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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.

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I am posting this comment even if it may not be considered by the Editor as I am from the same Institute where the first authors was affiliated until February 2020 and other authors are presently affiliated (and co-authors of some other publications on the topic together with me).

However I cannot leave without commenting on such a manuscript and leave it to be published at least for the foraminiferal part that is claimed to be robust and presents

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major weakness, as well as the paloeceanographic part.

In general, the interpretation is forced, giving CWC foraminifera a "clear, fixed and not questionable" significance, which may not be the case, especially for these ecosystems that are not completely well understood. These organisms can easily adapt and change their ecological preferences according to geographical location, oceanographic parameters, e.g., water depth, substratum, salinity, temperature, etc. . . ..(e.g., the same species can live in relatively shallower or deeper water according to the type of substratum, the same applies for all other parameters, e.g., salinity, nutrient and oxygen availability). In the manuscript all the discussion is based on given and fixed foraminifera ecological preference taken from the literature and in different geographical setting, instead of starting from establish proxies (e.g. TOC) and then interpret foraminiferal data.

Every situation must be evaluated case by case and anyhow a complete dataset including fractions smaller than 125 μm should be presented.Explainig everything with displacement is not a real reason. The same applies to the counting of the plankton, is more a problem of time consuming than scientific. To demonstrate that it is a scientific reason, data sghould be presented first and tehn excluded. The >125 μm can be useful when making taxonomic work e.g., taxonomic atlases and guides with plates (e.g., Milker and Schmiedl, 2012) but not for ecological purposes, in this case the 40 μm frection counts should be presented and eventually afterward not included in the discussion.

It is not clear how he density of benthic foraminifera has been calculated. The method used should be better explained and should be specified the reason for the choice. The method used in the manuscript does not correspond to any of the generally used in micropaleontology.

Line 226-227: "The benthic foraminiferal density was calculated by dividing the total number of foraminifera of a given sample by the sample fraction's weight".

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RC: However, only 300 specimens per split were counted (line 223-224). How the splits represent “the fraction’s weight”?

Usually density is calculated: - Number of foraminifera x gram of sediment (when sediments are generally homogeneous, not containing macrofaunal that can overestimate the weigh). It is calculated using number of foraminifera per single split, number of splits and weight of dry bulk sediments, and not fractions deriving from washing. (E.g., Moura et al. 2017 among many others)

- Number of foraminifera per volume (the most used for living assemblages and suggested in standard protocols). This method can be used also for fossil assemblages. In the article by Schönfeld et al., (2012) it is additionally and clearly stated that the 63 μm size fraction should be investigated when the environment is expected be more euthrophic. Or to show variations in organic matter content.

- Species percentages over the total specimens counted (in use for fossil foraminifera, especially planktonics). The first to use this metod were Haq et al. (1977) and successively Premoli Silva and Boersma (1989). Followed by many others.

I would like also to comment on Figure 10, which looks very fancy but presents a few problems. First of all it is upside down (even if the cardinal points are marked), we conventionally (and geographically) see the African margin at South and European Margin at North. Not the vice versa. This confuses the reader. As commented above during glacials the thermocline and pycnocline should be very shallow favoring water and nutrient mixing. In Figure 10 glacials are on the contrary described as stratified, the explanation for this is based only on comparison with modern times, it is generally confused and/or based on assumptions and circular reasoning. No clear evidence is presented.

On the contrary interglacial are represented as are the typical models for high latitudes/glacial times e.g., with strong mixing of water mass and nutrients. I First of all in the Mediterranean this cannot be possible, even in the past, also considering the temperate latitude and seasonality. Additionally, if during interglacials fluvial input increased, then the fresh water plume arriving into the sea must have produced a clear separation of water masses (fluids with different densities) and not mixing. The closest large river is only at 50 km (Mouloya) , if the fluvial input was so massive to trigger coral growth, then also the fresh water plume must have been significant enough to produce stratification not mixing. Other alternative processes must be discussed?

If responsible for stratification in glacials are the stronger ShW then it must be demonstrated that they are indeed stronger (what ever “stronger” means: denser? colder?) and remarkably colder than at the surface to justify such a stratification acting a physical barrier between the sea floor and the surface. And this is not possible with the present data. At least an intermediate water species should have been analyzed for oxygen isotopes and not only at the BRI site but also in the Atlantic waters, e.g., Cadiz to have the ShW signature, as these are the waters that are supposed to influence the Alboran Sea (e.g, as in the title). Only Atlantic or Mediterranean waters are marked in the figures. If there were rivers they have to be documented as they are not only today but also how they were in the past 400.000 years, according to geological information.

Last but not least and for respect to the funding agency the first author Robin Fentimen should also acknowledge the Swiss National Science Foundation Project Ref. 200020_153125 “Faunal assemblages from active, declining and buried cold-water coral ecosystems” that payed his salary for 3 years over the 4 years of his PhD, and that has co-funded with the amount of 54.000 Euro the cruise Eurofleets GATEWAY, MD194 during which the cores investigated in this research were retrieved.

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