

Clim. Past Discuss., referee comment RC1  
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## Comment on cp-2021-172

Anonymous Referee #1

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Referee comment on "Mid-Holocene rainfall changes in the southwestern Pacific" by  
Cinthya Esther Nava Fernandez et al., Clim. Past Discuss.,  
<https://doi.org/10.5194/cp-2021-172-RC1>, 2022

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### General

The present contribution is a welcome addition to the pool of proxy-data from speleothems for the South Pacific. This region is still poorly represented, and any contribution with precise ages is needed to test (and improve) the accuracy of climate models used to predict future trends.

Speleothem data are needed to increase the SISAL global speleothem database, which is being used by modelers.

There is, however, a major problem: lack of replication of the record is coupled with an interpretative framework that is not based on monitoring of the cave system. In particular, there is no information about the properties of present-day calcite growth in relation to rainfall under diverse drips. As recently highlighted by Treble et al. (2022), within the same cave different drips may be characterized by diverse hydrological pathways. These influence the  $d^{18}O$  values and trace-element concentrations (see also Riechelmann et al., 2022). Similarly, in-cave variability of drips chemical properties influence the crystallization pathways, which are the processes underpinning the formation of different fabrics. Such processes, seem to include dissolution-reprecipitation (Demeny et al., 2016; Frisia et al., 2018). Consequently, by considering hydrological and crystallization pathways, it is fundamental to replicate a stalagmite record for the same time interval.

It is now good, accepted practice within the speleothem-based palaeo research to analyse at least two overlapping records from the same cave, or two records from vicinal caves.

I am aware that this may be difficult in the South Pacific, but then this pitfall should be stated clearly both in the Introduction and in the Discussion. In the Materials and Methods the reasons for the impossibility of replication, absence of monitoring and poor knowledge of the hydrology and geology of the site (see my specific comments for the relative sections) should be provided.

## **Detailed Comments**

Introduction.

Line 45...ENSO control on SPCZ at interannual scale has been mentioned well before Timmermann et al., 2018. For example, Folland et al., 2002.

Note: I suggest providing a short explanation relative to the relationships between ITCZ and SPCZ. Clearly, as both are atmospheric phenomena, they are connected. Albeit extreme events in Tropical Australia seem to be influenced by the ITCZ migrations, as the majority of Australian Tropical Cyclones originate in (or near to) the ITCZ (Berry and Reeded, 2014). Similarly, the ITCZ influences rainfall and extremes in the Pacific board of Southern Asia. So, somehow, a reader like myself, who is not a scholar in atmospheric sciences, may get a bit confused. The SPCZ is mentioned in the Introduction as a driver of rainfall in the South Pacific, but then the ITCZ is mentioned as driver of Pacific climate. It is correct, but it would be best to briefly describe the teleconnections.

- In my opinion, as the research is focused on speleothems, the existing pool of similar archives should be provided, including: Partin et al., 2007 (Borneo); Sinclair et al., 2012 (Guam, Vanuatu, Solomon Islands); Partin et al., 2013 (Vanuatu); Moupin et al., 2014 (Solomon Islands); Haigh et al., 2014 (Queensland, Australia); Zhang et al., 2022 (S Asia). I suggest to stress that there is *already* a wealth of speleothem-based research that goes back for several millennia for the South Pacific, but the Mid and Early Holocene are not represented.

Actually, why the mid Holocene is an important subject of research in the South Pacific is not clear from the Introduction. I must admit that I thought that this was the result of dating after sampling...thus...chance. When compared to the other existing records, it comes to attention that there is a gap in South Pacific stalagmite-proxy-series between the last ca. 2000 years and this Mid-Early Holocene record. Thus, there is no long-term continuity and should probably be mentioned in the Introduction.

May I suggest rephrasing the introduction so that the uniqueness of this record is highlighted and the reader has not my same impression? I can only suggest something like: There are several available speleothem records for the South Pacific at annual to decadal resolution that cover the ca. last 1500 years (Faraji et al., 2021; Partin et al., 2013; Maupin et al., 2014 etc...). By contrast, little is known about the Mid-Holocene, when insolation progressively increased in the SH and decreased in the NH, with consequence on shifts in the ITCZ and....

Line 70...also on rainout (see Wang et al., 2019). And cloud physics, which is increasingly being identified as a driver of  $d^{18}O$  of rainfall and speleothems (cf. Haigh et al., 2014 and 2016; Sun et al., 2022).

Line 75. Before Fairchild and Baker (2012), Kendall and Broughton (1978) and Frisia et al (2000) recognized the importance of the physical features of stalagmites as markers of hydroclimate. I personally believe that it is best to refer to the original sources, rather than a compilation offered in a book that draws from these sources. I believe that it is somewhat "unfair" to those who devised concepts and methodologies and are not recognized for that. Particularly in a scientific environment where every single citation counts.

The list of processes that influence growth rate is longer than that reported. It includes also presence of calcite growth inhibitors and templates that favour crystallization. For

example, Faraji et al. (2021) reported that Na and Cl in solution may inhibit stalagmite growth.

- Geographic and climatic setting

I believe that information on the geology of the host rock should be provided. The composition of the host rock is relevant for the trace-element study and because it constitutes part of the metadata accompanying speleothem research, which will be archived for future studies. I have found it in Aharon et al. (2006), who reported that the Niue carbonate platform consists of *“partially dolomitized, shallow-water reef limestones that reach a mean thickness of ~400 m above an early to middle Miocene basaltic volcano. The uppermost unit below the surface consists of aragonitic and calcitic limestone with a maximum thickness of 20 m. Dolomite is generally limited to two vertically distinct units, a 55-m-thick upper dolomite and a >105-m-thick lower dolomite unit, separated by a 120-m-thick interval of undolomitized limestone”*.

Thus, it is possible to provide a brief description based on Aharon et al. (2006) if no further sampling was conducted. It would also be useful to add in the supplementary data a section reporting the cave within its geologic contextualization. It is clear from Aharon et al., (2006) that the host rock may be a dolomitized reef limestone, or an undolomitized, partially aragonitic and calcitic limestone. This has implications for the chemistry of speleothems that cannot be interpreted robustly without knowledge of the rock. For example, in the PCA (Discussion), Sr becomes a marker of WRI or PCP ...proxy of hydrology. But this depends onto where Sr (or Mg) is sourced from (aragonite, dolomite, Mg-calcite) and on the differential solubility of these phases. Discrepancies may be attributed to incorporation in the calcite lattice, which is indeed an issue, but also from pure hydrological pathways and the carbonate phases the solutions enters into contact with (cf. Treble et al., 2022). The influence of host rock properties on stalagmite trace elements and hydrology has also been recently highlighted by Riechelmann et al., (2022).

Line 110. The response of stalagmites to TC and/or exceptional rainfall (triggered by

climate drivers) may depend on the transmissivity of the hydrological pathway. This is the reason why replicate records are necessary. A multi-proxy approach helps, but when there is no monitoring, the only way to unravel the hydroclimate significance of the proxy data is through at least two replicate records spanning the same age.

- Materials and Methods

Line 115. Here it is explained why the Mid-Holocene was selected, because the top of the stalagmite was missing. Then, it would be honest procedure to state this in the Introduction, ... a fragment of a stalagmite helps deciphering climate in the Mid Holocene..

This is an important premise because, lacking replication and a monitoring benchmark, it is not straightforward to link stable isotope ratios to hydrology (Riechermann et al., 2013; Treble et al., 2022), or fabrics and trace elements to hydrology (Riechermann et al., 2014; Frisia et al., 2018; Riechermann et al., 2022). There are many uncertainties related to the interpretation. Statistics, I presume, may always find some significance in records that have some recurrences, as speleothem records often do. But what the actual significance of the relationships is depends on monitoring and/or replicate datasets. .

The definition of fabrics provided is based on Hartland et al (2014), who worked on a modern, hyperalkaline stalagmite. I believe that it is also based on the visual aspect of a polished slab, rather than on thin sections. Definitions commonly used by speleothem researchers can be found in Frisia (2015), Oster et al. (2015), Chiarini et al., (2016), Munoz-Garcia et al., (2016) Treble et al., (2016). If no thin section study of fabrics was carried out, please provide a simple explanation about the choice of descriptive terms.

### 3.4 Greyscale analysis

Please add: *vertical* growth axis.

3.5 I am under the impression that there is an assumption that layer counting is equal to counting annual layers. This is assumed from dry season/wet season layering contrast (Fig. S3). But without monitoring, it is somewhat a big assumption. This is why a second record covering the same age would be needed, to check that there is a replication of the layering. From Fig. 4 in the text, a reader cannot evince that these are annual laminae. The couplets compact/porous may arise also within a year. The hydrological pathway may have simply shifted from matrix to fissure flow following an exceptional recharge and then reversed to matrix flow. The fabrics are a powerful tool for the reconstruction of hydrology, but hydrology is not the same as hydroclimate. The assumption is big here.

### 3.8 Trace element analysis

Please add: vertical growth axis...

Line 165 "in the supplementary material" not "at..."

- Results

### 4.1 U-Th ages

"Two out of the eleven U/Th measurements were discarded due to large uncertainties, one due to likely hiatus (Table 1)". This confirms my idea that replication of the record should be necessary, albeit I can understand the implications. Yet, the stalagmite used for this study may have been subjected to U loss (through dissolution-reprecipitation) or to detrital Th contamination (maybe during exceptional infiltration). Without petrographic investigation by optical microscopy, it is impossible to know. There is an age inversion at ca. 213 cm from top, where there is no visible hiatus. It would be best to explain in detail why the date was discarded. Maybe this indicates that some process (such as diagenesis) occurred and may have altered the geochemistry. The hiatus visible at ca. 390 mm from top also suggests that the stalagmite may have periods of growth interruption. By simply looking at the image in Fig. 2 the reader can see that there was a change in the feeding system (the vertical growth axis of the stalagmite shifted), which could be due to any in-cave processes such as the breakage of the stalactite, a marked change in growth style (the diameter of the stalagmite changes and there is precipitation on the flanks) between 140 and 160 mm from top, some changes in morphology between 290 and 310 mm from top. In addition, there are some large uncertainties. At ca. 60 mm from top the uncertainty in the dating can be due to diagenesis. By visual inspection of the scanned slab it appears that the dating was performed on a relatively lateral part of the then outer surface, which has a slight concave morphology. It is then reasonable to infer that water resided at the top of the stalagmite. This may have posed consequences for potential leaching (for example if the film of fluid was depleted by unsaturated fluid).

- Statistical analyses

Would it be possible to have more information about "All records included in each PCA were standardised to the lowest resolution in the group by averaging the data corresponding to each lowest-resolution time interval to accommodate the differences in temporal resolution (Table 3)." I am afraid I do not understand the accommodation of differences in temporal resolution. I might be mistaken, but the way I understood it is that some "accommodation" has been carried out.

As I have already hinted at, monitoring data would have helped to support claims of fabric seasonality. That the rainfall is "higher in the wet season"... is understandable, as it is the "wet" season. But when and how is the signal of the wet season signal is incorporated in the speleothem calcite is not known. And the how is particularly relevant. The O isotope-ratios values seem to be influenced by the activation of fracture vs. diffuse flow, and, thus, rather than being a signal of direct infiltration, these values may be a signal of hydrology (Treble et al., 2022). Similarly, the greyscale record is a record of hydrology, as fabrics respond to drip-rate. Without the benchmark of calcite farming, it is very speculative to ascribe porous fabrics and compact fabrics to a wet/dry mode. The fabrics are a record of hydrology modes. These could be annual or inter-annual, depending on the rainfall. And we cannot assume that the Mid-Holocene at Niue had the same pattern as at present.

## 6.1

It is my thinking that the interpretation is simplified and does not account for the fact that all is really based on one stalagmite and on the assumption that fabrics record hydroclimate. Given that there is no information of the host rock, the "dissolved carbonate supply" is relatively unknown (from dolomite? Aragonite? Mg-calcite? Low-Mg calcite? A mixture?). Second, the formation of fabrics is unknown - without calcite farming. Diagenesis becomes a potential player in the game of speleothem fabrics formation (Bajo et al., 2017). For example, micritic carbonate fabric is cited. Well, this fabric is known to form under the influence of bacteria (cf. Frisia & Borsato, 2010). If it is "nearly-glassy", then there is the strong suspect that it can have undergone dissolution-precipitation and ripening (Frisia et al., 2018). Then, it becomes a possibility, as I have already mentioned, that some "compact" laminae are the product of diagenesis. Then the isotope ratio values would reflect either "degassing" and/or evaporation during the ripening process and/or the potential value of precursors if diagenesis occurred in a semi-closed system (with bacteria present).

Without the benchmark of monitoring and calcite farming, or support from thin sections, it is difficult to interpret fabrics. Hence, the whole discussion is speculative. Which is not a problem if it is clear that there are many assumptions due to the lack of replication and monitoring data. Yet, as I have already mentioned several times in the review, it is



important to keep remarking that this is a serious issue and care should be taken. The most important fact is the dataset, regardless to the interpretation.

6.1.4. In principle, one could agree, but, again, because there is no control on how speleothem calcite actually grows in the cave and there is no replication, this part of the discussion related to one old stalagmite whose hydrology is unknown. So, how representative is it of the whole Mid-Holocene climate at Niue? I am totally ignorant on statistics, but I start to worry that if one applies statistics on a single record, which is not replicated, which is interpreted on the basis of "assumptions" about fabrics, and where the age uncertainty is over tens of years (Table 1), then I am not sure I understand what the statistics is really demonstrating? If laminae formation occurred also inter-annually, because the hydrology of the feeding system is not known, then there could be a problem. The "compilation" by Baker et al. (2021) provides some good points about the difficulties of defining annual layering in stalagmites.

But this is this reviewer's point of view and, as such, reflects some principles derived from experience. I can only recommend that the dataset should be replicated.