

Interactive comment on “Monsoonal forcing controlled cold water coral growth off south-eastern Brazil during the past 160 kyrs” by André Bahr et al.

Robin Fentimen (Referee)

robin.fentimen@unifr.ch

Received and published: 24 August 2020

Manuscript review Bahr et al.

Title: "Monsoonal forcing controlled cold water coral growth off southeastern Brazil during the past 160 kyrs"

General comments

Based on a multiproxy study of sediment cores M125-34-2 (on-mound) and M125-50-3 (off-mound), Bahr et al. set out to constrain the long-term development of Bowie Mound and to understand the environmental forcing behind its formation. They essen-

Printer-friendly version

Discussion paper



tially conclude that an enhanced delivery of terrestrial organic matter during Heinrich Stadials (HS 1, 4 and 6) played an important role on cold-water coral growth off SE Brazil. They compare their set of sedimentological and geochemical proxies to previously published data in the area. As such, the study is based on a solid and plentiful number of proxies. The chronostratigraphy of the core is well constrained and allows, in my opinion, for a satisfying interpretation of the data. The discussion is to the point and not too lengthy, it could even go a bit more in depth (see Comment #3). The stable isotope analyses of infaunal foraminiferal tests is where the most improvement could be done. This is detailed in Specific comment #1. The conclusions drawn by the authors are arguably by the choice of method, and may have yielded more precise results and details if the approach would have been different (and more taxonomically precise; see comment #1).

The quality of the English in the manuscript is at times insufficient. Some corrections are listed in the section “Technical corrections”. In addition to these, the manuscript would need a few extra proof readings to reach the desired quality. I am however confident that this can be done shortly and satisfyingly by the authors.

All things considered, I would be happy to recommend this manuscript for publication in Biogeosciences if the points below (plus the English in the text) are addressed by the authors. The manuscript presents a novel and interesting dataset that falls within the scope of the journal and will be of interest to its readers.

Specific comments

Comment #1: My main comment concerns the grouping of different *Uvigerina* species for stable isotope analyses. The authors mention the genus *Uvigerina* spp in the material and methods. It would be good to mention here the species considered in this grouping. How many species were considered in the grouping? Was one species more abundant? Is one species more abundant during specific intervals (e.g. within CWC-bearing intervals)? Indeed, it has been demonstrated that the response to trophic

[Printer-friendly version](#)[Discussion paper](#)

conditions is species-specific for the genus *Uvigerina* (see for example, Theodor et al., 2016 *Marine Micropaleontology*). *Uvigerina mediterranea* is for example better suited than *U. peregrina* to reconstruct trophic conditions, since it is more of an opportunistic species. *Uvigerinids* do not share the same ecological preference (see for example Fontanier et al., 2006), thus I am quite skeptical about this grouping. In my opinion, the grouping of *Uvigerinids* together weakens the use of stable isotope analyses performed on their tests, since it is not monospecific (as mentioned by the authors at Line 339). Hence the conclusions of section 5.1.1 are not as solid as they could be if authors considered species alone. Although I understand that this approach was chosen as a second choice because of the lack of material, I suggest that the authors should address more and discuss this point more in detail in the material and methods section and in Section 5.1.1.

I recommend plotting the $\delta^{13}\text{C}$ of individual *Uvigerina* species and then to compare this to the results of the grouping (all species combined). The scatter of the normalized data may possibly be due to the effect of the grouping. This can be easily verified by isolating different *Uvigerina* species and adding an extra colour code to Fig. 4A. As such, the results presented by the authors would be clearer.

Comment #2: Although the interpretations and conclusions are in my opinion sound, the association of coral proliferation with HS 4 does not seem as clear as for HS 1 and 6. There is an offset between the Ti/Ca and speleotherm records presented in Figure 5 with the coral proliferation phase. Is this due to an age model uncertainty? I think this offset should be discussed a bit more in detail.

Comment #3: It would be appreciated if the authors took the discussion one step further by comparing the environmental forcing observed in the study area to other CWC settings, e.g. along the East Atlantic margin or in the Mediterranean. This could be done in the last section of the discussion. For example, Wienberg et al. (2010) suggested that aeolian dust had a local fertilization effect on coral growth in the Gulf of Cadiz, whilst Fentimen et al. (2020) propose that fluvial input triggered coral prolif-

[Printer-friendly version](#)[Discussion paper](#)

eration during Greenland Interstadial 1 in the Western Mediterranean (Melilla Mound Province). Authors should also consider the work of Mienis et al. in the Western Atlantic. As such, the conclusions of the authors fit in with other previous observations and add new evidence. This is something that I believe should be better highlighted and deserves to be developed. The last statement of the conclusion that “This study (...) points at a hitherto unrecognized intimate coupling between continental hydroclimate and ecological changes in the deep ocean” is in this sense too bold and should be tempered. Indeed, previous studies already suggest this.

Also the link between coral growth and monsoonal forcing is only written and stated clearly in the title. No mention of the term “monsoonal forcing” is done in the discussion and conclusion. I think that if the title uses this term, it should also clearly be stated and discussed in the discussion (noticeably in section 5.2).

Technical corrections Title: “cold-water coral”, missing “-“ Line 25: “located at” and not “located in” Line 42: “constrained” and not “constraint” Lines 48 to 52: These two sentences need to be rephrased; I cannot get the meaning of the sentences as they are. Especially in the second sentence, the verb is missing (“Changes the species (...).”). Line 53: Explain the abbreviations POC and DOC the first time you introduce them, some readers may not be acquainted with these. Line 55: This sentence needs to be reworked, it is not understandable as it is: “Note, however, that similar studies in the feeding in the properties (...).”. Do the authors mean feeding properties / feeding behaviour? Line 58: In the sentence: “All affect the capacity (...).” I would suggest repeating the word parameters or variables, i.e. “All these parameters (or environmental variables) affect the capacity (...).”. Line 61: check the grammar: “to play a role in” not “to play a role for” Lines 62 to 64: The end of this sentence is not clear, consider reworking it. For example: “(...) importance of surface productivity in providing food to the deep ocean”. Line 70: I would suggest not to start the sentence with an abbreviation (Here CWC). Line 72: “Adapted” and not “adopted” Line 82: rephrase the sentence: “demonstrates for the first time” instead of “for the first time demonstrates”.

[Printer-friendly version](#)[Discussion paper](#)

Lines 81 to 83: The combined use in this sentence of “for the first time” and “a so far underestimated” is possibly a bit redundant. I would recommend less emphasizing in this sentence. There is no need to say it is “so far underestimated” if it is the first time it has been observed. Figure 1: Numbers on the hydrographic section (top left) are barely readable. I would suggest increasing the size of these. Line 133: Spelling: “half” not “halve” Line 137: Correct the beginning of the sentence: “Core M125-34-2” instead of “The core (. . .)” Line 145: Correct the beginning of the sentence: “To constrain” instead of “for constraining” Line 146: Correct the English: “was sampled at (or sampled every 10 cm)”, instead of “was sampled in” Line 168: “Half” instead of “halve” Line 181: “at Heidelberg University” instead of “at the Heidelberg University”, or rephrase: “at the Department of Geosciences, Heidelberg University”. Line 184: “were analysed with the Diffract Suite (. . .)” instead of “was analysed with Diffract Suite (. . .)” Line 185: Avoid using the passive form to often when possible. For example here, rather write: “The Rietveld refinement program DIFFRAC.TOPAS (Bruker Software) was used to perform quantitative phase analysis”. Line 195: “Weighed” instead of “weighted”. The verb is “to weigh” (thus weighed in the past tense), the noun is “weight”. Line 195: I would suggest rather writing “filled to the top” instead of “filled until capacity”. Line 198: correct: “(. . .) and put into an ultrasonic bath”, instead of “(. . .), put into an (. . .)” Lines 204 and 205: Is there a mistake here: “The high number of replicates resulted from”. Do you mean: “resulted in” ? Line 257 and 258: No capital letter given to “core” (write “core”) Figure 3: The symbol (white diamond) of *Uvigerina* spp. appears to be missing on the figure. Line 369: correct to “seemed to have” Line 382 to 384: Check the sentence for grammar: “increasing” instead of “increase”, “suggests” instead of “suggest”. Line 483: The sentence needs to be rephrased, it reads: “Due to their baffling capacity, the additional sedimentary input would have aided mound formation”. I would recommend rather writing: “Due to the baffling capacity of CWCs, the (. . .)”. As it is, the sentence suggests that the mound baffles sediment, whilst it is the corals not the mound in itself.

[Printer-friendly version](#)[Discussion paper](#)

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2020-206>, 2020.