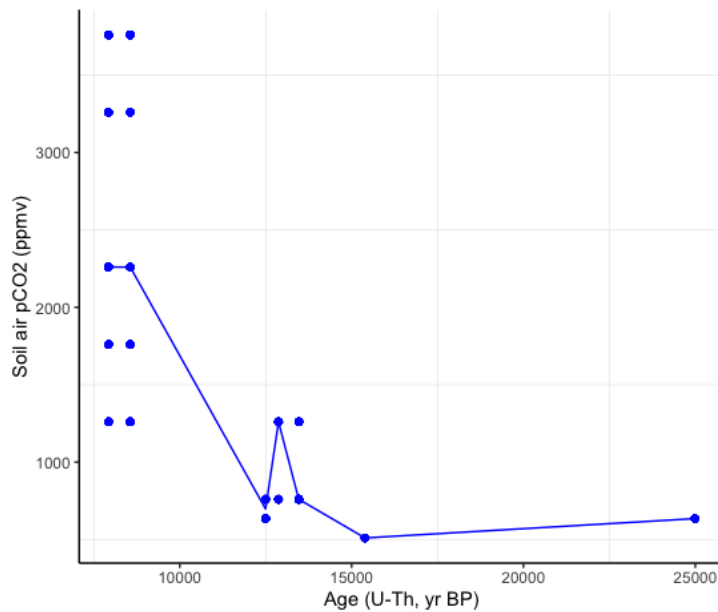
Figure 4: Example of soil air pCO₂ that leads to solutions matching the stalagmite data when calculating a new mixing line with respired pCO₂ of 4,000 ppmv.

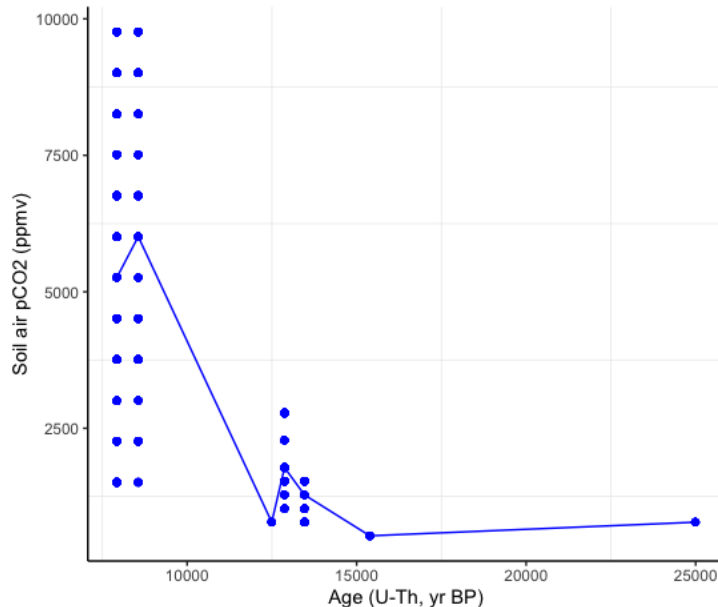*Figure 5: Example of soil air pCO₂ that leads to solutions matching the stalagmite data when calculating a new mixing line with respired pCO₂ of 10,000 ppmv.*

This sensitivity analysis shows that, while our model cannot reconstruct absolute soil pCO_2 values, the general trend over the last deglaciation is robust.

3) Holding soil pCO_2 constant and letting soil $\delta^{13}C$ vary leads to the entire 6‰ change in speleothem $\delta^{13}C$ being driven by changes in the respired $\delta^{13}C$. This is unrealistic, as biome-level values of respired $\delta^{13}C$ typically show little variation (e.g., Pataki et al., 2003), and therefore even a substantial deglacial transition from boreal to forested landscape would likely not lead to such a large shift in $\delta^{13}C$.

We also argue that substantial changes in hydroclimate are unlikely over the deglacial transition in northern Spain: this is shown by our $\delta^{44}Ca$ and DCF records (sensitive to infiltration and carbonate dissolution/reprecipitation dynamics), which do not show any

temporal trends. It is also supported by recent climate modelling results, which do not suggest regional aridity during the last glacial (Scheff et al., 2017).

Step 3: filtering for best model results
More discussion / sensitivity analysis should be done here. Were other options for finding the "best fit" considered? How does changing the thresholds for carbon and calcium isotope data affect the results? What happens if one uses a broader DCF threshold? If these decisions affect the results (or if they don't) it would be important to know.

Response: The model is not very sensitive to the choice of DCF threshold. We have tested increasing the DCF confidence intervals to +/- 3% and this did not lead to any meaningful change in the results. Changes in $\delta^{44}Ca$ however, are more important, and we had to increase the confidence interval from the uncertainty from the proxy measurement, as lower uncertainty led to the model not finding matching solutions for all three proxies. Of course, this could be circumvented by performing more simulations including more different parameter combinations, but this is out of scope for this paper as it would not lead to different conclusions.

The different sensitivities of the model to DCF and $\delta^{44}Ca$ illustrate how $\delta^{13}C$ is affected more strongly by changes in the processes influencing $\delta^{44}Ca$ (mainly PCP) than those influencing DCF (open-closed system carbonate dissolution dynamics).

Smaller comments and line-by-line
- Please clarify the use of "soil carbon" vs "soil-respired carbon". For example, line 199 states "The regression points toward a soil carbon end member…". Is this treated as just "soil carbon" in the modeling? Because the exercise seems to imply that the constraint is a "soil-respired carbon" end member. Line 190 also refers to the "soil carbon end member" but states it was constrained with data, not the modeled regression (which I think is accurate). Since soil respired carbon is defined as a component of soil carbon (line 182) this distinction is super important. It's still not fully clear to me how soil carbon vs soil respired carbon are treated in the model.

Response: We apologise for having caused confusion here. There are indeed differences between the soil respired carbon end member and the values used in the mixing line.

As discussed above, at our cave sites we can treat cave air CO_2 during summer months as a mixture between atmospheric, soil-respired CO_2. With the Keeling plot approach, we determine the $\delta^{13}C$ of the respired end member during pre-industrial periods to be -25‰ (see discussion in point 2).

For the modelling, we then define the mixing line along this regression, starting from a soil pCO_2 value that is reasonable considering our monitoring data and data from other comparable sites. This leads to the reported modelling end member of 8000 ppmv with a $\delta^{13}C$ of -24‰. As mentioned, we have now performed additional analyses using different end member compositions to test the robustness of the approach, which all lead to very similar results with respect to the deglacial trend in stalagmite $\delta^{13}C$.

We will clarify our use of the terms in the manuscript.

- I imagine that the soil respired end member of the mixing curve changes seasonally. If calcite formation is seasonally biased, could this affect the results? For example, are more model solutions at higher soil CO2 conditions possible when strictly summer-time inputs are used?

Response: Our monitoring data do not provide any evidence of a seasonal change in the $\delta^{13}C$ of the respired end member on the Keeling plot, as highlighted above. We

have modeled a single soil CO_2 and not a full seasonal cycle, but as analyses with higher pCO_2 have shown, such changes will not lead to different conclusions to our study. Updated monitoring information (Kost et al, in prep) indicates that many locations do not have a strong seasonal bias in calcite formation.

Line 36: Check out Bova et al., 2021 (Nature) for updated Holocene climate constraints.
Response: We thank the reviewer for this suggestion and will add the reference in the next version of the paper.

Line 49: I'm not convinced by Figure 1 that these records are "highly consistent in timing, amplitude, and absolute $\delta^{13}C$". I worry that the words "highly consistent" are overstating the data. Consider focusing on the main trends that are clearly robust, like the general shift to lower $\delta^{13}C$ values from 18ka to 6ka.
Figure 1B: Consider labeling the El Pindal and La Vallina sites.
Figure 1C: Is the straight blue line from ~18ka to 15ka just due to the fact that there are no data? It might be clearer to disconnect the timeseries lines whenever there is a sufficiently long duration of no data (maybe wherever there is ~500 years of no data or something).
Response: We have changed the wording of the figure description in the text. The sentence now reads: "Speleothem carbon isotope ($\delta^{13}C_{spel}$) records from the temperate region of Western Europe are often clearly correlated to regional temperature reconstructions during the last glacial (Genty et al., 2003) and the deglaciation (Baldini et al., 2015; Denniston et al., 2018; Genty et al., 2006; Moreno et al., 2010; Rossi et al., 2018; Verheyden et al., 2014) (Fig. 1), pointing towards a regionally coherent mechanism driving the response to the temperature increase".
Figure 1B: We have added a label to the El Pindal-La Vallina site on the map.
Figure 1C: This issue was raised by both reviewers. The record in question from Villars cave has very low resolution, but no hiatus was reported at that depth. We have added a sentence clarifying this issue in the figure caption.

Line 129: I don't know if CP allows citing papers in review, just adding it here as a note (although I assume that the authors have already confirmed that this reference is okay!)
Response: Thank you. This manuscript should be published soon and we will update the reference accordingly.

Line 169: Is this really deriving the soil carbon "…response to temperature change"? I think the link to temperature change is solely based on interpretation, not model derivation.
Response: This is correct, we have removed the last part of the sentence.

Line 184: Not a paper strictly on soil CO2, but Slessarev et al., 2016 (Nature) might be useful here for linking parameters of the soil carbonate system to the water balance.
Response: We thank the reviewer for this suggestion and will add the reference in the next version of the paper.

Line 194: "…by linear regression of the summer cave monitoring data". I assume these are the large-diamond points in Figure 2. But looking at figure 2 I assume that the regression data are spring, summer, and fall (since monitoring is said to be monthly and there is no indication that spring/fall data are removed). Which data are actually used in the regression?

Response: We apologise for the confusion. The monitoring data used are from May-November, reflecting "summer" conditions when surface bioproductivity is high and cave pCO_2 values are elevated. We will clarify this in the figure caption and text.

Line 196: While I suspect the offset of the forest data may actually be due to mixing with modern pCO2 (not pre-industrial levels), if the authors wish to keep this turbulence/advection effect argument I think it is important that a reasonable hypothesis for the signature of the third, unaccounted for air mass is added. Based on atmos circulation and likely boundary layer $\delta^{13}C$ in upstream ecosystems, is this mixing trend reasonable?
Response: As discussed above, we will now add more details, including a post- and pre-industrial mixing line to the figure and update the text to clarify our choice of end members.

Line 201: "… but they provide the best available constraints on the end-member". Wouldn't directly measuring soil CO2 provide a better constraint? (Although, as stated above, I disagree with using a modern calibration to get a Holocene end member)
Response: Directly measuring soil pCO_2 at the site would provide the most direct constraint on present-day composition of the respired end member. However, since our study calibrates the mixing line for a pre-industrial scenario, this would not be useful in our case.

Line 217: Why was each simulation for each timeslice repeated twice? Were they varied from one simulation to the other? (Table 1 only gives single values for each timeslice)
Response: Apologies, this is probably phrased confusingly. What is meant is that for each combination of all parameters, two simulations were performed, once using EH values for atmospheric CO_2 (concentration and $\delta^{13}C$) and temperature, and once using LG values. These were derived from the literature. However, our new approach now calculates the mixing lines differently depending on the time period, so this step is not necessary anymore.

Line 221-226: I'm a bit confused. Is there one set of binary filtering for the three mixing line simulations, and a different filtering approach (just selecting the best 5%) for the sensitivity tests?
Response: Yes, this is correct. It was necessary to use different approaches since the sensitivity test would not lead to matching solutions within the constraints of the proxy uncertainties, as there is not enough variation in the $\delta^{13}C$. On the other hand, for the mixing line experiments, using the constraint of the best 5% does not appropriately constrain the solutions and leads to excessive spread of the data.

Figure 4: Are measurement uncertainties considered in these regressions?
Response: No measurement uncertainties were considered here as these regressions are mainly meant to illustrate the concept. We will add information on the uncertainties in the next version of the manuscript.

Line 293-294: I don't think that encouraging model results is confirmation that "the estimate of the soil respired end member composition is accurate". More sensitivity tests are needed to demonstrate that other soil respiration end member compositions lead to problematic results (particularly when the end members are allowed to vary with time, as discussed above).

Response: We have now performed additional sensitivity tests and added changing atmospheric and respired end member compositions. These results do not significantly change the main findings of our study.

Line 309-311: This is another instance where I'm tripped up by terminology. I think "initial soil gas" is the same as "soil respired CO2" and not the same as just "soil gas" or "soil CO2"?
Response: As discussed above, this reflects the parameter selection used for the model, where we calculated our mixing lines that reflect a high pCO_2 end member (but not reflecting respiration only). Therefore, we use the term "soil gas". We will clarify this in the text.

Line 310-311: I would like to know more about this. Why does the sensitivity test require such an enriched $\delta^{13}C$ end member? How is the use of this end member justified over the use of the "calibrated" one? Is it a problem that the -22 per mille value does not yield many positive results?
Response: Our tests have shown that to obtain such high stalagmite $\delta^{13}C$ values as recorded during the last glacial, the soil gas $\delta^{13}C$ that the solution equilibrates with also has to be quite high. With the mixing lines, this results in the selection of an initial soil gas composition that has a larger atmospheric component (i.e., higher $\delta^{13}C$ and lower pCO_2). With the sensitivity test, this is not possible, as we keep $\delta^{13}C$ fixed. To avoid having too many simulations not matching the data (as would be the case when using a very negative end member), we opted to use a more enriched end member. This is not a problem, but rather illustrates how the speleothem $\delta^{13}C$ trend over the deglaciation requires a change in the initial soil gas $\delta^{13}C$.

Line 328: What is meant by "depth" here? I don't think the Pataki paper actually measures anything over soil depth.
Response: We have removed this expression.

Line 347: This is probably just my own problem, but I'm confused with terminology again. I thought initial soil gas might be the initial CO2 from soil respiration, but this sentence implies initial soil gas and soil respired CO2 are two distinct things.
Response: See comment for lines 309-311.

Line 433: "…best explained by.." my version of the document just says "c."
Response: Apologies for this mistake, it is now corrected and the sentence reads: "… the temperature sensitivity of $\delta^{13}C_{spel}$ over the last deglaciation in Western Europe is best explained by increasing soil respiration."

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