

Interactive comment on “Last Interglacial (*sensu lato*, ~130 to 75 ka) sea level history from cave deposits: a global standardized database” by Oana A. Dumitru et al.

Anonymous Referee #2

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I have now completed my review of the manuscript of Dumitru et al. I think it is an useful contribution with several interesting considerations, which would be further expanded in the future works. Considering I'm not an expert of the WALIS database I don't give feedback on the database itself even if the most relevant information seem to have been considered. I have several points along the text which I can summarise in some general comments:

- From the text there is no a clear discriminant petrographic, geochemical and morphological criteria to separate POS from other speleothems which forms in freshwater pools in caves. I think this would be very useful to give this description to clarify

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that these are unique speleothems typology. - SVS are basically terrestrial limiting points. The use of hiatus as indicator of sea level submersion in absence of other clear evidences is probably dangerous and risk to be overexploited (hiatuses form in spelethems form many reasons). This is clearly stated in the manuscript. But then is not considered anymore as criteria and probably a more critical approach for previous data should be considered. - Detrital correction in speleothem dating is critical. Probably a short mention on the fact that different laboratories applies different correction is useful (also in the past). It would be useful to have a range of $^{232}\text{Th}/^{230}\text{Th}$ values found in POS speleothems for the reader in the text. - Along the text there are many sentences unclear and/or not very precise (in my opinion). I have suggested modification. - Some sentences need to be supported by references. Overall the manuscript is easy to read (even if not all sentences are consequential) and general conclusions interesting even if probably (considering the nature of the special issue and that of the manuscript) a section including some general more methodological approach in selecting material and advices as use data for some conclusion would be useful.

Specific comments (generally minor)

Line 40 "... (Capron et al., 2019), and MIS 5e is considered an analog for the Holocene." The Holocene is quite different in terms of insolation and sea-level history. Please avoid oversimplification. MIS5e is not a good analogue for the Holocene, but it is the closest interglacial we can study with relatively good details. The main point is that MIS5e has temperature higher than pre-industrial Holocene, but other boundary conditions are very different. Line 43 "... uncertainties in the reconstructed sea level." Please Insert a reference

Lines 42-43 "Fossil corals can be dated to relatively high precision but have meter scale uncertainties in the reconstructed sea level" Please insert references

Lines 43-44 "Other indicators such as erosional notches pinpoint sea level, but lack age control". Please add citation Are you sure you want refer to erosional notches (they

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are not the best, but in case I can suggest Bini et al. 2014 Earth Surface Processes and Landforms, 39 (11), 1550-1558). Note that this is the same problem for tidal notches, probably some like erosional sea level markers are difficult to be dated? Or? Some more general?

Lines 44-45 For this reason, there is a growing demand in exploring additional sea level indicators that can complement the information derived from fossil corals, while simultaneously having robust chronology.

Maybe the sentence is not completely true most of the indicators are well known but the point is to clarify the indicative meaning and to date correctly them.

Line 46 “. . .as coral reefs (Thompson et al., 2011). . .” are these unique of karst environment?

Line 50 “. . .submerged vadose speleothems (SVS; suggesting maximum elevations of sea level position).”

Not very clear. Why they should be maximum elevation? They indicate the cave inundation (see your line 80) or air filling, so they are terrestrial limiting points. I think we must be conservative in these concept or be more clear in the explanation I have the same observation to other points

Line 95 “ was actually located throughout the bulk of the rise-fall cycle (Richards et al., 1994; Surić et al., 2009). Therefore, it can be difficult interpreting the relationship between vadose speleothems growth and sea-level history.”

Yes, I agree. Translating in a few word they are terrestrial limiting points? Is that you want to say? Probably this concept can be introduced here considering the general nature of the special issue, to avoid to return to the same concept later. I think this point should highlighter later in the conclusion or in a special section where it should be stated that use of SVS hiatus need to be considered with cautions. Maybe showing some examples.

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Lines 166-170 “Several tools with different uncertainties have been used to measure the elevation of the cave deposits: barometric altimeter (± 0.1 m; Moseley et al., 2013), metered tape or rod (± 0.5 m; Harmon et al., 1978), inclinometer (± 170 0.05 m; Dorale et al., 2010).” I think here there is some confusion between instrumental precision and accuracy of the measure. Not clear. For instance barometric altimeter can give you this kind of precision, but it is quite far from the accuracy of the elevation measurement. All of this have been reported to local datum? Just to clarify.

Line 175 “...or that precise measurements are not so relevant because the uncertainties 175 related to local tectonics are much larger (Surić et al., 2009).” Sentence unclear precise measurements are mandatory. Maybe delete?

Section 2.5 “Given that speleothems are less likely subjected to alteration and diagenesis when compared with organic precipitation of corals, an elaborate sample pre-screening is not critical. Thus, only some of the studies compiled here report the mineral assemblage of the samples by X-ray diffraction (Surić et al., 2009; De Waele et al., 2018). However, we cannot exclude the possibility that screening was performed, but not reported.”

This is misleading section. XRD are fundamental in corals because they are aragonitic and tend to transform in calcite. Most speleothems are not aragonitic but calcitic. However, you stated at line 420 (why not before) that “The phreatic overgrowth mechanism that deposits calcite/aragonite at sea level...” So the problem for POS is real, and aragonite risk to be recrystallised all the same. However, it is a fundamental practice in speleothem science to select samples after a screening in thin section because also calcite can experience open-system behaviour. So this sentence should clearly state that (especially for POV and SVS) a check for potential alteration is necessary. A good case is the discussion made by De Waale et al 2017,2018, but for vadose speleothems there are many useful references for checking potential alteration. I think this section should be rewritten. I think what is lacking here which are the criteria to cite that POV are really POV and not formed in a lacustrine environment. I think there are not a very

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precise description here to discriminate them and to characterised better POV.

Line 201-202 “While not always necessary, including information on mineral assemblage, and diagenetic and crystalline descriptions are useful.”

I think this is a mistake and I don't think is the thought of the writers. Petrographic and diagenetic description is fundamental and mandatory!

There is no mention here on the clastic contamination (but it is later). Presumably in POV would be minor but according the compilation proposed it would be nice to have a range here of measured $^{232}\text{Th}/^{230}\text{Th}$ and eventually to give a range of correction performed by different laboratories. This can have an effect on the final age.

Line 215 “The development of TIMS and then MC-ICP MS in measuring U-series isotopes constituted a major step forward from the alpha spectrometric method.” Maybe here a reference is necessary.

Line 223-224 “Hellstrom (2006) suggested that a ratio of $^{230}\text{Th}/^{232}\text{Th}$ larger than 300 can be considered as an indicator of clean samples not requiring correction for detrital thorium.” It is probably useful to mention that different laboratories applies different correction (e.g. Bulk upper crust or iterative calculation of the initial ratio) (see also previous point).

Line 232 “. . . .age errors are now possible to ± 100 years. . . .” yes, but this kind of error for speleothems is probably just an analytical error. Considering clastic contamination and growth rate to have such a high “accuracy” is highly improbable (but of course not impossible).”

Line 241-242 “. . . . but the interpretation is hampered by the challenges of finding pristine and well-preserved corals and to the uncertainties related to the water depths above the corals.” Considering the general sense of this short introduction in the conclusion the point of “well-preserved” is out of scope here.

Lines 243-245 “i) POS have the ability to define the discrete position (Fig. 3), hence,

they are considered sea level index points, whereas ii) SVS provide only an upper bound, and they are called limiting points (Fig. 4).”

I’m wondering if considering the nature of the special issue this discussion on the indicative meaning of the two archives can just mentioned before and this section starting directly with the discussion of the indicative meaning of POS.

Line 255 “. . .with absolute errors. . .” What do you mean precisely?

Line 315-329 I think that SVS are just terrestrial limiting point. So they cannot used to infer any special cases for the position of the RSL. Also hiatuses if not accompanied by clear evidences of marine deposition, for speleothems above sea level are dangerous to be considered as indication RSL changes. A more critical approach probably is necessary also considering the discussion you made at the beginning of the manuscript.

Moreover, here would be important to discuss a little also Yucatan Peninsula (which is mentioned as important at line 342)

Figure 4. SVS elevation indicating maximum positions of RSL during the time of their deposition. Not very clear SVS are terrestrial limiting point so the RSL is below them, and it is not always clear when they stop, it is an assumption that they stop just during flooding. They can stop also before. The top can experience dissolution and so on. Just a note of caution.

Considering Fig. 4 there also included not submerged speleothems now above sea level and (on the contrary some need to better explained to the reader). If it is the case probably some other papers are forget. For instance in the Mediterranean there is the recently published paper Bini et al. 2020 QSR but there are also others. So, I think probably there Maybe the original paper explain why these speleothems can be considered SVS, but this not emerge from the manuscript. Note that Wainer et al. stated: “The timing of growth of speleothems, at elevations close to sea level can provide records of minimum relative sea level (RSL) (note you often state maximum?).

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In this study we used U–Th dating to precisely date growth periods of speleothems from Bermuda which were found close to modern-day sea level.” This is a special prerequisite (even if in my opinion growth stop is not enough evidence of sea level changes.). I think the reader looking at figure 4 maybe confused, so some more explanation is necessary.

Overall, I think the manuscript needs some clarification and expansion of the discussion in some points but careful (moderate) revision I hope will help improving the general quality and importance of this contribution.

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