Letter to the Editor – manuscript cp-2021-162

Dear Editor,

You will find attached a revised version of manuscript cp-2021-162 (+ tracked changed version) together with a point by point response to the letters by the reviewers. We have followed the recommendations made by the reviewers and the corrections that were proposed. The introduction and discussion have been thoroughly reworked. We now better introduce the current knowledge about Mediterranean cold-water coral mounds, noticeably the recently long-term records published by Krengel (2020) and Corbera et al. (2021; 2022). These revisions made to the introduction are in line with the changes made to the structure and content of the discussion: in a first part we discuss the build-up heterogeneity among Mediterranean coral mounds before exposing the interglacial and glacial environmental controls. This first part discusses our observations in comparison to other Mediterranean coral mound records and insists on the glacial coral occurrences in core MD13-3462G (a point which reviewer 2 asked us to insist better upon), points which were lacking in the former version of the manuscript. The conclusion and abstract have been changed according to the changes that were made to the discussion. Overall, we have strived to boil down the discussion, to insist on key points, to better compare our observations to newly available literature, and to avoid over-using some proxy data (the Si/Al ratio is no longer mentioned, whilst we only mention shortly the sortable silt values) as suggested by reviewer 1.

Some figures have been improved and laid out in landscape format (following reviewer's 1 comments). We have placed the cores mentioned in the introduction and discussion in Figure 1; we now plot the $\Delta^{13}C$ and $\Delta^{18}O$ in Figure 5 and have removed the Si/Al ratio. We decided to remove former Figure 7 as we believed it was no longer relevant to the study (and the manuscript is already figure-rich). In contrast, we have added two plates illustrating important benthic foraminiferal species (Figure 9) which are presented in the text. This addition follows a recommendation that we had received for the previously submitted version of the manuscript and had not yet been able to meet.

We hope that you find the manuscript improved,

Best regards,

Robin Fentimen and co-authors

Response to review Report #1

We thank the reviewer for the comments and recommendations made. We have answered each comment separately below (blue font). We have followed the recommendations made, though we have not included any multivariate analysis as explained in the response to comment 4. We have strived to refocus the manuscript on discussing our results in light if what has been observed in other coral mound records from the Mediterranean (see also response to Reviewer 2). The figures have been improved following the recommendations made. Furthermore, we have worked on only incorporating the most relevant proxy data (Rb/Al ratio has been removed, we spend less time discussing variations in the sortable silt records since it does not show good correlation with other proxies). The discussion has been reworked (also following comments made by Reviewer 2) to better insist on major findings and to remove superfluous sections.

The authors provide a comprehensive multiproxy study in which they propose a link between a) increased run-off and b) enhanced water column mixing during interglacials fueling cold water coral (CWC) proliferation at Brittlestar Ridge, Alboran Sea (western Mediterranean Sea). They further find that Bryozans are dominant mound-builders during glacial periods, as they are more adopted to the glacial environmental conditions than CWC. The paper is very well written, with an instructive summary sketch (Fig. 10), albeit the figures might be improved in some instances. In summary, the authors present a very interesting data set shedding light on a variety of environmental parameters that might influence CWC growth, while also acknowledging the limits of their approach due to the particular environmental setting of the CWC mound that generates a particular hydrodynamic microenviroment and stratigraphic uncertainties.

In general their main hypothesis appear plausible on first sight, however, when looking at the proxy data more closely the claims are not always convincingly and consistently corroborated by data. This might be partly due to the graphic display which does not allow direct comparison of paleoenvironmental parameters with CWC abundance (see below), but also leave the impression of over-interpretation and over-simplification of the proxy data and involved processes. Hence, in a revision the authors need to better support their conclusions by data or re-evaluate their interpretation. Notably, this paper has been previously submitted to CP, and my concerns partly echo those raised in relation to the earlier version of the manuscript.

Main comments

1) The authors infer that enhanced terrestrial sediment and nutrient input fueled CWC proliferation during interglacials. However, when looking at either Si/Al and Rb/Al, both ratios do not show an glacial-interglacial pattern. The long-term smooth of Rb/Al appears to show no distinct fluctuations at all, Si/Al has some variability which might be more related to precession rather than glacial/interglacial cycles. A precession forcing of dust input is also not unlikely given the precession imprint on the African Monsoonal system which determines the expansion and proliferation of dust from North Africa. As mentioned above it would help to plot the CWC abundances from Fig. 3 next to the Si/Al and Rb/Al curves to allow for a direct comparison. Because all parameters are measured on the same core, stratigraphic uncertainties do not play a role here. I would also recommend to expand the scale for both parameters to better visualize their variability. For this purpose the figure might be rotated by 90° (the other figures as well). The same issue arises also for the other environmental proxies discussed in relation to CWC abundance: please show them in direct comparison to the CWC variability.

R: As recommended by the Reviewer, we have revised the figures and now display the macrofaunal abundances next to the Si/Al ratio (and also next to the benthic foraminiferal abundances and other proxies used in the study). Following the comment made, we do not present the Rb/Al ratio in the revised version of the manuscript. Indeed, Rb/Al did not show any distinct fluctuations and was not useful to interpret macrofaunal and microfaunal distributions. Si/Al discussion is also kept at a minimum and only as a supporting proxy for macrofaunal and microfaunal distributions. The figures have been turned 90° and are now in landscape format.

2) The same lack of good correlation arises also for the discussed parameters sortable silt (bottom current speed) and planktic-benthic $d^{13}C$ gradient (vertical mixing). Even considering that the CWC mound itself might bias both proxies (as discussed in the text), a better tentative correlation to CWC abundances should be expected. I also wonder why the authors did not compute the d18O and d13C gradient and plot them next to CWC abundance. A decreased d18O gradient should also indicate a better vertical mixing by an increased gyre activity.

R: Following the recommendations made, we have now provided the d18O and d13C gradients and have plotted them next to macrofaunal abundances (Fig. 5). These gradients, noticeably d13C, would suggest stronger stratification during the last two glacial periods and a better mixing during interglacial periods. We have tried to better correlate the d18O and d13C gradients with sortable silt. However, these correlations (biplots) were not conclusive (possibly as a result of the low temporal resolution). Following this, we have removed the sortable silt proxy from the discussion as it would necessitate further refining (i.e. possibly increasing the sampling resolution and further improving the stratigraphy of the core by adding new coral U-Th ages). We only refer to the higher sortable silt values recorded at the end of interglacials (MIS 5 and MIS 7) as possibly inferring increased turbulence (section 5.2).

3) It seems that the best fit of CWC abundances is with benthic d18O and the presence/absence of certain foraminiferal species (e.g. *Bulimina* spp.) pointing at a major influence of sea level and bottom water oxygenation and/or nutrient availability. While I can follow the arguments for a stronger gyre activity during interglacials from a conceptual point of view, the relatively shallow water depth makes this site very sensitive to vertical shifts in water masses. When I understand correctly the authors infer from the presumed change in the d13C gradient between surface and bottom water that stratification would be the driving force behind d13C variability, but what if a different water mass is bathing the site, which might be a simple effect of glacial/interglacial sea level changes?

R: We agree with the point that the shallow depths of the mounds make them sensitive to glacial-interglacial sea level changes. However, our interpretation, based on the d13C and d18O gradients and the foraminiferal assemblages, is that the site is situated within the envelop of LIW during both interglacial and glacial periods due to the depth of the site and the current knowledge regarding the water mass profile at the location of BRI (see Figure 2). Indeed, MAW makes up more or less the top 200 m of the water column (Fig. 2; Katz, 1972). Hence, glacial sea levels may have brought the site (327 m depth) closer to the LIW-MAW interface (though not within it), though the weaker inflow of MAW at the Strait of Gibraltar needs also to be considered in the equation (regarding the position of the pycnoclyne). In addition, WMDW circulates below 600 m (Millot and Taupier-Letage, 2005), hence it is unlikely that BRI would have been situated within it's envelop. We do propose however that LIW circulation was accentuated during glacials and that deep circulating waters (WMDW) may have contributed to LIW during these periods. This would explain increased stratification during glacials.

4) One way to tackle the factors influencing CWC presence more objectively might be by including statistical methods such as simple correlation coefficient or via conducting a PCA.

R: We have applied such methods (PCA and RDA) to the dataset presented in this study but these were not conclusive (i.e. the methods did not allow to discriminate between different variables and did not demonstrate any clear correlations). We believe that this can be explained by the relative low temporal resolution at which core MD13-3462G was sampled (assuming a constant sedimentation rate: one sample every 3200 years). This resolution cannot be improved since samples were taken every 5 cm (NB: it is complicated to sample for benthic foraminifera at a lower spatial resolution given the nature of the core). Thus, the decision was made not to include any multivariate analysis.

However, to improve the visibility of the correlations between key benthic foraminifera species (Buliminids and *U. mediterranea*) and coral abundance, we have added 3 scatter plots to Figure 8 (*B. striata, B. marginata, U mediterranea* vs. coral abundance). These simple scatter plots illustrate the correlation between high organic matter indicator species and high coral abundances (in turn related to interglacial periods, see Fig. 8).

5) The authors should reference the dissertation by Thomas Krengel (2020) "550,000 years of marine climate variability in the western Mediterranean Sea revealed by cold-water corals" (https://archiv.ub.uni-heidelberg.de/volltextserver/27990/), in which he investigated CWC mound growth at Brittlestar Ridge based on a MeBo drill core, covering the past ~700 ka. In his thesis he basically conveys a similar idea as the paper by Fentimen et al., i.e. that an increased interglacial hydrological cycle on the continent invigorates CWC proliferation due to enhanced input of nutrients. Hence, including the data provided by Krengel (2020) and referencing this study is pivotal.

R: We know reference the PhD thesis of Thomas Krengel (2020) and introduce his observations in the Introduction (following also the comments of the second reviewer). Moreover, we now discuss both our observations and the ones made by Thomas Krengel in the Discussion section. In the revised version, we strive to focus better on Mediterranean cold-water coral mounds and have shortened the section comparing our observations to NE Atlantic mounds. We also better include the papers by

Corbera et al. (2021; 2022) about the long-term build-up of the Cabliers Coral Mound Province and the Tunisian Coral Mound Province.

6) The authors devote quite some effort in both introduction and discussion to compare CWC in the Alboran Sea with the North Atlantic CWC provinces. The comparison to North Atlantic CWC is listed as the major research aims (l. 93). To me becomes not clear why they compare their record specifically with those in the North Atlantic which is a hydrographic distinctly different basin as the Alboran Sea. What insights should be gained, or hypothesis tested? The authors should much more focus the respective parts of the discussion on similarities and divergences of the environmental factors inferred to have been instrumental for CWC growth (or its suppression)

R: We have refocused the manuscript (goals and discussion) in the way suggested by the reviewer (which also follows the suggestions made by Reviewer 2).

Response to reviewer Report n°2

We thank the reviewer for his constructive comments. A point by point response to each comment is provided below (responses in blue font). Overall, all recommendations have been followed except moving the first part of the results (chronostratigraphy) to the beginning of the discussion. The introduction has been considerably reworked as a result of the recommendations: we now better introduce the current knowledge about long-term cold-water coral mound build-up in the Mediterranean (Krengel, 2020; Corbera et al., 2021; 2022), i.e. oldest coral occurrences, mound aggradation rates. The part introducing Atlantic coral mounds has been reduced. The discussion has also been reworked following the comments made. We now better discuss our observations in comparison to the observations previously made in the East Melilla Coral Province (Krengel, 2020) and the Cabliers and Tunisian coral mounds (Corbera et al., 2021; 2022). We have overall strived to make the message of the manuscript clearer and to boil down the discussion to its most important elements. The abstract and conclusion have been fully modified accordingly.

General comments

The work by Fentimen and co-authors is dealing with the already well characterized Brittlestar Ridge I, within the East Melilla Coral Province. The authors analysed one sediment core, collected from the later mound to improve the information on this mound's development during the Middle and Late Pleistocene in relation to palaeo-environmental variations. The authors based their discussion and conclusions on a combined chronostratotography of ^{14}C, U-Th ages, $d^{18}O$ results and the environmental interpretations extracted from the relative abundances of foraminifera, macrofauna and chemical proxies of terrigenous input.

This work presents novel and remarkable results in the area, providing the first evidence of coral growth in the Alboran Sea during the last two glacial periods, but especially during the MIS 4-2. These results contrast with the current knowledge of coral mound development in the Alboran Sea and in the Brittlestar Ridge 1 itself, where mound formation was mainly confined to temperate interstadials and interglacial periods. Although, the current paper shows coral growth both during glacial and interglacial periods, the authors suggest that coral mound formation in the Brittlestar Ridge 1 is linked to increased sediment inputs, enhanced organic matter flux and high food availability at the seafloor during interglacial periods.

Although the results acquired and methods followed comply with good scientific standards, I believe that there is considerable room for improvement in the introduction and discussion sections, as the authors disregard a part of the studies on coral mounds and CWC assemblages carried out in the Mediterranean Sea. While the paper focuses on a Mediterranean coral mound, the authors give much more detailed information on the environmental variables and processes controlling coral mound formation in the North Atlantic. For instance, there are no comments on the current state and species dominance of CWC assemblages in the Mediterranean Sea. In a similar way, although there is relevant information on coral mound development in the Alboran sea going back >300 kyr, the authors do not comment on it. Although one of these studies is an unpublished PhD thesis (by Thomas Krengel), I believe the authors should still comment on the conclusions acquired there, especially because the thesis is built around MeBo cores collected in the Brittlestar Ridge 1 itself. In addition, the studies from the Cabliers Coral Mound Province, showing the only currently thriving coral reefs in the Alboran Sea and their development since the Middle Pleistocene are also ignored. The authors should add all this information and provide numbers on aggradation rates and oldest dated corals in the Mediterranean Sea to set a good ground for a more relevant discussion of their results.

R: We have considerably reworked the introduction to take into account these remarks (see tracked version of revised version of the manuscript). We now do not insist on North Atlantic coral mounds but spend more time introducing Mediterranean coral mounds (their current states and their average aggradation rates and development phases). This introduction supports the revised discussion (noticeably the new section 5.1).

With regards to the discussion I recommend moving the first part of the results where the authors discuss the chronostratigraphy of the core and set it as the first section of the discussion. I suggest to also add here or just before chapter 5.3 all the information from section 5.1.3 together with a more in-depth discussion of the paper's results, in terms of coral growth periods and mound aggradation rates with those previously published for other coral mounds of the Mediterranean Sea (e.g. Fink et al., 2013; Stalder et al., 2015, 2018; Wang et al., 2019; Krengel, 2019; Corbera et al., 2021, 2022). And then continue the discussion as it is, with environmental setting during interglacials and glacials and the final section comparing with North Atlantic coral mounds. Regarding the later chapter, I recommend to shorten this section avoiding too many details on the specific environmental controls for the Atlantic coral mound provinces as they are located in very contrasting environmental settings.*

R: We have preferred not to follow the recommendation to move the first part of the results to the top of the discussion section. We understand the point raised by the reviewer, we however prefer to stick to the original layout so to discuss the following results relative to age and not depth (which would be necessary if we moved the mentioned section to the first part of the discussion). We have moved the text from section 5.1.3 to the first part of the discussion as recommended (see revised version of the ms) and followed the recommendation to build a new section at the beginning of the discussion to better discuss aggradation rates relative to the recent observations made by Corbera et al. (2021; 2022) and Krengel (2020).

Nonetheless, as this study provides a significant contribution to the current knowledge on coral mound formation in the Mediterranean Sea, I recommend to publish this manuscript subject to moderate revisions of the introduction and discussion chapters as commented above, and after addressing the specific comments below, which will hopefully help to improve the quality of the manuscript.

Specific comments

ABSTRACT

Line 22: Please change "300 ky" for "300 kyr", and revise throughout the text. The abbreviation "kyr" should be used when the authors refer to a period of time and "ka BP" when they refer to a specific moment in the past.

R: Correction made

Line 26: Coral mounds in the Gulf of Cadiz develop during glacial periods, while Norwegian and Irish mounds develop during interglacials, both of them being in the Northeast Atlantic. On another note, I believe it would be more relevant to the work presented here to compare the results acquired with those already published for other mounds in the Mediterranean Sea. Especially, because this is the first time that such amount of coral ages have been obtained in an Alboran Sea coral mound for the last glacial period (MIS 2-4).

R: The abstract has been reworked to better reflect the changes made to the discussion. We now insist on the last glacial ages observed in core MD13-3462G.

Lines 28: Saying that coral mound build-up in the Southeast Alboran Sea presents aggradation rates < 10 cm kyr^{-1} is not accurate. What about the high aggradation rates of the brittlestar ridges and Cabliers mounds during the Early Holocene? The Dragon mound and Cabliers also display ARs higher than 10 cm/kyr in the MIS 5. See Thomas Krengel PhD Thesis (2019; available online), Wang et al. (2019) and Corbera et al. (2021). In fact, the authors comment on such aggradation rates (75 to 420 cm/kyr) in line 83.

R: The abstract has been reworked in the same way as the text; we now insist that our observations only reflect conditions where the core was recovered (northern part of BRI). We also strive to point out the striking discrepancies with the core studied by Krengel (2020) which was retrieved only ca. 1 km south of core MD13-3462G. These observations point to local differences in mound build-up across the ridge.

INTRODUCTION

Line 38: Please change “common on Earth” by “widespread in the world ocean”

R: Correction made

Lines 44-46: Please add that such oceanographic features also accumulate particulate organic matter due to their sharp density gradient.

R: This point has been added to the sentence

Line 50: Please add the reference by Hebbeln et al., 2016, dealing with the baffling capacity of coral frameworks and the role of the hemipelagic sediments in stabilising the coral frameworks.

R: Reference added

Line 53: The study focuses in the Alboran Sea, where there are already many examples of aggradation rates equal or even higher than those of the porcupine Seabight. Hence, I believe it would be more appropriate to use one of those mound aggradation rates as an example.

R: We now reference instead to the East Melilla Coral Province aggradation rates calculated by Fink et al. (2013).

Line 54-56: Please change the structure of the sentence for an easier lecture. For instance: “As such, and in spite of mound formation being generally discontinuous, coral mounds…”

R: This sentence has been reworked.

Lines 60-75: Too much detail on the controls of CWC mound development for each "province" of the Atlantic. Instead, I would state the main development periods and give and overall explanation on the environmental controls at the end of the paragraph and explain better the environmental variables driving mound development in the Mediterranean Sea.

R: Following the recommendations made this section has been shortened. The following paragraph covering the development of Mediterranean mounds has been on the over hand strengthened as suggested.

Lines 72-75: Gulf of Cadiz coral mounds only develop during glacials. Check Wienberg et al. (2010). It displays one coral age in the MIS5 but this cannot be attributed to mound formation.

R: This has been corrected, the sentence has been deleted.

Line 78: Please change "concentrated in the East Alboran Sea" for "concentrated in the Alboran Sea" as Lo Iacono et al. (2014) is not really East Alboran. Nonetheless, there are also many documented mounds in the Corsica channel (Remia and Taviani 2005; Angeletti et al., 2020), south of Pantelleria (strait of sicily; Martorelli et al., 2011) and on the Tunisian Plateau (Camafort et al., 2020; Corbera et al., 2022), that should not be disregarded.

R: Mention of these previously omitted coral mounds has been added.

Lines 77-88: Although the manuscript focuses on Mediterranean CWC mounds, the introduction gives more detailed information on the environmental controls for the Atlantic coral mounds than the Mediterranean Sea ones. Here it focusses only on information from the Melilla Mound Field and just during the MIS1. I recommend to add the results from Thomas Krengel PhD thesis and Corbera et al. (2021, 2022), which expands the information to other areas of the Alboran and Mediterranean Sea, and to the Late and Middle Pleistocene. It should also be stated somewhere in this paragraph that most of the Mediterranean coral mounds are in a stagnation stage, except for the North Cabliers Coral Mounds (Corbera et al., 2019) and the Corsica Mounds (Angeletti et al., 2020).

R: The introduction has been reworked in such a way to include the results from Krengel (2020) and Corbera et al. (2021; 2022). We now strive to focus on introducing the Late and Middle Pleistocene history of coral mound build-up in the western Mediterranean. We have added that corals are absent or scarce on most Mediterranean coral mounds (except for the North Cabliers Coral Mounds, the Santa Maria di Leuca mounds, and the Corsica Channel mounds – see Line 80 in the revised version of the manuscript).

Line 91: This is not entirely true. Although the data has not been published in a peer reviewed journal there is information on long-term mound development and environmental forcing from the Brittlestar Ridge 1 and Dragon Mound (both from the EMCP) that goes back > 300 kyr.

R: This sentence has been corrected, we know state that: "Although the development of coral communities at the EMCP during the last 30 kyr is well documented and that novel long-term records are emerging from the western Mediterranean Sea (Krengel, 2020; Corbera et al., 2021; 2022), the long-term environmental forcing affecting the EMCP remains poorly constrained".

Line 93: Considering that this paper shows for the first time the presence of coral proliferation in the Alboran Sea during the last glacial period (MIS 2-4), I believe that instead of comparing the development of BR1 with Atlantic mounds, the paper would benefit and become stronger from comparing its results with the development of other Mediterranean Coral Mounds. Especially, with results acquired from gravity cores previously collected from the BR1 itself, where no glacial ages have been reported before.

R: Indeed, we now concentrate more on comparing our results to the findings of Corbera et al. (2021; 2022) and Krengel (2020). This also matches the comments made by Reviewer 1. The sentence has been modified accordingly, we have now written: "2) to assess long-term CWC mound formation in the area and compare it to Mediterranean and North Atlantic counterparts".

Do the authors have any hypotheses on why this part of the mound presents limited coral growth during the last glacial, whereas less than 2 km to the south west and around the same depth (i.e. ~330 m) no coral growth has been observed during this period?

R: This is indeed an intriguing and interesting point. We are quite convinced that coral mound build-up on BRI is particularly heterogeneous (temporally and spatially). This is observable over the last 20 kyr (we have submitted a manuscript to Marine Geology illustrating this heterogeneity by comparing 3 gravity cores taken along the ridge: MD13-3455G, MD13-3459G and MD13-3462G). We emit the hypothesis that this heterogeneity in mound build-up is linked to local differences in bottom currents, the southern tip profiting from increased turbulence whereas the areas closer to the Provençaux Bank would be more sheltered (as evidenced by the moat at the tip of the mound). However, this is only a hypothesis and needs to be checked through further investigations. In this sense, it would be interesting to compare and correlate, by using similar proxies (possibly stables isotopes on benthic and planktonic foraminifera), the BRI core studied by Krengel (2020) and the one investigated here.

STUDY AREA

Lines 98-100: Please move these two sentences to the start of the paragraph.

R: The two first sentences of this paragraph have been removed as we believe that they are not essential for the study. This section is more concise and to the point when starting by the two sentences cited above (which is now the case following the removal of the first two sentences).

Line 105: Please change to “The ridges are 3 to 20 km in length and vary in height from 50 to 150 m…”

R: Change made

Lines 106-108: Please change sentence to “These mounds are characterized by dead coral framework with some living corals at their summits…”

R: Correction made

Line 136-138: Please modify to “It is important to note that, as it moves towards the west, the LIW receives contributions from other water masses and hence, its characteristics gradually change as it gets closer to the Strait of Gibraltar (Millot, 2013)”.

R: Correction made

Line 139: What is this water mass? Which are the differences in physicochemical conditions with the LIW? There is barely more than one paper where someone mentions this water mass. Also Ercilla et al. (2016) is not a physical oceanography paper, but an study dealing with contourites.

R: As this point between ShW and LIW is problematic it has been deleted. The references to ShW, which is indeed not covered by many papers, have been taken out or replaced by reference to LIW. In hindsight we recognize that the double mention of both LIW and ShW isn’t clear. Figures have also been changed accordingly (Figs. 2 and 10).

Line 144: Please find another REF, as this is not a physical oceanography paper.

R: The reference has been replaced by: “Millot, C., Tapier-Letage, I.: Circulation in the Mediterranean Sea, in: The Handbook of Environmental Chemistry: The Mediterranean Sea (HEC5, volume 5k), published by: Saliot, A., Springer-Verlag Berlin Heidelberg, 29-66, 2005.”

MATERIAL AND METHODS

Line 189: Please modify to “*MaterialStatistics*”

R: Modification made

Lines 200-201: Please change to “background sediment (i.e. aluminum)”

R: Change made

Lines 208-211: Please modify to "Since the Saharan region, which is the dominant source of aeolian dust in the Mediterranean Sea, is essentially composed of silicates with high quartz content (REFS) and considering silica is rare in the Alboran Sea sediments (REF), the Si/Al ratio has been used to track variations in terrestrial inputs"

R: Correction made

Line 215: Please modify to "Rb/Al ratios provide robust and valuable records of terrestrial input".

R: Correction made. Following the comments made by Reviewer 1, we have removed the Rb/Al record from the manuscript and that stuck solely to the Si/Al ratio.

Line 224: Sometimes written as "grain-size" and others as "grain size". Please revise and keep consistency throughout the manuscript.

R: Has been consistently changed to "grain size"

Line 279: Please modify to *L. lobatula*, as this species has already been mentioned in the manuscript (line 261)

R: Correction made

RESULTS

Lines 309-316: These are not really results and thus, I recommend to move this paragraph to the discussion section. Maybe just at the start of the discussion, before talking about the environmental setting

R: We agree that this section can be seen as a "pre-discussion" and could be placed at the beginning of the discussion. However, doing this would lead to describing all the results relative to depth and not age (since the stratigraphy would not have yet been introduced). We have considered doing this, also in a previous version of the manuscript, but we do prefer describing the results relative to age since this is much more meaningful than depth. Following this argument we have decided to leave where it currently is (i.e. at the beginning of the results section).

Line 323: What test is this package doing? A linear regression of the data points? Please specify and in any case the authors should also show the p-value of the test.

R: Correction made

Line 340: By correlation coefficient are you referring to R^2? If so please display it in a consistent way throughout the manuscript and add the p-value.

R: Correction made

Line 345: Add p-values please.

R: Correction made

Line 363: Please modify "surrounded by" for "embedded in"

R: Correction made

Line 368: Please change "of essentially terrestrial.." to "essentially of terrestrial…"

R: Correction made

Lines 387-388: Please change to "G. bulloides goes from -2.2 ‰ at 12 cm to -0.5 ‰ at 292 cm, whereas that of the benthic *L. lobatula* goes from 0.9 ‰ at 872 cm to 1.8 ‰ at 362 (Fig. 5)."

R: Change made

Line 414: If you specify the core depth for the MIS 3, please do the same for the MIS 6.

R: Core depth has been specified for MIS 6

Line 419: Please change to "(Figs 3, 8)".

R: Change made

Line 419-421: Please modify to "Although the dominant coral species in the core is the scleractinian *D. pertusum*, in the upper 20 cm this species is replaced by *M. oculata* (Figs. 3, 8)"

R: Change made

Line 422: Please specify approx/average coral contents for glacial and interglacial periods.

R: Average values are now given (22.2 vol% during interglacials against 14.5 vol% during glacials).

Line 426: Please explain with a bit more detail the changes in aggradation rates from MIS 6 to MIS 1.

R: Additional detail has been added in the following sentence: "In contrast, lower Aggradation rates of mound sediments characterize MIS 5 (ca. 2 cm/kyr) together with MIS 1, 2 and 4 (ca. 4 cm/kyr) (Fig. 4B)".

Lines 438-441: Please change to "*G. vitreus, T. retusa* and *B. pectunculoides*" as they have already been fully mentioned at the beginning of this paragraph.

R: Changes made

Lines 460-461: Please change to *D. coronata* and *L. lobatula*.

R: Changes made

Line 470: Please write full genus for "*B. aculeata*" as it is the first time this species is mentioned

R: Correction made

Line 475: Please modify to "*U. mediterranea*" it has already been mentioned before.

R: Correction made

DISCUSSION

Line 486: I recommend to add the discussion on the chronostratigraphy construction as a general discussion at the beginning of this section.

R: This recommendation has not been followed (see response to comment ***Lines 309-315***).

Lines 493-496, 500-502, 505-508: The authors talk about organic matter flux in all these sentences. Such repetition is a bit confusing for the reader and it makes it difficult to follow the authors argumentation. Please merge these statements in a couple of sentences prior to lines 497-499.

R: Merging done

Line 513: Please change "Overall" to "Indeed, the.."

R: Changes made

Line 516: Please modify to "towards the end of such periods"

R: Changes made

Lines 537-539: This is a bit contradictory as both this mound (i.e. BRI), the dragon mound (also EMCP), West Melilla and southern Cabliers show aggradation rates > 20 cm/kyr during interglacial periods. Please check Krengel PhD Thesis, Wang et al. (2019), Corbera et al. (2021). In fact, there are papers of CWC reefs thriving under hypoxia off Angola and Namibia. Check Tamborrino et al. (2019) and Hebbeln et al. (2020).

The present paper also shows higher coral content during the MIS 5, indicative of a more prolific coral growth/mound formation period (as observed in Fig. 3 and mentioned by the authors in lines 568-570). However, the lack of enough coral ages (n=3) does not allow to properly constrain the actual aggradation rate during this interglacial (encompassing > 1m of core).

R: We agree with the comment made and have reworked the text accordingly.

Line 541: CWCs are not pelagic ecosystems. This should be changed to epibenthic.

R: This has been deleted after the comment made above (about lines 537-539).

Line 567: Please change title to "Variability of cold-water coral mound formation in the Alboran Sea"

R: This section has been removed (parts of the contents have been moved to section 5.3 – which has also been reworked).

Lines 573-577: Here the authors compare the youngest age of different cores from the EMCP and the WMCP. Please discuss with more detail how the aggradation rates observed in this core compare to those of the EMCP during the MIS1 (Fink et al., 2013; Stalder et al., 2015, 2018 and other refs) and previous MIS (Krengel PhD thesis results). Also compare with detail with other nearby provinces such as the WMCP (Want et al., 2019) and the Cabliers coral mounds (Corbera et al., 2021), which has information on coral mound development going back to > 300 ka BP.

R: We now compare our observations to those made on other CWC provinces (Cabliers, Tunisian and WMCP) in section 5.1. This also follows the general comments made by both reviewers.

Line 580: These are not papers assessing the resilience of *D. pertusum* or *M. oculata* to env. conditions... Add examples of experimental studies and maybe also the review by Wienberg and Titshack (2015), where they show the distribution of these two species along the Atlantic with respect to env. variables.

R: We have now deleted this section as we have strived to focus on the novelties of the work (glacial occurrence of CWCs, low mound aggradation rates, see new section 5.1). The *M. oculate / L. pertusa* shift has already been well covered (and is discussed in the manuscript we have submitted to Marine Geology).

Lines 582-583: Also consistent with most of the currently living CWC assemblages in the MED, which are mainly dominated by *M. oculata* (Orejas et al., 2009; Gori et al., 2013; Taviani et al., 2017; Angeletti et al., 2020). You have many examples, including living reefs in the Eastern Alboran Sea (Cabliers; Corbera et al., 2019). Please add all this information.

R: Section deleted (see response to comment above).

Lines 608-610: Please modify to "Higher bryozoan content during glacials at BRI is in tune with previous observations made at the Great Australian Bight, where bryozoan proliferation during glacial periods has been able to promote the formation of mounds (James et al., 2000; Holbourn et al., 2002)." Are they looking at the same species? If not, I would not compare the env. conditions but just the fact that bryozoans can form mounds.

R: The two studies cited do not observe the same species distribution. We agree with the change and have replaced the sentence in the text.

Lines 610-611: Please remove "Conversely, higher temperatures and downwelling during interglacials halted bryozoan extension at the Great Australian Bight (James et al., 2000; Holbourn et al., 2002)."

R: We have decided to leave this sentence since we believe that the parallel with the palaeoenvironmental conditions observed at BRI is interesting.

Line 615: Please modify to "bryozoans, due to the high concentration"

R: Correction made

Line 618: Please change to "*G. vitreus*"

R: Correction made

Lines 633-637: Please check and rephrase this sentence, it is hard to follow.

R: We have reworked the two sentences and have deleted the mention of the study of Milker et al (2009): "following Milker et al. (2009), high abundances of *C. laevigata* could be related to the presence of fine-grained material in the western Mediterranean". Indeed, we believe that this sentence was not useful and over detailed for the points we were addressing in the discussion.

Lines 680-682: Please modify for "The last glacial shows a strong variability in macrofaunal and benthic foraminiferal assemblages, whereas maximum coral content is reached during the MIS 3 (Fig. 3)".

R: We have preferred not use the conjunction "whereas" since it is used to indicate a contrast in ideas or two observations and that is not the case in this sentence. We have linked the sentence with the following sentence mentioning foraminiferal abundances. The sentence now reads: "Maximum coral content is reached during MIS 3 (Fig. 3) and is associated to higher numbers of *G. subglobosa* and *C. laevigata* (Fig. 8)".

Lines 682-683: I think using Rb/Al as an explanatory variable here is an overinterpretation of the data as this proxy barely changes along the core.

R: We no longer mention and use Rb/Al in the manuscript. This part of the sentence has hence been deleted.

Lines 683-685: Didn't the authors say river run-off was considerably reduced during glacials (Lines 622-628)? Please revise and clarify. In addition, the aggradation rates of 10 cm/kyr cannot be considered as fast mound formation rates, when there are many records of aggradation rates >100 cm/kyr in the Alboran Sea (Please check the literature).

R: Indeed, the sentence is awkwardly put. Based on our observation, noticeably the benthic foraminiferal assemblages, we do believe that river run-off was reduced during glacial periods. The benthic foraminiferal assemblages observed during MIS 3 (noticeably the high numbers of *G. subglobosa* and *C. laevigata*) may however indicate that this period was influenced by periods of increased organic matter import. We have now rephrased the text, it now reads: "These observations suggest that corals and the benthic foraminiferal community positively responded to short phases of increased surface productivity during MIS 3". We remain purposely unprecise concerning the mechanisms behind this increased surface productivity since we cannot emit a solid hypothesis with the data at hand.

Line 693: What is the reason to compare the data acquired here with North Atlantic coral mounds and not the Mediterranean Sea? As I have already commented in this revision, this paper would benefit from a better comparison with Mediterranean mounds, as the environmental variables and climatic events affecting their development will be more similar (if not the same) and relevant for discussion.

R: We have reworked the discussion to follow this recommendation (see section 5.3).

Lines 697-698: The results obtained here show sporadic coral growth during interglacial and glacial periods with the first ages ever acquired in the Alboran Sea during the last glacial. This is very interesting, but the aggradation rates showed are not high enough to be attributed to mound formation (check Frank et al., 2009 where the authors set a threshold of 15 cm/kyr to consider that there is coral driven mound formation).

R: Aggradation rates are now discussed in greater detail in section 5.1 and include the above-mentioned observation of Frank et al. (2009). Since these are key observations, we have followed the recommendation to move them up to the first section of the discussion (see revised version of the manuscript).

Lines 729-733: Again, what about the information in Krengel's PhD thesis. He obtained ARs up to 83 cm/kyr during interglacial periods and no growth during glacials. I also see a lack of discussion in the potential effect of sapropel events on alboran sea coral mound development as discussed by Krengel's PhD and Corbera et al (2021).

R: The observations made by Krengel (2020) are now discussed in section 5.1. The potential effect of sapropel events has been integrated to section 5.2.

Line 735: This is not true. As already commented above, there are records of significant coral mound formation during interglacials in this mound. Limited mound formation of the BR1 is just observed in this core/region of the BR1. Please discuss this in more detail.

R: This part has been corrected and discussed in section 5.1.

Lines 739-742: Exactly! In the sector of the BR1 from where this core was collected, the corals grew under stressful conditions. However, there are parts of the discussion where this message is not clear. Please revise and modify accordingly.

R: We have strived to make this clearer in the discussion. We state it now at the beginning of the discussion (first paragraph of the discussion). We have gone through making sure the matter is clearer (also changes have been made to the abstract and conclusion accordingly).

Line 723-749: What about the Gulf of Cadiz coral mounds?

R: The Gulf of Cadiz coral mounds are now discussed in section 5.1.2 (glacial mound build-up : a recurrent Mediterranean trend?).

CONCLUSIONS

Line 754: This is not entirely accurate. Please replace “Cold-water coral mound build-up takes place during both interglacial and glacial periods” for something in the lines of “Although previous results indicated that CWC growth and mound formation in the Alboran

Sea occurred just during interglacials, this study shows that CWCs did not disappear from the BR1 during the last glacial periods."

R: The conclusion has been thoroughly reworked to reflect changes made in the discussion.

Line 757: Please modify for "conditions in this sector of the Brittlestar Ridge I".

R: Correction made

Line 763: Please add "and surface productivity" after terrestrial input

R: Correction made

REFERENCES

Line 1093: The link provided here does not work, please revise and try to provide a working link.

R: Correction made

FIGURES

Figure 1: Please add the locations of the West Melilla Coral Mound Province and the Cabliers Coral Mound Province in panel B. I also believe that in panel C the authors should add the location of other gravity and/or MeBo cores acquired from the Brittlestar ridge 1. Check previous literature and Thomas Krengel PhD thesis. The red box just comprises the BRI and not the entire EMCP, please revise and modify.

R: Other gravity cores taken in the area have been added to panel C. The locations of the East Melilla Coral Province, the West Melilla Coral Province and the Cabliers Coral Mound Province have been added to panel B. We have also added the location of Dragon Mound and have also revised the name of the red box. Other cores acquired on BRI have been added to panel C.

Figure 4: Please add p-values to all R^2 in the figure caption. In line 334 change "high" to "higher"

R: Correction made. "High" has been changed to higher.

Figure 7: Add p-values please.

Figure 10: Still not convinced about the existence of the Shelf Water as an independent water mass different of the LIW.

R: The mention of Shelf Water has been removed in the text and figures. Moreover the figure has been slightly modified (the seafloor diagrams) to better reflect the conclusions drawn in the manuscript (bryozoan and coral cover during glacials and interglacials).

Technical corrections

Line 130: Please change "East to West" to lower case.

R: Correction made

Line 131: Please add a coma after Sicily.

R: Correction made

Lines 161-163: Please remove space in between sentence.

R: Correction made

Line 247: Please change from "(percentages)" to "(%)".

R: Correction made

Line 350: Add a coma after "ages"

R: Correction made

Line 373: Please remove the "is" after SS.

R: Correction made

Line 374: Please modify "MIS 6 were SS…" for "MIS 6, when SS…"

R: Correction made

Figure 2: Please change the label "Oxygen (µmol.kg^{-1})" to "Oxygen (µmol kg^{-1})". Please modify this also in the figure caption.

R: Corrections made

Figure 3: Please revise and modify the oldest ^{14}C age (i.e. 30.1 ka BP), as in table 1 it says 30.9 ka BP.

R: Changes made throughout figures to 30.9 ka BP.