General comments

In their manuscript Maccali and coauthors present a new multiproxy speleothem record of MIS3 climate variability in South Africa. The manuscript covers two bases, a climate record of AIM12, and a high-resolution comparison of two different fluid inclusion-based speleothem temperature proxies. The manuscript does very well not to 'fall between two stools' and covers both components well. The record itself is good, but relatively short, and while the growth phases show regional coherency and is worth remarking on, the climatic conclusions are necessarily limited by the time span. However, the additional comparison between microthermometry and fluid inclusion water isotope-based temperature reconstructions is strong and novel, and elevates the study. It is well worth consideration for Climate of the Past.

The novel result of the paper is the lack of significant temperature variability in the YRZ during AIM12, despite millennial (and centennial) scale hydroclimate variability. The result is disputed by the two temperature proxies, with FIWI method showing warming and the microthermometry showing no change within error. The authors reason that the microthermometry is more reliable, as the FIWI signal is influenced by enhanced evaporation due to the dry conditions. If true, then this marks a significant moment, where the relatively nascent microthermometry technique appears to outperform the more established FIWI method as it is less influenced by in-cave hydroclimate variability. I think the argument made in the manuscript for this being the case is reasonable as the temperature change using FIWI is unreasonably large.

My main issue with the result is the lack of presented consideration of nuance, error bars and reliability of microthermometry. The Indian Ocean cooling from 48-46ka is less than 2C while the mean-to-mean temperature warming of the microthermometry is as high as 3C. Therefore, absence of evidence for change (within error bars no change) here is not sufficient for the evidence of absence concluded by the paper. I'm not sure I would expect such a large temperature change in the subtropics to millennial scale variability, unless a major front was involved. This research group is doing tremendous work to improve microthermometry, but this result is perhaps overstated and needs nuance. – While we have aimed to make the uncertainty of the microthermometry method clear in text and figures, we agree that some of our statements can be read as implying complete absence of any temperature variations. We meant to say that we do not reconstruct a detectable change beyond the method's uncertainty, and notably not on the scale that would match the variability in d18O, d13C and Sr/Ca. We will modify our conclusions on the absence of temperature change accordingly.

The manuscript is excellently presented. It is well-written and concise, covering all major bases, with few errors. The number of technical corrections is very small. Congratulations.

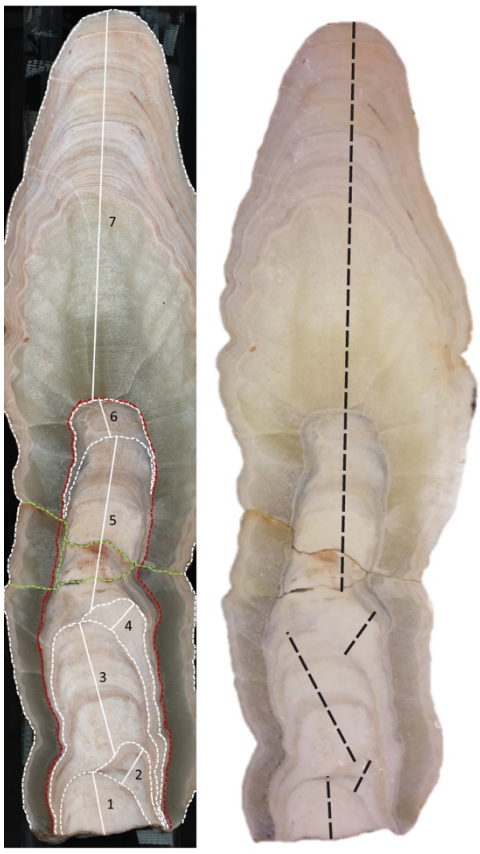
After writing my review, I have also had the chance to look at the RC1 comments from Dr. Braun. They seem reasonable and I agree with the majority of them.

Specific comments

Should the title include reference to the microthermometry, as this is one of the highlights of the paper.- We prefer to keep our original title as we would like to highlight the complementarity of all the different methods rather than the merits of one method over the others.

Should prior calcite precipitation be changed to prior carbonate precipitation? Can prior calcite precipitation be demonstrated (calcite stalactite, U/Ca information)? - Indeed, we will change prior calcite precipitation for prior carbonate precipitation as we cannot demonstrate calcite precipitation.

Was the XRF core scanning of the lower sections orientated perpendicular to the sampling axis of the stable isotopes in each individual growth phase, or just to the entire stalagmite. The former is not easy with an ITRAX and I would be curious as to how this was achieved. Was the stalagmite raised above the bed, was the bed adjusted, or was there enough room to reorientate the stalagmite? If the latter then by what technique was the data adjusted to the different depth scale, and could the authors comment to what extent was the data smoothed or otherwise compromised relative to the stable isotopes. - We think there was a misunderstanding; our XRF scans were performed parallel to the sampling axis, not perpendicular. The stalagmite was cut into flat slabs and further cut into smaller pieces. A flat support was inserted on the rail and the different pieces were placed on that support and rotated horizontally to align the isotope transects with the scanning direction, resulting in scans parallel to the isotope transects. The slabs were taped to prevent movement and adjusted for horizontality. The XRF scans and stable isotope transect were performed on different slabs, resulting in a slight depth offset, as illustrated in the inserted figure and figure 2 in the text. The data presented here are an average of three parallel scans performed a few mm apart. We will add these explanations to section 2.3.2 in the manuscript.



XRF scanning slab Stable isotope slab

Paragraph starting Line 121: The MIS3 growth phase is mentioned here, for consistency the late Holocene growth phase should also be mentioned here, rather than wait until line 199. - We will mention the Holocene growth phase and the MIS 3 growth phase already here.

Line 185: I recommend moving the number of replicate measurements for microthermometry up from line 231 into the methods section. - We will move the replicate information into the method section.

Is the ‘too old’ outlier age excluded due to Uranium loss? Could that indicate any potential bias in microthermometry at those depths? - We lack evidence to unequivocally assess the cause of

this ‘too old’ age. Both U-ratios and U-concentration for that sample are in the same range of values as the other ‘closed-system’ samples, making U-loss unlikely. In addition, it would require significant loss of U to alter the age while minor addition of ^{230}Th could significantly impact the age (Borsato et al. 2003, Studi Trentini di Scienze Naturali, Acta Geologica, 80, 71-83). This sample might have a higher content of organic matter adsorbing ^{230}Th and leading to that ‘older’ age. Such processes would however not impact the microthermometry data which we hence consider to be as reliable at this depth as throughout the rest of the record.

Line 283: Drip rate is not strictly the control on PCP, rather it is the measure. The control is the rate of infiltration through the karst and cave ceiling. – We will revise the manuscript accordingly.

Line 291: Dissolution is a feature of undersaturation, which can be caused by very wet conditions. There is a need further supporting evidence of dry conditions. Dust is already included, but trends of proxies into may also help. – We agree with the reviewer. However the dissolution features were observed on top of organic layers (e.g. stromatolite-like structures) and could be explained by microbial activity during periods of lower drip rates allowing bacterial communities to colonize the speleothem surface. We will revise the text.

Line 304: If unresolved hiatus are present then the duration of dry events could be even longer, not shorter. - We meant the duration of the isotopic peak could be shorter, but the duration of the dry event including both the isotopic peak and the growth stop would indeed be longer. We will revise the text to clarify this.

Figure 3/5: The Indian Ocean SST record should be shown alongside the microthermometry temperature reconstruction. It could either be in Figure 3 or Figure 5, depending on whether the authors view these as ‘results and discussion’ figures or ‘temperature and hydroclimate’ figures respectively. - We will consider adding the temperature reconstruction in Fig. 5 or add a figure in the supplementary material.

Figure 3: With a good choice of colour and transparency, overlapping shaded error ranges for both FIWI and microthermometry should be possible. - We will try to show both FIWI and microthermometry error ranges in shaded coloring.

Figure 5: There should be a better EDML age model through this interval. If not/alternatively, the Antarctic Temperature Stack (Parrenin, Science, 2013) and WAIS Divide (WDPM, Nature 2015) ice core records provide continuous Antarctic records through this period. - We will plot the complete EDML record (EPICA Community Members (2010): Stable oxygen isotopes of ice core EDML. doi:10.1594/PANGAEA.754444) as also suggested by reviewer 1.

Supplement: In the interests of transparency and open science, full age chemistry data should be reported. - These data were indeed missing; we will report the full dataset.

I think the authors are correct to go with an Antarctic dominated influence on regional climate variability. The 46.1 kyr BP and 45.5 kyr BP change seem to be suitably distant from Greenland millennial scale events (on the Buizert corrected INTIMATE chronology: GS13 (H5a) starts

48.59. GI12 starts 47.11, GS12 (H5) starts 44.51 kyr BP). The match of growth phase to AIM12 fits better, as does the onset of cooling with the Antarctic Temperature Stack (ATS).

Acknowledging the caveat that there is only so much one can determine from a single specimen, I wonder if there is room to comment on the growth periods of the BL3. 48.1 to 45.3 ka corresponds nicely to Antarctic Isotope Maximum 12. Further, this growth period fits very well with a speleothem growth interval in SW Madagascar at 47.9ka-43.6ka, attributed to the combined impact of high summer insolation and an Antarctic influence of Indian Ocean SSTs (Burns et al., 2022, QSR). There is a reasonable match also to a growth phase in Inland Namibia which starts at 47.3 ka, albeit one which lasts much longer (Railsback et al., Palaeo3, 2016, Railsback et al., QSR in review). Regional coherence of growth phases is suggestive of a genuine climate control, while the differences imply that the Antarctic millennial scale variability is more important at the more southerly latitudes of SW Madagascar and the YRZ, than in inland Namibia where the insolation control seems to be less sensitive to millennial variability. – We thank the reviewer for bringing these studies to our attention. The AIS influence on speleothem growth from Madagascar is interesting, and so is the growth in Namibia. We would like to mention here that on-going work on new speleothem samples collected recently indicates a continuous (except for potential shorter hiatuses) growth from ~90 to 45 ka which contrasts slightly with the discrete growth phases in Madagascar. We will revise the manuscript to include the studies of both Burns et al. 2022 and Railsback et al. 2016 and extend the regional comparison and the discussion on climatic control.

The Holocene growth phase really picks up around 3.7 ka, again approximately matching growth phases in inland Nambia (4ka onwards) and SW Madagascar (3.1 ka onwards)(Burns et al., QSR 2022, Faina et al. Malagasy Nature 2021), at the most recent summer insolation maxima. Maybe this is too speculative, but it's a useful regional comparison of well-dated high-res records. - These studies indeed provide useful records for regional comparison during part of the Holocene. However, as we explain in our reply to reviewer 1, we prefer not to put more weight on the Holocene section to keep the main focus of the manuscript on the MIS 3 section.

Technical corrections

- Throughout: missing superscripting of 18, 13 and 2 for isotopes. - We will correct this.
- Line 8: Misspelling of Sciences - This will be corrected.
- Line 54/62: SST should be defined at first occurrence. - This will be corrected.
- Line 67: Specify timeframe of radiation. Mean annual solar/summer/etc. - We will add the missing information.
- Line 77: also include soil respiration processes as a major control of speleothem d13C - We will revise the text to include respiration processes.
- Line 164: "subscript c stands for calcite" has been mentioned before. - This was indeed mentioned line 80. We will remove it from line 164.
- Line 224: "As with the Sr/Ca record" - This will be corrected.
- Line 373: I recommend including the specific dates of these samples (46.7 and 47.7?) here. - We will add the specific dates of the samples.
- Figure 5: The lines are very flat and do not 'show off' the data very well. Is there a way of making the y-axis variability more pronounced. Either by increasing the degree of

overlap between panels and/or by making the figure narrower. - We agree and will try to increase the spread on the y-axis.

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