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# *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.

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Reply to Anonymous Referee #1

Manuscript title: The influence of Atlantic climate variability on the long-term development of Mediterranean cold-water coral mounds (Alboran Sea, Melilla Mound Field) submitted to Climate of the Past response by: Robin Fentimen et al.

In the following document, the responses to the comments made by Anonymous Referee #1 are addressed one by one.



Coral ages and stratigraphic interpretation

Comment Referee #1: In most cases, coral mound aggradation is intermittent, as also mentioned in this manuscript (lines 55 & 275, incl. some of the relevant references). That means, that short pulses of high mound aggradation (very vivid reef development) are interrupted by no growth periods (or maybe the presence of some individual corals, but no reefs) or even erosion, often resulting in a hiatus between core sections representing the vivid reef stages. This is also the case for Brittlestar Ridge (BR) 1, the study site for this manuscript, as has been shown for the last _14 kyr by several studies (Fink et al. 2013) including also work coming from the same group as this study (Stalder et al., 2015, 2018). Now, this common feature of coral mounds also applicable for – at least – the upper part of the BR 1 record, has been ignored in this study – it has not even been discussed with respect to the core presented here. In contrast, for core sections between individual coral ages, the stratigraphic interpretation is based on linear interpolation assuming that the core represents a continuous record. I strongly doubt the validity of this approach on this long coral mound core and to my knowledge there is no (or hardly any) long coral mound core reported that provides a continuous record.

Response: We are well aware that coral mound aggradation at Brittlestar Ridge 1 is intermittent, as supported by the publications mentioned by Reviewer #1. We also fully agree that there are very few, if any, continuous records provided by coral mounds (especially covering 300 ky). However, it is not correct to state that we have ignored this common feature, as written at Line 275 and 276: "In contrast, constructing a continuous age model based on stable isotope records is generally considered untrustworthy for cores collected from coral mounds since sedimentation is intermittent (Dorschel et al., 2005)". We have, at no point in this study, considered or hinted that the core represents a continuous record. We are well aware that the record is discontinuous, hence the decision to plot the different sedimentological, micropaleontological and geochemical records against depth, and not against an age model (see for example Fig. 3). As described in section 4.1 (Lines 264 to 292), corals and foraminifera were selected at major sedimentological boundaries (clear facies changes, e.g transition from a Lophelia pertusa (coral) to a Buskea dichotoma (bryozoan) horizon). In conjunction with the stable oxygen isotope record, we then defined the chronology and limited important time intervals (interglacial vs. glacial periods). We do not assume that the record is continuous between two coral ages; we simply isolate and define key time intervals corresponding to Marine Isotope Stages in order to understand the major environmental changes having affected Brittlestar Ridge 1 (this thanks to the wide variety of proxies used). It has to be pointed out here that the core is only 926 cm long, and covers ca. 300 ky. Thus, it presents a very condensed record, nothing like the cores described in the studies by Fink et al. (2013), Stalder et al. (2015, 2018) or Fentimen et al. (2020) which cover only the last 15 ky (for core lengths ranging from 350 to 490 cm). In comparison, the record presented here is, in this sense, more comparable to core GeoB 6730-1 taken from the Propeller Mound region (Northeast Atlantic) which is 360 cm long and covers ca. 207 ky (Dorschel et al., 2005; Rüggeberg et al., 2007). Thus, we followed a similar approach in order to define the stratigraphy as the one used by Rüggeberg et al. (2007), i.e. plotting the different datasets against depth and not age, and being aware of the fact that the core section can be discontinuous. We can add a sentence to state more clearly that we do not consider the record to be continuous.

Comment Referee #1: Actually, the interpretation provided here results in the observation that the by far highest observed mound aggradation rates in this record of 9.1 cm kyr-1 are indicated for MIS 4, which basically is in contrast to the main conclusion of the authors that the corals preferentially lived during interglacials.

Response: We do not agree with Reviewer #1 on this point. Highest mound aggradation rates (which are, as correctly pointed by Reviewer #1, still comparatively very low) during MIS 4 are not incompatible with the preference of corals for interglacial periods. In core MD13-3462G, the bryozoan B. dichotoma appears to play an equally important role on mound build-up as cold-water corals (see macrofaunal quantifications, Figure



3). This is developed in section 5.4.2 (Lines 654 to 680). This is one main conclusion of this study, that the bryozoan B. dichotoma plays an important role on mound development (when considering mound development over the last 300 ky, not only the last deglacial). Thus, at site MD13-3462G, mound aggradation does not go hand in hand with coral reef aggradation.

Comment Referee #1: On the other hand, this high aggradation rate for MIS 4 is not backed up by any dating referring to MIS 4; it is only based on linear interpolation. Earlier reported mound aggradation rates for BR 1 linked to well-established reefs in the Deglacial and the Holocene reach up to >400 cm kyr-1 (Fink et al., 2013; Stalder et al., 2015; Wienberg 2019). And - even for periods with less well established reefs in the mid- and Late Holocene, mound aggradation rates are in the decimeter kyr-1 range. Thus, most likely, also the record presented here by Fentimen et al., would reveal a very different stratigraphic pattern with periods of high mound aggradation rates interrupted by hiatuses given that more effort would have been put into the dating of corals. This, definitely, would be needed, before this record is ready for publication.

Response: Indeed, only the beginning of MIS 4 is backed up by coral dating. The end of MIS 4 is identified thanks to the stable oxygen isotope record (transition from high to low values, see Figure 3 and section 4.1). As pointed out in section 4.1, coral ages at the upper and lower boundaries of coral build-up phases in core MD13 3462G (e.g. at 390 and 507 cm depth) correspond to changes in the stable oxygen isotope records, which in turn match the changes between Marine Isotope Stages (Lisiecki and Raymo, 2005). Thus, the significant change in the stable oxygen isotope record at 320 cm can be attributed to an important change, which we interpret as the end of MIS 4. This change is also observed in other proxies, such as benthic foraminiferal assemblages and the macrofaunal distribution. It is the combination of all these proxies, in addition to the stable oxygen isotope records, that allows us to interpret this boundary as the end of MIS 4. We believe that combining different proxies (stable oxygen isotope record, facies analyses, foraminiferal asssemblages, macrofaunal abundances) proves to be stronger than basing interpretations on one single dating (although we recognize that these are also necessary)

We agree that the addition of supplementary coral ages would reveal more and shorter periods of higher mound aggradation rates interrupted by hiatuses. However, the average mound aggradation rate for a given longer time period (for example MIS 5) would not be any different (it would just show a higher variability). Again, we believe that the time scale considered in this study (300 ky for a 920 cm long core), does not allow for the precise study of mound aggradation rates that Reviewer #1 suggests. This is why this manuscript concentrates rather on characterizing changes between interglacial and glacial periods and describing average mound aggradation rates for those longer time periods. Moreover, the Holocene and last interglacial are very reduced in core MD13-3462G compared to other published records (Fink et al., 2013; Stalder et al., 2015, 2018). This is a peculiarity of core MD13-3462G in comparison to other records that we are aware of (thus the decision to focus on older parts of the core).

We insist that calculating mound aggradation rates as precisely as suggested by Reviewer #1, and as done by previous studies focusing only on the last Deglacial and the Holocene (Fink et al., 2013; Stalder et al., 2015), is - in this case - impossible. However, the coral ages presented in this work, are sufficient to delimit interglacial and glacial periods, and hence to characterize the environment at Brittlestar Ridge I during these periods (which is the goal of the study).

Comment Referee #1: Reading the coral mound record Fentimen et al. define the major coral build-up phases based on highest coral contents in their core. A detailed analyses of coral distribution as well as coral fragment orientation in a well-dated core from BR 1 revealed that highest mound aggradation rate (_400 cm kyr-1) coincides with rather low coral contents with coral fragments often preserved in an upright position (Titschack et al., 2016). Basically, this setting is interpreted as reflecting the partly preserved, fast growing reef being quickly filled up with sediments. In contrast, densely packed corals (usually flat laying) in a sediment core are often interpreted to reflect a



coral rubble facies indicative of strongly reduced coral growth. In the core presented here, actually the highest aggradation rate in MIS 4 correlates with low coral contents . . . Thus, the basic assumption used here (high coral content = best developed reef) is not valid.

Response: To discuss the orientation of coral fragments (cfr. upright position or coral rubble) and to avoid any discussion in terms of coral rubble versus in-situ coral framework, we are adding the visualization of the CT-fragments bigger than 2 cm to the manuscript. We are aware of the fact that the highest cold-water coral content cannot be always interpreted as the best-developed reef. The reviewer wrongly states that the authors are making this assumption. Coral content has been compared with other microfaunal observations. Although coral content is varying in the studied core between glacials and interglacials, the overall mound aggradation rates are low (seeing the mound as a system, cfr. sediments + microfauna + faunal content). Moreover, mound aggradation rates are similar for glacials and interglacials (= one of the conclusions of the manuscript). So – indeed – mound aggradation rates and coral content are not comparable, as correctly stated by the reviewer and by Titschack et al., 2016.

This being said, the statement that the work of Titschack et al. (2016) evidences that highest mound aggradation rates coincide with rather low coral contents is disputable and might be dependent upon regional and temporal differences. The unit with the highest mean aggradation rate at BR 1 in the study of Titschack et al. (530 ky.cm-1; Unit B; Titschack et al., 2016) demonstrates in some parts of the section a coral content of up to 30 % (average ca. 10 %). In this study (core MD13-3462G), a coral content of up to 30 % is considerably higher to what is observed during MIS 4 or MIS 2 (maximum 5 to 10 %) and is actually close to the maximum coral content documented in our study during interglacials (Fig. 3). When coral content is as low as 5 % and when bryozoan content reaches over 60 % (for example during MIS 2), we believe that it is correct to say that bryozoans are thriving and corals are not.

Moreover, we believe that the comparison to the work of Titschack et al. (2016) is hindered by the time interval considered in their study in comparison to the one considered in this manuscript. Titschack et al. (2016) considered a 447 cm long core, covering the time interval between 11.2 and 9.8 ky, while this study is focusing on a 926 cm long, covering 300 ky and studying mainly the time interval between 14.3 and 180 ka.

Current reconstructions

Comment Referee #1: The authors use the sortable silt to infer past variations in current strength. This approach works very well in normal, current-controlled sediments. However, within a coral reef the current velocity is usually reduced compared to the coral-barren seabed. This effect is mentioned by the authors and their conclusion is that nevertheless relative variations in the sortable silt reflect relative variations in bottom current strength. However, this only would work out if the reef would be a constant feature. But the authors also conclude that reef growth was quite variable through time. Consequently, the changing structure of the reef (from a large complex reef to few coral colonies) has a strong effect of the deceleration of the ambient bottom currents and, thus, on the sortable silt signal. Thus, only when the authors would have a good proxy for the state of the reef (and this cannot be the coral content) and if they could estimate the state-dependent effect of the reef on the bottom currents, finally an interpretation of the sortable silt data in respect to changing bottom currents might become possible. As yet, it is not possible. The authors added Fig. 5 to show the very good correlation between SSmean and SS% testifying the importance of the sorting process due to currents. This is not in contradiction to what has been said above: simply the reef state is another factor (in addition to the ambient bottom current strength) that has an effect of the actual current strength controlling sediment deposition within the reef. Consequently, the SSmean of the sediments deposited within the reef is not controlled by ambient bottom currents alone. Furthermore, when interpreting the data, the authors refer to a glacial/interglacial pattern with low glacial SSmean data. When looking at Fig. 4 I cannot see such a pattern. There are low SSmean values in MIS 6, but MIS 8



and 4 show rather high values and MIS 2 displays the full range of high and low values.

Response: We agree and are well aware that the SS mean of the sediments is not controlled by ambient currents alone (see section 3.4, Lines 205 to 211). Numerous studies have shown that the coral framework results in a local reduction of current velocity (as stated by Reviewer #1). Thus, the current velocity calculated thanks to the SS mean is actually an underestimation of the ambient bottom current (this is shown by a number of studies also, e.g. Huvenne et al., 2009; Titschack et al., 2009; Fentimen et al., 2020). Taking this into account, it is possible to compare relatively reef build-up phases to phases without any or very few cold-water corals (for example between MIS 4 and 5 or MIS 2 and MIS 1, see Figure 3). We agree with Reviewer #1 that using the SS mean alone and in a core with little coral content variations (e.g. between 20 and 30 %) would require a good proxy for the state of the reef and the effect of the reef on bottom currents. However, in this manuscript we aim to compare coral build-up phases (for example MIS 5) with intervals when corals are absent or near to absent (e.g. MIS 4 and 2).

Moreover, in this study the SS mean is used as supporting information for interpretations essentially based on benthic foraminiferal assemblages and the macrofauna. Indeed, the benthic foraminiferal assemblages and the macrofauna are also a valuable proxy when reconstructing bottom current velocities (this is stated in section 3.4). For example, we utilize the abundance of the benthic foraminifera species Trifarina angulosa (Lines 604 to 611), Cassidulina laevigata (Lines 517 to 523) and the abundance of the bryozoan Buskea dichotoma and the brachiopod Gryphus vitreus (Lines 502 to 504, Line 539) to identify respectively increasing and decreasing bottom currents. Indeed, the infaunal dwelling Trifarina angulosa is widely documented to live in areas dominated by strong currents and to resist winnowing (Mackensen et al., 1995; Schönfeld, 2002; Margreth et al., 2009, references in the manuscript). The SS mean is not used alone and is in support of the other proxies used to reconstruct bottom currents. It is important to point out that the interpretations made in this manuscript are dominantly built around the information gathered thanks to the benthic foraminiferal assemblages (see Discussion section, e.g. Lines 389 to 444, 514 to 536). However, Reviewer #1 made no remarks or comments concerning interpretations related to benthic foraminiferal assemblages, although it is the most widely used proxy in this work and a solid dataset within a coral mound environment. We believe that this lack of consideration of benthic foraminiferal assemblages may have led to a wrong understanding of certain interpretations made in this manuscript. This manuscript presents for the first time a high resolution assessment of benthic foraminiferal assemblages at Brittlestar Ridge 1 (total number of samples: 92). In comparison, previously published work in the area only presented a reduced dataset (Stalder et al., 2015: 29 samples, Stalder et al., 2018: 38).

XRF data

Comment Referee #1: From the methodological point, it would be good to know, how the authors dealt with the effect of coral fragments on their element records. With a measurement taking every 5 mm, many of the individual measurements most likely will reflect the element composition within a single coral fragment. The authors refer to a post treatment of the data was carried out for data points affected by the uneven surface of the core, but what is with coral fragments being measured as part of the flat core surface?

Response: It is indeed not detailed in the manuscript how we dealt with the effect of coral fragments on elemental records. However, the authors are fully aware of this effect. The presence of aragonite and calcite is diluting the background sediments. Using ratios of element intensities instead of intensities of single elements, may account partly for those so-called dilution effects (f.e. amongst others Weltje et al., 2015; Weltje and Tallingii, 2008). Spot analyses on coral-skeletons are – indeed – explaining the much higher small-scaled peaks – especially - in coral-rich units. For this reason, authors were calculating the running mean (using the Loess smoothing in R) on the datasets reflecting the overall trends. To read more about the Loess smoothing in R,



please read for examples the following document: "W. S. Cleveland, E. Grosse and W. M. Shyu (1992): Local regression models. Chapter 8 of Statistical Models in S eds. J.M. Chambers and T.J. Hastie, Wadsworth & Brooks/Cole". For the background sediments, normalization with Al has been performed because it behaves conservatively.

Comment Referee #1: However, probably more importantly here is the interpretation of the data. To be honest, the ups and downs in the element ratio curves interpreted by the authors are not obvious to me. Instead, it reads as first there was the idea about the meaning of the data and then the data were interpreted accordingly. For instance, the authors refer to an overall increased fluvial and reduced aeolian input during interglacials (line 418) with lowest (highest) input of aeolian material during interglacials (glacials) (line 380). Looking at Fig. 8, it indicates (1) lowest but also highest Si/Al and Ti/Al ratios during glacials and (2) hardly any variability in the XRF data at all and MIS 5, 7, and 9 – the only interglacials covered by the XRF data do not show a clear trend as stated. Strongest variability is within MIS 3 with reaching highest and lowest values during this period. Actually, the strongest signal revealed by this data set is a decrease in aeolian AND fluvial input in MIS 2. The discussion, however, is oriented along the line either more fluvial and less aeolian or vice versa . . . These data are used to back-up the conclusion that more humid conditions offer a better environment for the corals than more arid conditions. However, on a chronological much better resolved BR 1 record for the Early Holocene, Fink et al. (2013) exactly show the opposite with enhanced Si/Al ratio (more arid) corresponding to fastest mound aggradation (i.e. best living conditions for the corals). Without making any judgement, what is the right solution, I want to make the point here that the findings of the few papers dealing with cold-water corals in the region should be properly discussed.

Response: We agree with Reviewer #1 that this part needs to be reworked. Indeed, highest but also lowest Si/Al and Ti/Al ratios are recorded during glacial periods. This will be corrected in the revised version of the manuscript. Furthermore, the decrease in aeolian but also fluvial input during MIS 2 needs more attention. However, some points addressed are wrong: at no point do we state that interglacials show a clear trend (the word “clear” is not used). The observation that strongest variability within the XRF data is found during MIS 3 is correct and stated twice in the manuscript: Line 383 to 385: “In the same way as for Ti/Al and Si/Al records, Zr/Al and Rb/Al ratios demonstrate an important variability during MIS 3, in comparison to other periods where the records are comparatively stable” and Line 511, and is further discussed in section 5.2.3. The conclusion that more humid conditions (increased fluvial input) during interglacials are a more suitable environment for corals is essentially based on the benthic foraminiferal assemblage composition. It is noticeably based on the high abundances of the infaunal Buliminids, Uvigerina mediterranea and Bolivina spathulata during interglacials (Figure 7). These species are, in the Mediterranean, dominant in the vicinity of the Po and Rhone river deltas (Jorissen, 1987; Mojtahid et al., 2009). Nevertheless, we do agree that the XRF records need to be described and interpreted with more reserve. Thus, also following the specific comment below (i.e.”the link between high d18O and high Ti/Al and Si/Al ratios during the last glacial is not at all obvious”), discussion linked to the XRF records has been reduced and reworked.

TOC and productivity

Comment Referee #1: The TOC contents in the lower part (>250 cm) of this core range between 0.2% and 0.8% and get slightly higher in the upper part of the core reaching rarely above 1%. So overall, these variations are really minor. The increase towards the top, a feature common to very many marine TOC records, might reflect on-going early diagenetic degradation of organic matter. In addition, the reported mound aggradation rates vary between 1 and 9.1 cm kyr – that is a factor of 9. Obviously, sedimentation rate has an effect of organic matter preservation and this might me important here seeing the range of aggradation rates. Furthermore, the authors invoke – partly severe – changes in bottom (and pore) water oxygenation – also this would affect organic matter preservation. So, using the only slightly varying TOC contents presented here as indicators for changing productivity (or organic matter flux), despite



such other factors, is in my eyes over interpreting the data – unless the authors have good reasons to do so, but those are not presented.

Response: We understand the interrogations addressed by Reviewer #1 concerning the low TOC content throughout the core and the potential effects of mound aggradation and water oxygenation. These interrogations have been discussed and pointed out for some time (Doyle and Garrels, 1985). However, we had to our knowledge, no means to counteract and take into account these potential effects. This is why the TOC contents are in the manuscript only scarcely used and only in support of interpretations made thanks to benthic foraminiferal assemblages and macrofauna (TOC contents are only mentioned twice in the entire discussion, and always in support of other datasets such as foraminiferal assemblages: Lines 399 and 501). The abundance of infaunal benthic foraminifera, e.g. Uvigerina mediterranea and Bulimina spp., are indeed a more trustworthy proxy for increased productivity than TOC contents. Nonetheless, the combined use of foraminifera, macrofauna and TOC content, gives a good indication of productivity. Low TOC contents are, to our knowledge, common in coral mound records. If Reviewer #1 prefers, it is possible not to address TOC content or to indicate more clearly that this proxy is considered untrustworthy and is only used in support to interpretations made thanks to benthic foraminiferal assemblages and macrofauna.

Comment Referee #1: If the authors counted all the benthic foraminifera, why didn’t they used the benthic foram accumulation rate as a productivity proxy?

Response: We did not use the benthic foraminiferal accumulation because we consider it as an untrustworthy productivity proxy. A number of micropaleontological studies have pointed this out (see the review Jorissen et al., 2007). Noticeably, Naidu and Malmgren (1995) showed that in low oxygen environments, BFAR (benthic foram accumulation rate) does not reflect surface-water productivity. Since we suspect that the seafloor at BR1 was at times depleted in oxygen, we further avoided to use the BFAR as a productivity proxy. Moreover, taphonomic processes, which directly impact BFAR, are not well constrained (see for example Murray, 2006; Fentimen et al., 2020).

Overall, since benthic foraminifera were identified at species level throughout the core, we prefer to consider benthic foraminiferal assemblages rather than the BFAR (which ignores species composition).

Comment Referee #1: From line 397 onwards the link between TOC contents (the text partly refers to flux or ex- port, however, no such information in terms of rates exists for this core) and benthic foram fauna composition leads to the conclusion that interglacials were more productive. However, TOC contents are highest in MIS 3 (and late MIS 5) and very low in MIS 7, 5e, and 1. As said before, it reads if first there was the interpretation and later on the data were analysed with the interpretation already preset. In line 644ff the authors refer to published knowledge that corals thrive on fresh organic matter. In the next sentence, the needed phytoplankton blooms in the study area are explained to be triggered by "input of degraded fluvial organic matter". Never heart about something like that. The river might bring (real) nutrients supporting the phytoplankton, but the phytoplankton cannot thrive on degraded organic matter. The link to the degraded OM is based on the statement of the authors that the OM in their sediment core is essentially of terrigenous origin (line 303). In a marine, productive setting like the Alboran Sea, this sounds rather unlikely . . .

Response: There is no mention in the manuscript of MIS 5e, we do not believe that the stratigraphy presented in this manuscript allows to identify sub-stages. On average, the TOC content during MIS 5 is indeed higher than during MIS 6 or MIS 4 (see Figure 4). So this statement is not incorrect.

We agree that such a pattern is not as clear for MIS 7, although an increase can be observed. Corrections will be made to indicate that this pattern is not applicable for MIS 7 and 1 (possibly due to the biases indicated above). Details will be added to section 5.2.3 mentioning that MIS 3 shows high TOC content, this actually matches the conclusions made in the first paragraph of section 5.2.3 that highlight that corals and the benthic foraminiferal community positively responded to short phases of increased surface productivity related to important continental runoff during MIS 3.



We agree that the phrasing at line 644 is awkward, this will be corrected. Correction: "In contrast, corals at BRI are likely supplied by plankton blooms triggered by river-transported nutrients during interglacial times". The statement that organic matter in the sediment core is of terrigenous origin is based on the high oxygen index (OI) values. This can be observed in the Supplementary data (this may not have been added to the online submission, if this is the case we apologize for the inconvenience, Lines 301 to 303). Plotting a Van Krevelen index (see for example, Espitalié et al., 1985), i.e. the Hydrogen Index (HI) against the Oxygen Index (OI), demonstrates that the organic matter is indeed of terrestrial origin and well oxidized (see figures below). Plotting a pseudo Van Krevelen index (i.e. OI vs. Tmax) also indicates that the organic matter is of terrestrial deltaic origin (see below).

Figure 1. Van Krevelen diagram

Figure 2. Pseudo Van Krevelen index

Oxygen

Comment Reviewer #1: Line 438 ff refers to dysoxic conditions during interglacials that would have hampered coral proliferation as demonstrated by low mound aggradation rates. Well, the same group (and others) also published mound aggradation rates for BR 1 for the Early Holocene of >400 cm kyr-1 (Stalder et al., 2015) – that is 40 times higher as everything reported here. Obviously, corals can be very happy at BR 1 under such conditions . . .

Response: Indeed, Stalder et al. (2015) and Fink et al. (2013) published mound aggradation rates for BR 1 of over > 400 cm.ky-1, respectively 457 cm.ky-1 (between 13023 and 12717 ka) and 416 cm.ky-1 (between 12874 and 11240). So these rates are not for the Early Holocene if we accept that the Holocene Epoch started at 11.7 b2k (see Walker et al., 2018). This stratigraphy has been accepted by the International Commission on Stratigraphy, and formally ratified by the Executive Committee of the International Union of Geological Sciences on 14th June 2018 (Walker et al., 2018).

Cold-water coral communities are nowadays rare at BR 1 (Hebbeln et al., 2019).

Moreover, we believe again that the time scale considered in this study (300 ky for a 920 cm long core) allows to identify more long-term environmental changes than those from Fink et al. (2013), Stalder et al. (2015) or Fentimen et al. (2020). The time-scale covered by these studies allow to identify precisely short but rapid periods of mound aggradation. This study does not aim to do this, but rather to look at the wider picture (see previous paragraphs). Again, the mound aggradation rates presented are average values. We believe that a core covering the last two interglacials allows to draw more solid conclusions about the impact of environmental changes on mound development than a precise study of the last 15 ky.

We would like to add that is the opinion of the authors that, besides general trends, very local environmental variations at BR 1 may account for important differences between cores recovered in the area. A manuscript discussing such local differences is in preparation at the moment.

Comment Reviewer #1: Furthermore, one of the main conclusion of the present manuscript is that the coral predominantly thrive under interglacial conditions . . . And, finally, later on it is argued that oxygen decreased at the transition from interglacials to glacials . . .

Response: This is not a correct citation of the manuscript. For example, conclusion section (Line 705) "(. . .) corals did not thrive but rather developed under stressful environmental conditions at Brittlestar Ridge I". The term "thrive" is not associated to coral development at BR 1 in the manuscript. Or Line 671: "CWCs did not thrive at the site of core MD13-3462G but rather developed under stressful, possibly dysoxic, environmental conditions".

It is not written that oxygen decreased at the transition from interglacials to glacials, but rather that "These results suggest that transition phases between interglacial and glacial periods were characterized by winnowing at the seafloor (Line 609)" and that



"The seafloor was possibly depleted in oxygen at the end of interglacial phases (Line 690)". However, this sentence is possibly awkward and can be reworked to insist that the end of interglacial periods were possibly marked by oxygen depletion, whilst the beginnings of glacial periods were rather marked by increased bottom currents.

Specific comments

Reviewer #1: Line 41: I would strongly suggest to differentiate between nutrients (nitrate, phosphate etc.) and food. In aphotic depths corals do not need any nutrients, but food. Later on in the text when you deal with river input, you really mean nutrients . . . Make a clear distinction between these terms. Line 73: see also Glogowski et al., 2015

Response: Corrections made

Reviewer #1: Line 81: ref should be Lo Iacono et al. 2014

Response: Reference to Lo Iacono et al. 2014 added

Reviewer #1: Line 96-104: not relevant here, skip

Response: This part introduces the overall geological setting of the study area. It explains how such ridges as BR 1 /the study site) were shaped. Although possibly not of interest and redundant for readers acquainted with the area, we believe that this information is interesting for readers not acquainted with the geology of the Western Mediterranean (especially since this work is directed at an international audience, not exclusively European researchers). Thus, we would prefer to keep this as such (especially since this part is short, i.e. 8 lines).

Reviewer #1: Line 106: ref should be Fink et al. 2013 (first mention of BR)

Response: Correction made and reference added. Although we agree that Brittlestar Ridges were mentioned and named by Fink et al. 2013, Comas et al. (2009) do also mention ridges in the area: "On the seafloor, mounds appear as ridge-like buildups

100–250 m wide, 2-6 km long, and 20–60 m (up to 100 m) high above the seabed". Thus this reference should still be mentioned. We also noticed that the reference to Comas et al. (2009) was missing in the reference list. This has been added.

Reviewer #1: Line 134: "northwest" instead of northeast

Response: Correction made.

Reviewer #1: Line 141: "westward" instead of eastward

Response: correction made "westward circulating branch of the Eastern Alboran Gyre"

Reviewer #1: Line 224: this means, when a sample contained >300 specimen (e.g., 320) then it was split. In this case only 160 specimen were counted?

Response: Indeed this sentence is wrong and needs to be corrected. It now reads: "If the residue contained more than 600 specimens, it was split using a dry microsplitter." Samples were split if the residue contained over 600 foraminifera (targeted number of 300 individuals).

Reviewer #1: Line 234: should read >2mm

Response: correction made

Reviewer #1: Line 303: you really think that the organic matter preserved in your core is of essential terrestrial origin? Later on, you use the TOC data as an indicator for productivity . . .

Response: Yes the RockEval results support this, see response page 7 (and attached Van Krevelen diagram). At no point in the manuscript is TOC data used as an indicator for productivity. In the discussion section, the mention to TOC can be found twice: Line 399: "The overall higher TOC levels during interglacials confirm that the sediment during these periods was relatively enriched in organic matter in comparison to glacial periods", and Line 501: "Low SSmean values and reduced TOC content in the sediment confirm that glacial periods were marked by weak bottom current velocities and



organic matter flux".

Reviewer #1: Line 332: this is discussion, does not belong to results

Response: Which sentence does Reviewer #1 refer to? We do not think that either of the two sentences are out of place in the results section.

Reviewer #1: Line 390: the first sentence of the discussion refers to higher abundances of e.g. B. spathulata during interglacials. According to Fig. 7, their highest abundance is in MIS 6 and at the MIS 3/2 boundary . . .

Response: We agree that B. spathulata shows lower abundances than Buliminids and U. mediterranea (see Figure 7). However, B. spathulata do increase during MIS 5, 7 and 9 (when compared to values at the onset and end of these stages, see Figure 7). Thus, the sentence has been reworked, it now reads: "During interglacial periods, benthic foraminiferal assemblages are marked by high abundances of the infaunal Bulimina spp., U. mediterranea and to a lesser extent B. spathulata". It is correct that B. spathulata reaches highest abundance during MIS 6, a mention to this is now added in the results section. We took the decision not to discuss this point, since the discussion is already long and we had to focus on main trends. Other benthic foraminiferal species show interesting abundance patterns but are not graphically represented here (166 species were in all identified). The species graphically represented here were chosen because they are good discriminating species, they represent dominant species in the core, and their ecology is well constrained (which is not the case for all species, making their use as an environmental proxy more complex and limited). However, the complete benthic foraminiferal dataset is available as supplementary information.

Reviewer #1: Line 451: "westward" instead of eastward

Response: correction made "westward circulating branch of the Eastern Alboran Gyre"

Reviewer #1: Line 454: this is already documented by Wang et al. (2019)

Response: We agree that this follows what has been documented by Wang et al.

(2019) for the Bolling-Allerod and Early Holocene, although our interpretations are here based on a broader time scale. A reference to this has been added.

Reviewer #1: Line 455: How do you know? Any reference for this statement?

Response: Knowing that BR 1 is situated in the path of the Eastern Alboran Gyre and that and that mixing between surface and intermediate water masses is documented to occur down to 300 m (e.g. Heburn and La Violette, 1990), it is conceivable that corals profited from this oceanographic setup. This interpretation is noticeably based on benthic foraminiferal assemblage composition and the foraminferal stable isotope (O and C) values (see discussion). This is an interpretation of the dataset (thus discussion), and as all interpretations (especially when paleoenvironments) it is tentative and may be revised if new information and knowledge is gathered and fuels the debate. We do not "know", we are proposing an interpretation, which with the information at hand, seems the most plausible.

Reviewer #1: Line 463: there is no section 6.1.1

Response: corrected, section 5.1.1

Reviewer #1: Line 503: think about, if these mollusk layers may represent hiatuses . . .

Response: this interpretation was considered. However, considering that most of the shells were intact or near-to intact (Figure 6), and that they are quite fragile (and brittle), we do not think that these layers represent hiatuses. Furthermore, new radiocarbon datings (performed at ETH Zürich in collaboration with Dr. Irka Hajdas) from the first meter of core MD13-3462G confirm that these layers do not correspond to hiatuses. These radiocarbon datings are part of a manuscript that is currently being prepared.

Reviewer #1: Line 511: there is no section 6.4

Response: correction made, section 5.2.3

Reviewer #1: Line 527: how do you know about the quality of the organic matter?



Response: The benthic foraminiferal assemblage is used as a proxy for organic matter quality. This is developed prior to the sentence Line 527 (see lines 515 to 528). Noticeably, Cibicides spp., D. coronata and C. laevigata share a preference for high quality fresh marine organic matter (e.g. De Rijk et al., 2000; Milker et al., 2009, Stalder et al., 2018).

Reviewer #1: Line 563: Stronger contribution of nutrient-rich and well ventilated West. Med Deep Water to the coral sites only can have supported bryozoan proliferation with respect to oxygen. Nutrients provided by the WMDW would be "real" nutrients such as nitrate, phosphate etc. which would be of no use for any organisms in these aphotic depths. Be more precise in using the terms nutrients and food!

Response: correction made, "food" instead of "nutrients"

Reviewer #1: Line568: cannot see "particularly unstable" isotope values during the last glacial in Fig. 4!

Response: Indeed, the sentence is not correct it has been taken out.

Reviewer #1: Line 582ff: The link between high d18O and high Ti/Al and Si/Al ratios during the last glacial is not at all obvious, thus it cannot "confirm" (line 587) anything! Actually, between _100-200 cm you have high Si/Al ratios aligned with either high or low d18O values . . .

Response: We agree that this statement is not supported by the data. This whole section has been deleted (Lines 581 to 602).

Reviewer #1: Line 591-598: This Heinrich event discussion has no real relevance for this story . . .

Response: Section has been deleted (see previous comment)

Reviewer #1: Line 600: where is the logic link?

Response: Section has been deleted (see previous comment)

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



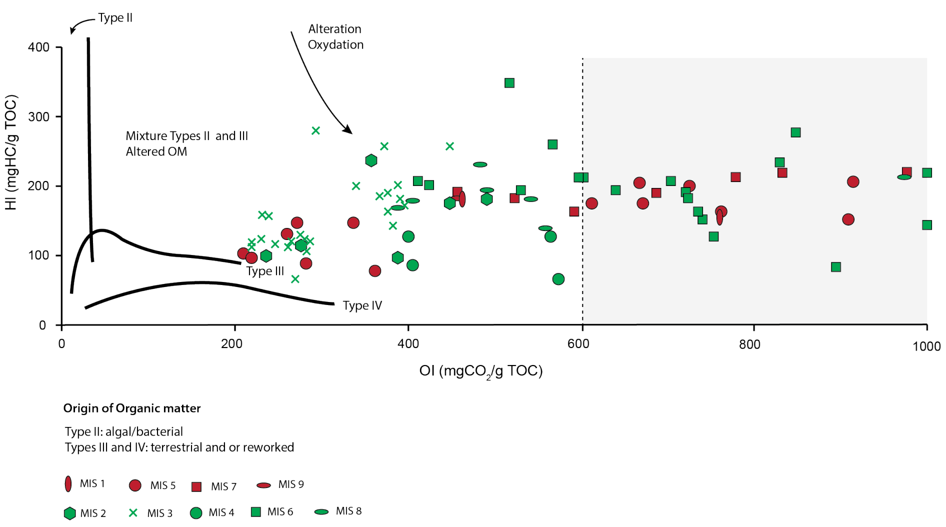
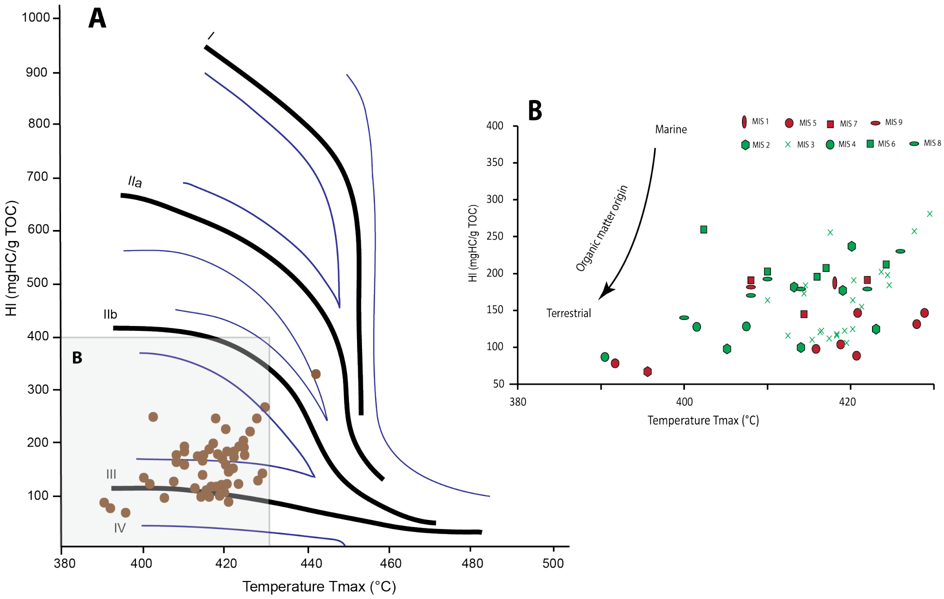



**Fig. 1.**




Origin of Organic matter

I - Lacustrine
IIa - Marine
IIb - Mixed marine-terrestrial
III - Terrestrial deltaic
IV - Terrestrial, high rank

**Fig. 2.**