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Climate of the Past Discussions

CPD

Interactive comment

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.

Anonymous Referee #1

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In this manuscript, Fentimen et al present for the first time cold-water coral ages reaching beyond the last deglacial period. This is the first sediment core-based (i.e. +/- continuous) coral record from the Mediterranean reaching back to ∼300 kyr BP. These very interesting data set is compared to sedimentological, geochemical and micropaleontological data obtained from the matrix sediments of the same core in order to put the long-term development of the cold-water corals into a paleoenvironmental context. Unfortunately, this part has some serious problems (as outlined below) addressing the core stratigraphy and the interpretation of the applied proxies. There is a reason that

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many other groups doing similar work try to use coral-barren cores taken nearby the coral mounds to reconstruct the paleoenvironment. As in most cases coral growth is intermittend and, thus, matrix sediments preserved among the corals only represent periods with coral growth. The often longer periods without coral growth are usually not preserved. In addition, some conceptional problems (e.g. linking highest coral percentages to best coral living conditions or explaining plankton blooms by enhanced fluvial input of degraded organic matter) and some "very brave" and/or selective interpretations of the data contribute to the perception of a paper that is far from being publishable. Thus, although I like the coral data set, I have to recommend to reject this manuscript. The most important step to solve the key problem would be to add considerable more coral ages.

Coral ages and stratigraphic interpretation 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 $\sim$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. 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

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authors that the corals preferentially lived during interglacials. 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.

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.

Current reconstructions 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.

XRF data 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? 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.

TOC and productivity 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 ongoing 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. If the authors counted all the benthic foraminifera, why didn't they used the benthic foram accumulation rate as a productivity proxy? From line 397 onwards the link between TOC contents (the text partly refers to flux or export, 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 . . .

Oxygen 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 . . . 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 . . .

Specific comments 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 Line 81: ref should be Lo Iacono et al. 2014 Line 96-104: not relevant here, skip Line 106: ref should be Fink et al. 2013 (first mention of BR) Line 134: "northwest" instead of northeast Line 141: "westward" instead of eastward 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? Line 234: should read >2mm 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 . . . Line 332ff: this is discussion, does not belong to results 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 . . . Line 451: "westward" instead of eastward Line 454ff: this is already documented by Wang et al. (2019) Line 455: How do you know? Any reference for this statement? Line 463: there is no section 6.1.1 Line 503: think about, if these mollusk layers may represent hiatuses . . . Line 511: there is no section 6.4 Line 527: how do you know about the quality of the organic matter? 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! Line 568: cannot see "particularly unstable" isotope values during the last glacial in Fig. 4! 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 . . . Line 591-598: This Heinrich event discussion has no real relevance for this story . . . Line 600: where is the logic link?

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