RC1

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 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 $\delta^{18}O$ 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 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 more discussion type material.

The conclusions have been shortened.

RC2

We thank to Referee #2 for the useful comments and suggestions. Below we detail our replay point by point.

RC#2: Some improvements in English are needed throughout the manuscript.

English has been improved by a native speaker.

RC#2: I think the paper is well-suited to be published in CoP, but given that >90% of the paper is devoted to a comparison of Mg/Ca & alkenone-derived SSTs and their implications, I suggest that the paper be accepted for publication, but not in the "Special Issue"

The Mg/Ca-Alkenone discussion is a necessary step in order to argue the paleoclimatic value/interpretation of the new Holocene high resolution Mg/Ca-SST record. This new Holocene SST record is the main goal of the paper. It is true that the 4.2 event is one structure among several others along the Holocene record. But we still believe that this record highlights the relevance of the 4.2 event in this Mediterranean region as a cold event but, it also marks an inflexion point in the main SST trends. These evidences support this as a period when fundamental changes occurred between the Mediterranean-Atlantic oceanic-atmospheric connections. For this reason, although we recognize that the 4.2 is not the only/main target of the manuscript, this event is relevant enough in the discussion of this manuscript in order to be considered for this special volume.

RC#2: lines 137-140: "...Seawater _18O (_18Osw) was obtained after removing the temperature effect, with the Shackleton paleotemperature equation (Shackleton, 1974) on the G. bulloides _18O signal using the G. Bulloides Mg/Ca–SST values. ..." In view of the issues discussed later (re MG/Ca discrepancies with alkenone-derived SSTs) can you say whether this is significant in your derivation here? This may require a brief explanation, expanding on this point.

This is an interesting point raised by the reviewer. Considering the large differences in the deglacial warming recorded by alkenone and Mg/Ca SST reconstructions, the temperature correction on the d18Oc to obtain the d18Osw would be very different regarding the chosen SST record. In this respect, it is very important to highlight that if any change occurred in the habitat preference of G. bulloides during the deglaciation, as it is argued in the manuscript, that will not only affect the Mg/Ca ratio but also de d18Oc. That brings us to a warming on the danger of applying SST corrections based in very different signal carriers which may have completely different response to the major environmental changes occurring during the deglaciation. This also stresses the relevance of the Mg/Ca-SST reconstructions as the only reliable tool to extract the temperature effect on the d18O signal of the foraminifera carbonate shells. A comment on this regard has been added in lines 375-379

RC#2: lines 338-340: ": : :from transitional to subpolar water (Kucera and Darling 2002; Kucera et al., 2005) but they start to be scarce in water with temperatures over 18_C.."
RC#2: As this is written "they" refers to genotypes. I think "they" should be replaced by " G. bulloides"...?

Done, changed for G. bulloides.

RC3

We thank to Referee #2 for the useful comments and suggestions. Below we detail our replay point by point.

RC#3: I also recommend checking the English by a native speaker

English has been improved by a native speaker.

RC#3: As other Referee, I also doubt about to be published in the Special Issue about the "4.2 ka event" due to no major discussion is centered on that time period.

This is already argued to the RC#2

RC#3: My general comments are mainly concerning the absence of discussion between cores ALB-2 and ODP976 since they are located in the same site (western Alboran) at different points: - There is a notable difference on _18O profiles between both cores (lines 232-235) for the whole time period, and specially during the YD-Holocene transition, whereas using Mg/Ca ratio on same species (i.e. G. bulloides) from the different cores, it is generally obtained a good correlation. It would be also good to add the error bars. - For the deglaciaton-YD there is notable differences on Mg/Ca ratio derived-SSTs.

We want to stress, as it is already mentioned in the text and figure caption, that both ALB-2 and ODP976 $\delta^{18}O$ records are plotted in Fig.2 with independent y-axis and the absolute values are totally comparable. We do not see such a notable difference between the two records as the reviewer mentions. Referee stress on the YD differences but fig.2 includes the age control of all the considered records indicating their error bars. It can be observed in that figure that the chronological constrain of ODP976 for the YD period it is very poor, and the structure there could have an error of several centuries, according to that, these discrepancies could be moved to fit in a good agreement with those of core ALB-2. We do not think that the aim of the manuscript is to go in the detailed discussion of minor structures, even more since the YD is not the main target of the manuscript. Regarding the Mg/Ca records it also has to be taken in account that the resolution of ODP976 in that part of the record (YD) is extremely low. That record had significant contamination problems and several of the samples were removed after a contamination check as is described in the original manuscript. Taking in consideration all these issues we consider that the comparison needs to concentrate in the main patterns and not in the little details.

RC#3: Major differences are observed in SSTs-derived from alkenones and Mg/Ca ratio. They are finally explained as different seasonal and depth habitat differences, suggesting that Mg/Ca-SST reflects spring season. However, if you compared SST values from the most superficial samples, Mg/Ca-SST are much lower (more than 2_C) than Uk'37-SST. Present-day SST differences between annual spring and autumn temperatures are less than 1_C, so Mg/Ca-SST might also reflect a deeper depth habitat of G. bulloides.

This is a good observation that we noticed was not treated in detail in the original version of the manuscript. The new submitted version includes an analysis and discussion of current

temperature and $\delta^{18}O$ distribution along the year and water depth. This is included in a new figure (Fig. 4a and b) and discussed in Section 4.2 (lines 293-324). It illustrates that the proxy differences in absolute SST estimations are coherent with the habitat preference in both season and water depth for the different proxy carriers.

RC#3: In general, there is not addressed the influence of hydrodynamic of the Alboran gyres on the different proxies derived SST.

The manuscript focuses in the discussion of the ALB2 Mg/Ca SST record. The multi record comparison is used to argue the regional consistency in the main patterns and document the commune response to the deglacial changes. Resolution of the records is very different and in some cases the chronology no very robust, for that reason we did not wanted to address the manuscript into little difference between the records that could be attributed to the gyres or other regional hydro graphic structures. Nevertheless, we recognise that there is the potential there for further studies.

RC#3: I also miss any further hypothesis about the effect of salinity changes on the different proxies, since Mg/Ca ratio is susceptible to be affected.

It has been incorporate a brief discussion on this in section 3.3 (211-220). We argue in there that the here discussed Mg/Ca ratios do not have a significant salinity overprint.

RC#3: Concerning the meaning of the UP10 proxy, if it is related to major paleocurrents during cold periods, there is not an increase at 4.2 kyr and later on there is a peak at ca. 3.5 kyr and 2.5 kyr that are not punctuated by a strong cooling signal.

The UP10 shows an increase in relation to the 4.2 event, but as the text acknowledges (lines 433-436) it is not one of the major ones within the Holocene. We agree with the reviewer that any increase in the UP10 occurs during the 3.5 and 2.5 cooling events and for that reason it is not commented in the text. We never argue that every cold event in Alboran has to have the exactly same pattern.

RC#3: Detailed comments

These detailed comments have been changed or added.

RC#3: Finally, I recommend rewriting and shortening accordingly the conclusions.

The conclusions have been shortened.