RC 1:
The manuscript by Kost et al. presents cave monitoring data of air and drip water including element concentrations and isotope data of La Vallina Cave, NW Spain. The data are very well presented and their interpretation is concise. I just have a few suggestions for improving the manuscript and thus, recommend minor revisions.

General comments:
In my opinion, the statement that speleothem precipitation is restricted to summer AND winter seems a bit contradicting. For example, $\delta^{13}C_{DIC}$ values and pCO_2 cave air concentrations (thus, cave ventilation pattern) hint at preferred precipitation in winter. How valid are the model data for this? Or do you mean at some locations in the cave, there is summer precipitation and in other cave locations, there is winter precipitation? Especially in the abstract, this is quite confusing. Probably needs a more detailed explanation in the abstract or a rewriting, cause in the conclusions, it is far better explained.
[We thank the reviewer for this comment. To be clearer in what our findings show, we revised this passage in the abstract. The used model (I-STAL; Stoll et al., 2012) is well established and fed with monitoring data (Ca concentration, cave air pCO_2, temperature, drip rate). The findings of growth cessation in spring and fall leads to two growth phases during the year (winter AND summer) at most sites when conditions are favorable for calcite precipitation. So, growth conditions are favorable in summer and winter mainly driven by low cave pCO_2 in winter (ventilated conditions) and high oversaturation of dripwater in summer (high soil pCO_2 enhancing bedrock dissolution). The modelled calcite precipitation shows differences between sampling sites depending on the input parameters. One would expect rather similar growth behavior at Gloria, Gravel and Skyscraper (two growth phases in winter AND summer with growth cessation in spring and fall) and a single growth phase in winter at Playground. The example of Playground shows that irregular growth is possible in La Vallina cave since potential calcite precipitation at Playground is expected only during winter 20/21. Hence, the actual growth of a stalagmite growing in the cave depends on its location and cave/dripwater conditions. The growth cessation in fall is likely related to strong PCP effects as shown in Fig. 6 with increased Sr/Ca. In spring, low soil CO_2 and increasing cave pCO_2 restricts calcite precipitation. The negative growth rates during these phases could even suggest calcite dissolution.]

Methods: Please provide the information on which instrumentation was used for measuring Cave air temperature and relative humidity as well as the precision of the device(s). What is the measurement uncertainty of the Picarro for pCO_2 and $\delta^{13}C_{CO2}$?
[We appreciate the request on cave air measurement instrument and its precision. A transparent reporting of instruments and uncertainties corresponds with our reporting philosophy. We now provide information on the instrument and its measurement accuracies used to measure humidity and temperature. Furthermore, we added measurement uncertainties for the Picarro used to measure the cave air pCO2 and $\delta^{13}C$. The figures already showed error bars but we missed to add the information in written format in the previous version.]

Results section: There are already some interpretations /discussions occurring here: Cave air CO_2, hydrological conditions, 4.4 Isotopic composition of the drip water. Please go through that section and thoroughly separate what is a result and what is interpretation/discussion and move those parts to the discussion section.

[We identified several parts of discussion elements in the results section and carefully tried to remove them from the results section and incorporate them into the discussion section. We have made our best effort to present results without significant interpretation, in a few gray areas the later results sections require brief explanation of the rationale (e.g. Sr/Ca-index as a PCP indicator or the principle of cave ventilation) and we retain these brief sentences for clarity.]

For results section on PCP: I suggest to use also Mg/Ca and Ba/Ca ratios to help determine variations in PCP. The PCA shows that Mg, Ca, Sr and Ba seem to be influenced by the same environmental factor (PC1). Thus, I suggest including those ratios in the PCP part at least for Gloria, Skyscraper, Playground and Gravel drip water sites, which seem less influenced by seawater aerosol input. Also, what about a Sinclair plot?
[As suggested by the reviewer, a Sinclair plot is now added to the supplementary figures, and cited in the main text section 4.5. This is the best argument for a similar control by PCP. For some sites like Playground the range in Mg/Ca is too low and no statistically significant slope was found (p-value too large). Therefore, we only report slopes (and slope error = SE) of statistically significant data sets. We realize generally slightly smaller slopes (mostly within error though) than suggested by Sinclair et al. (2012) or Wassenburg et al. (2020); however, this can be explained with varying partitioning coefficients. A paper in prep. discussing effects of partitioning coefficients on the slopes is in the pipeline.
As Fig. 4 (or the figure below) shows, Ba concentrations indicate a higher variability suggesting other factors additionally controlling Ba (e.g. detrital particles). However, the long-term trends, as suggested by PC1, are fairly similar to Sr/Ca- and Mg/Ca-index.
To keep the manuscript slim and avoid further discussion (there's a lot of data already and reviewer #2 prefers a shorter version), we decided to stick to Sr/Ca in the main text only but now mention that Mg/Ca shows the same pattern. We do not comment on Ba as a PCP proxy.]

Lines 545-570: I miss correlations between $\delta^{13}C_{DIC}$ and cave air CO_2 concentrations in this section. In figure 2, $\delta^{13}C_{DIC}$ and pCO_2 concentrations seem to show anti-correlations for the different sites. Please check, if that is the case. Thus, this will highly strengthen your argumentation that the degree of degassing (which depends on CO_2 concentrations of cave air) has an influence on $\delta^{13}C_{DIC}$ values. Revise text here and elsewhere.
[We thank the reviewer for this input. Previously, we had not included the (anti-)correlation figure for the sake of keeping the large paper more concise. We add the figure to the supplement (Fig. A5b) and now mention in section 5.3:
"Additionally, the modest inverse correlation between drip water $\delta^{13}C_{DIC}$ and cave air pCO_2 in these sites is consistent with greater extent of degassing (and potentially PCP) during periods of low cave air pCO_2."]

I think you use "epikarst" when you mean the karst zone. Epikarst is just the uppermost zone of the karst, which is in contact with the soil. You cannot use it to describe the whole bedrock above the cave. Please clarify this. See for example Fairchild and Baker (2012) and Bakalowicz (2012).
[We thank the reviewer for this comment. We now use the more general word "karst" instead of "epikarst" to avoid any misunderstanding. It is mostly used in relation to soil/karst air and since in this case the CO_2 production by roots extends all the way to the cave (roots penetrate cave in some areas), we broaden our description to include the entire rock above the cave.]

Further comments:

Lines 19-20: This is a bit misleading. Please write instead: The carbon isotope signature of dissolved inorganic carbon of drip water…

[We agree with the reviewer that this phrase might be misleading for the reader. This is changed in the revised version.]

Line 23: What kind of cave air measurements? Be more precise.

[We added more specific information in brackets.]

Line 124: What does the abbreviation DEM stand for?

[A DEM is a digital elevation model. The full description is added now in the figure caption (Fig. 1).]

Lines 154 and 203: ConFlo IV

[Good call! A typo which is revised in the new version.]

Line 296, 316 and elsewhere: Please write hydrogen isotopic composition. When speaking of deuterium, only the hydrogen isotope with the mass 2 is meant.

[We totally agree with the reviewer's suggestion. We changed it throughout the manuscript accordingly.]

Line 411: Also due to the good ventilation in winter diluting and removing the CO_2 degassed from drip water in winter. I suggest adding that factor a bit more in the discussion in this part.

[Good point! We added this mechanism in the discussion.]

References

Sinclair, D. J., Banner, J. L., Taylor, F. W., Partin, J., Jenson, J., Mylroie, J., Goddard, E., Quinn, T., Jocson, J., and Miklavič, B.: Magnesium and strontium systematics in tropical speleothems from the Western Pacific, Chemical Geology, 294-295, 1-17, https://doi.org/10.1016/j.chemgeo.2011.10.008, 2012.

Wassenburg, J. A., Riechelmann, S., Schröder-Ritzrau, A., Riechelmann, D. F. C., Richter, D. K., Immenhauser, A., Terente, M., Constantin, S., Hachenberg, A., Hansen, M., and Scholz, D.: Calcite Mg and Sr partition coefficients in cave environments: Implications for interpreting prior calcite precipitation in speleothems, Geochimica et Cosmochimica Acta, 269, 581-596, https://doi.org/10.1016/j.gca.2019.11.011, 2020.