

Interactive comment on “The influence of Atlantic climate variability on the long-term development of Mediterranean cold-water coral mounds (Alboran Sea, Melilla Mound Field)” by Robin Fentimen et al.

Naoufel Haddam (Referee)

anhaddam@ifremer.fr

Received and published: 27 August 2020

General comments: Fentimen et al. provide interesting and new data on cold-water corals for the Alboran Sea covering the last 300 ky. They present a multiproxy comparison, using lithological properties and benthic foraminifera faunal assemblages to assess the environmental changes in the area. These new data are valuable, well presented in a well written manuscript, and I strongly advocate for their publication. However, the interpretation of the X-ray fluorescence (XRF) is a bit problematic for the production of decisive environmental interpretations, considering that the principal aim

[Printer-friendly version](#)

[Discussion paper](#)



of this study is to define the climatic processes susceptible of affecting the coral mound formation. I suggest the addition of some figures and more importantly, to either tone-down some of these interpretations, or if possible, to add (or compare to) more records that support the environmental reconstructions presented in this article.

Specific comments: In the introduction it might widen the scope of the manuscript to add that the selected coral mound is (strategically) located at the interface between different water masses as showed in fig. 2.2. It is later an important aspect of the discussion. The Oceanography section could be improved by describing water masses from shallower to deeper, if possible. More Calls to the figures in this section would be helpful. It would be helpful to add in fig.2 the water masses flowing directions (crosses and dots, in and out of the paper for example). Could you please elaborate whether Alboran Sea gyres strengths and structures display seasonal changes, as you mention that they are non-permanent? Concerning the benthic foraminifera faunal assemblages in section 3.5, I was wondering whether you checked the 63 -150 μm fraction? By doing so, you could (qualitatively at least) assess if there is a bias on small species (eg. the opportunistic specie *Epistominella exigua*)? Also, you did not mention on which literature you based your foraminifera species identifications? This should be added in the material and methods. I am aware of the difficulties due to the inconstant depositional processes in this area (which is also a problem in this study but I won't insist on it as you dealt with it fairly in the manuscript), but did you estimate the accumulation rates of benthic foraminifera (BFAR), and compared it to the foraminiferal density and TOC? It would be very helpful to add pictures of the benthic foraminifera cited in the text, especially those selected for any geochemical measurements. Providing pictures should be generalized in the litterature as confusions persist. I don't have access to the Annex, but you could add there a reference list to the original description of each foraminifera specie, at least for those cited in the main text. It is hard to get decisive and conclusive interpretation from the TOC, according to the curve presented in fig. 4. It would be interesting to compare this curve with the BFAR. In any case, the interpretation should be toned down. If available, other proxies of primary productivity changes

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

would be appreciable. If the sieved samples are still available, the fastest (but not the best) way to try check this would be to count the planktonic foraminifera and calculate their accumulation rates? Section 4.3 You only show the section 1 in the figure, will the other sections be added in supplementary? This would be interesting to illustrate your description line 320. I agree that Zr/Al and Rb/Al differ from Ti/Al and Si/Al but mostly in the fact that the first two are harder to interpret than the later. Unfortunately, I am not sure that any conclusive strong interpretation can be extracted from these curves. In the discussion it would be very helpful to have a figure showing TOC + benthic forams assemblages (at least the species that have a “significant ecological meaning”) + $\delta^{13}\text{C}$, especially to illustrate the discussion lines 555 - 560. Line 412-413 You only described *B. spathulata* as opportunistic (line 394), but you used the plural form “taxa”. Do you consider all the fresh organic matter-feeding species as opportunistic? Also, it has been suggested that relatively small shelled species that rely on fresh organic matter show a faster (/stronger?) response to seasonality changes (Fontanier et al., 2006). Are there any signs of *E. exigua* in the samples? Since we are in the seasonality topic, are there any past and present evidences of seasonal changes affecting the surface stratification in the area and also the primary productivity? This would be the right place in the article to discuss this topic, and eventually support the benthic foraminifera findings (especially for what is observed at MIS 5 and 7). This addition would be a solution to balance the ““weakness”” of the XRF trends. You could also add a figure showing XRF + benthic forams for the runoff hypothesis. Is there any occurrence of deep infaunal and/or dysoxic species such as *Globobulimina* spp, *Chilostomella*, etc ? Section 5.1.2 It is very difficult to see systematic interglacial SS increases supporting seafloor turbulence. Also, the abundances of *T. angulosa* which is often associated with strong bottom water energy do not support this here. The steady fluvial increase during MIS 5 suggested by SS figure 8 might be plausible plausible, but it is still hard to observe a systematic strong glacial/interglacial signal. The second half of section 5.1.1 is well argued. But I still have questions about the fate of these runoffs. How can we be sure that this “material” is not displaced laterally by the currents, away from

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

the studied area? Line 621 I am not sure that there is enough arguments in the discussion to dismiss water mass rearrangements yet. Line 646 What about seasonality changes? For the conclusion and also as a conclusive remark for all the manuscript; I suggest toning down the terrestrial inputs implications as the XRF data far from being clear enough to give decisive interpretations. I also suggest keeping the door open to other processes such as water mass reorganization or maybe the impact of primary productivity changes due to seasonality variations (affecting the gyres?).

Other suggestions: - Lines 24 – 36 I would suggest a reorganization of the second half of the abstract, as it feels that information are randomly presented, which might confuse some readers. - Line 134 isn't it northwest instead of northeast that the MAW enters the Alboran Sea? - Line 226 I think that it is important to mention in section 3.5 the error on the benthic foraminifera relative percentages. With ~300 specimens counted, variations of less than ~5 % are not very trustworthy. For more statistics of the sorts you can check and cite Patterson and Fishbein, 1989 and Fatela and Taborda, 2002. - Lines 300 – 304 are a bit too interpretative and should be placed and developed in the discussion. - Line 309 The sentence "This trend is mirrored in GS (Fig. 4)" is not useful as you are describing both SS and GS starting at line 306. - I think that you could place figure 5 in supplementary information, and put the sentences lines 310-313; "The percentage of sortable silt (SS%) increases with. . .and SS% is indicative of a sorting process induced by bottom currents (Fig. 5)", in the discussion, to support your (toned-down) interpretations. - Line 323, is it possible to indicate quantitatively the dominance of *B. dichotoma* ? - Could sentences lines 327-329 be simplified by just saying that bryozan and coral content is generally inversely correlated? - Lines 346 onward, it would be helpful to display the mean percentages of each species within the *Bulimina* grouping. Just out of curiosity, is the offset between the 3 species relatively stable down-core? I would also displace the diversity sentences (lines 343 – 345) to the end of the 4.4 section. - Line 359 I think you meant *T. angulosa* which is the one showing a ~30 % abundance during MIS 6. I can't see 30 % for *D. coronata* during MIS 4 in figure 7. I advise to recheck the description of this figure in general. - Section

4.5 The Holocene is not mentioned, yet it is among the periods showing the most changes. - Line 398 I would replace “support” by “suggest”. As mentioned before it would be better to tone down the interpretation. It is also a good spot in the text to put the origin of the TOC. - Line 428 could you please precise where these inputs occur?

Interactive comment on Clim. Past Discuss., <https://doi.org/10.5194/cp-2020-82>, 2020.

CPD

Interactive
comment

Printer-friendly version

Discussion paper

