

Responses to referee comments on “Climatic variations during the Holocene inferred from radiocarbon and stable carbon isotopes in a high-alpine cave”

Our response is marked in blue.

We marked in yellow the questions of the referee.

Referee #2:

The strength of the paper is the combination of radiocarbon concentration and $\delta^{13}\text{C}$ in stalagmites samples at a continuous and high spatial resolution to understand what happens above the Spannagel cave during some period of the Holocene. However, I found that the manuscript is moderately written (please don't take this personal, and as a reviewer I am not qualified to comment in detail about the writing, but I strongly believe there is a room for improvement and writing is a learning process).

Also, in my opinion, the manuscript requires a lot of reorganization of ideas to make it clearer. For example, the first section of the discussion section, the anomalies in the old section of SPA 127 do not highlight the importance of the proxies being studied. Instead, it discusses the result based on methodological approach. Please note that the journal “Climate of the Past” is not a methodology journal (if this paper was submitted to a method paper, then I would not argue about having this part in the discussion). In fact, I see that this whole section either belongs to the method or some part of it could go as a quick interpretation of the data in the result section, but it should not belong to the discussion section.

We agree and we will follow the advice of the referee to move this part from the discussion section and only shortly describe it in the results and in the SI. Additionally, we will move Figure 2 and 3 in the SI and instead add a new zoom-in Fig. of current Fig.4 as suggested by Referee 1

Another example, a separate generality section about dcf can be helpful here (see detailed comments) where the authors could explain the difference between radioactive ^{14}C and dead carbon, and what are the potential sources of them. With this said, the sections at L 67-105 could belong to that independent section right between the introduction and the Materials & methods. If that general & fundamental notion is separated from the introduction, I am certain that the introduction could become concise and clearer, with a clear statement of the problematic, and a proposition of the new method and its potential relevance in future paleo-reconstruction.

As stated above, we will only use the dcf and not the initial ^{14}C .

I also feel the title does not fully capture the content of the manuscript. While the authors entitled their manuscript “Climatic variations during the Holocene inferred from radiocarbon and stable carbon isotopes in a high Alpine cave”, I found that the manuscript mainly use radiocarbon and $\delta^{13}\text{C}$ as a proxy for local changes and specifically what happens right above the cave in the epikarst, and not directly to climate. In their conclusion, it was made clear that these two are good proxies to understand carbon dynamic. Hence, I think the authors should emphasize the importance of $\delta^{13}\text{C}$ and F^{14}C in the use of stalagmites in paleoenvironmental reconstruction and build their discussion based on that, rather than jumping directly to climate, which at this stage seems more speculative.

-There are several points by the authors in manuscript that support my comments. For example, in the abstract, the authors used the variation in ^{14}C and $\delta^{13}\text{C}$ as an evidence of host bedrock dissolution or organic matter reservoir contribution from the epikarst to the cave. And in fact, this has been one of the focuses of the interpretation/discussion. The authors should make that clear that from using such inferences, information from the local place can later be applied to climatic context.

Same as stated above-

-In my reading of the manuscript, the bridge ‘local response-climate’ is quite obscure (possibly by the current way how the manuscript has been organized, or because this aspect is still difficult to fully link with confidence). Realistic suggestion: reorganizing the ideas would significantly improve the manuscript.

We will add a paragraph to the introduction (see above)

-In addition, interpretation of $\delta^{13}\text{C}$ is very complex compared with $\delta^{18}\text{O}$, although the water-rock interaction may also complicate its interpretation. Among the factors that complicate the interpretation of the C records in speleothems is the so-called PCP (or prior calcite precipitation, or to be general Prior Carbonate Precipitation, to avoid discrimination between the two common CaCO_3 polymorphs, calcite and aragonite). Could this factor influence the proxies being investigated in this study? E.g., for the large range (-8 to +1 per mil)?

We agree and we will add a third paragraph (iii) under hypothesis 2 and name it PCP. PCP can have an effect on $\delta^{13}\text{C}$, even as large ones as observed for our stalagmite. While this would not have an effect on $\delta^{14}\text{C}$, we would expect, that $\delta^{18}\text{O}$ should show a similar behavior, which is not the case. Thus, we can safely assume, that PCP is not responsible.

Minor but crucial: There are some confusing technical terms used in the manuscript that need to be specified. For example, the word 'precipitation'. The authors should specify if the precipitation reflects rainfall which is climate or if it represents the carbonate precipitation leading to the formation of speleothems.

We will clarify this throughout the manuscript.

To summarize my general comment, I see that the dominant aspects of the hypotheses are focused on the local processes that may affect the carbon stable composition. The paper and the research are interesting, but there is plenty of room for improvement. I hope my general comment and the detailed comments would help improving the paper.

We will also address the detailed comments.