Clim. Past Discuss.,
https://doi.org/10.5194/cp-2020-82-RC3, 2020

Climate of the Past Discussions

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

Received and published: 7 September 2020

Response to the authors' response: Despite the points raised by the authors in their response, I am still convinced that this manuscript should not be published. Although I still do not agree with some of the interpretations (which could be improved in a revision process), the main point for suggesting a rejection is the poor age model as pointed out before. In my eyes, the lack of a convincing age model correctly reflecting the presumably intermittent development of the sampled coral mound/ridge, precludes the publication with the available data base. And increasing this data base will take time and most likely significantly change the interpretations. I will not respond point-

by-point the comments by the authors. But below I will highlight a few points made by them. TOC data: The authors argue in their response: 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 . . ." Okay, this does not necessarily relate to productivity, but seeing this in context with the sentence just before (line 397): ". . . the benthic foraminiferal assemblage during interglacials would support a high organic matter export to the seafloor" indeed relates the TOC content to productivity. If the high organic matter export to the seafloor is "confirmed" by the TOC record, then I only can read it as TOC being used as a productivity indicator. Thus, I cannot follow the response by the authors mentioned above. Furthermore, the authors did not resolve the question about the meaning of the TOC record for this paper if it only represents terrigenous organic matter.

Stratigraphy: The authors respond that the chronology of the core has been developed based on the coral ages in conjunction with the d18O data and that this manuscript concentrates rather on characterizing changes between interglacial and glacial periods. However, in the ms it is stated that "due to difficulties to define precisely the stratigraphy of this section of the core (MIS 6 to MIS 9), it will not be considered in detail during this study (line 286)." Thus, if MIS 7 and MIS 9 will not be considered in detail and as MIS 1 is not discussed, how can generally conclusions about "interglacials" be made if "solid" information only exists for MIS 5? Basically the same refers to glacials . . . As pointed out in the first comments made, the stratigraphy of this core is not sufficiently constrained to allow the conclusions made. Furthermore, the authors refer in their response to the pioneering work of Dorschel et al. (2005) and Rüggeberg et al. (2007) from the Irish margin. As nicely pointed out there, coral mound records might have considerable age offsets between coral fragments and foraminifera dated from the same core level. This is also reflected in peculiar d18O data reflecting mainly intermediate (non-interglacial, non-glacial) climate stages while the corals are from in-

terglacial periods. Thus, at Propeller Mound at the Irish margin the foram data do not allow to make any conclusions about the environment at times of coral growth. That is not necessarily the case for the Alboran Sea, but it might be and only a better age control could help here.

Foraminifera analyses The authors state that most of their conclusions are mainly built on the foram data, while other data sets (e.g., TOC, sortable silt) are only meant to support these. This is a valid approach, however, seen the diversity of benthic forams and their different behavior in different settings, often they are used to back up other proxies. In the first comment, I questioned the use of sortable silt in a coral mound setting. Now the authors state that despite all the problems raised, it is used to support the current strength record provided by the infaunal (!) T. angulosa. But to my perception, there is no fit at all between these two records. As pointed out before, it seems that first there was the interpretation and afterwards the data "have to fit". The authors also state that the interpretation of more humid conditions also is based mainly on the foram assemblage and only backed up by the XRF data (see comment on XRF data below). Well, higher abundances of some infaunal species probably points to higher OM fluxes driven by higher productivity. However, if this was fueled by fluvial discharge, by enhanced mixing or by advection of nutrients cannot be resolved from the benthic foraminifera assemblage ... Overall, as the benthic forams are the most important proxy here, a more critical discussion about the interpretations based on the benthic forams provided here is lacking.

Mound aggradation In my response I made a hint to high mound ARs of >400 cm kyr-1 in the early Holocene as reported by other studies from the region. Now, thanks to the authors, I learned that these only partly occurred during the Early Holocene, but partly during the very end of MIS 2. Nevertheless, high ratios of >200 cm kyr-1 extend well into the Holocene. Indeed, this is a nice example that during the last deglaciation the corals were most active between ∼13.5 kyr BP and 9 kyrs BP, neither giving a clear hint to preferred glacial or interglacial conditions. As this was the state of the art, on

which the present ms should be built, why do the authors look for glacial – interglacial differences?

XRF Sorry, but the hint given in the response that the running mean on the XRF data would help solving the problems with measurements on corals is not sufficient at all. Talking about coral contents of up to >30% this does not work. Still, in my eyes the XRF data are very problematical to use. In addition, as pointed out before, the minor changes in the elemental records are heavily overinterpreted.

Finally, the authors respond: 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. This only will work based on a solid stratigraphy. And I am pretty convinced, a solid stratigraphy will show, that also this record provides a serious of short intermittent pulses of mound aggradation, which very nicely will fit the records of these previous studies (as at almost all up to now known coral mound sites). Thus, it can at least be questioned if there is such an impact of long-term environmental changes ... In addition, the authors "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.". Well, assuming intermittent mound aggradation also prior to 15 kyr BP (what is not questioned by the authors), I cannot follow this statement.

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