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Climate of the Past Discussions

Interactive comment on "Holocene hydrography evolution in the Alboran Sea: a multi-record and multiproxy comparison" *by* Albert Català et al.

Albert Català et al.

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We thank to Referee #1 for the useful comments and suggestions. Below we detail our replay point by point. RC#1: The overall quality of this paper is good. RC#1: The technical quality of the figures is generally very good.

We thank the ref#1 for this positive comment.

RC#1: The authors can easily deal with this by having a native English-speaking scientist help to edit the paper.

English has been improved by a native speaker.

RC#1: The paper would be improved if the authors presented a little more data on

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Discussion paper

the reproductive season and depth habitat of coccolithophorids in the Mediterranean, since the differences depth habitat and seasonal is the basis for proxy interpretation.

We incorporate further information about the depth habitats of E. huxleyi and G. bulloides in to the discussion in section 4.2 (lines 293-324) based on previous studies of plankton depth preference. But also a new analysis of current seasonal and depth temperature and δ18O distribution in the region is incorporated and it allows to evaluate the feasibility of the two considered SST-tools to reproduce the preference depth and season of their respective proxy carriers (also included in Fig. 4a and b).

RC#1: Although is likely that the relative few forams needed for a Mg/Ca analyses will introduce more variability (good or bad) to the data, it is not entirely clear that the alkenone record would integrate “several seasons”. It is not clear what several seasons means. Is it the same season from several years or is it actually the Integration of a larger portion of the year in the alkenone samples (and from several years in addition)?

According to sediment trap results from the Alboran Sea (Bárcena et al., 2004) E. huxleyi is represented during the whole year however it blooms during November-December while G. bulloides is exclusive of the mixing seasons and particularly centred at spring (April-May). Since E. huxleyi growths during the whole year (all 4 seasons), each sample would represent an annual average but slightly biased to the bloom season. Moreover, one sample integrates alkenones produced during several years, according to the local sedimentation rates. Regarding G.bulloides, since it growths during the spring months (April-May) each sample would record preferentially these spring months. Mg/Ca measurements would also integrate several years but, in contrast to the alkenone measurements that represent the signal of the whole amount of alkenones present in the sample, the Mg/Ca ratio is measured in about 30-50 specimens and thus biased towards the more productive months/years when the conditions are optimum for G. bulloides. This has been better described in section 4.2

RC#1: I assume that the equation used for the alkenone data is calibrated against

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the average annual SST, however, when does coccolithophorids bloom in the Mediterranean today? Is it really a “well-averaged anual signal”? Could there have been any changes in coccolithophorid ecology between today and the deglaciation?

As explained in the previous comment the manuscript includes now a more detailed discussion on the preferential habitat of coccoliths in the Mediterranean. Regarding glacial-interglacial variability, previous studies from the Alboran Sea that combined both alkenone-SST reconstructions with nannofossil taxonomy did not interpret any relevant change in coccoliths ecology that was relevant for the SST interpretation (Ausin et al. 2015).

RC#1: However, it would be useful if the authors could look at this from a different angle and also address how changes in the water column could have affected coccolithophorids. What do we know about how coccolithophorids handle large changes on hydrography such as those during the deglaciation?

The paper is dedicated to the discussion and interpretation of the new Mg/Ca record. We agree that to some extend it needs to be discussed the coccolith habitat but we do not think that the manuscript has to devote to the discussion of coccoliths environments.

RC#1: The hypothesis involving SPG/STG dynamics at the end of the discussions needs a bit more attention considering that this would affect intermediate waters. How would these changes be transferred to the surface layer where the forams are usually reproducing?

We agree that this discussion is particularly relevant and interesting but the limitation is that very limited information exist for the Holocene regarding intermediate waters in the North Atlantic Ocean. Thus, this discussion, with the current available information, becomes really hypothetical and very poorly contrasted by actual data.

RC#1: The conclusions section is in my opinion is too long as it is now. Parts of it are

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more discussion type material.

The conclusions have been shortened.

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Figure 4

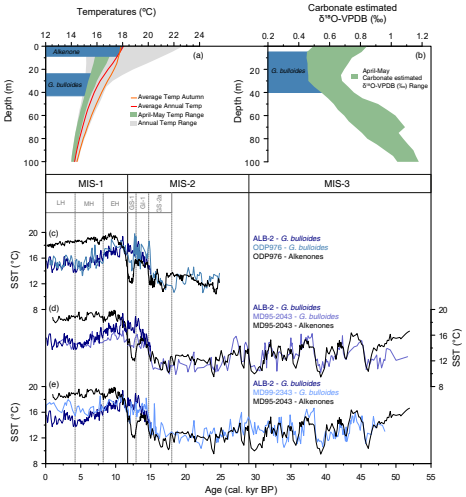

Fig. 1. Figure4

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