

Interactive comment on “Development of coccolithophore-based transfer functions in the Western Mediterranean Sea: a sea surface salinity reconstruction for the last 15.5 kyr” by B. Ausín et al.

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Dear Editor and Referees,

Firstly, we would like to thank you the time devoted in reviewing this manuscript and your comments, which we found appropriate and relevant. As a general comment, syntax, spelling and grammar were checked by an English native professional translator. The main changes undergone by the text in relation to the comments made by Referee #1 are explained below and marked in the final text. Please note that comments not

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mentioned here have been accepted and changes have been made accordingly to the Referees' suggestions.

"Material and Methods chapter, section 2.3 (lines 98-120): I miss a justification for the splitting of *E. huxleyi* and *G. oceanica* into two size classes. Did the author implemented this splitting because of differences in size classes between fossil and modern taxa (ie. no large *E. huxleyi* and/or *G. oceanica* in the modern dataset)? Other reason(s)?"

-The size criteria to split *E. huxleyi* responds to the well-known existence of two sized morphotypes ($< 4 \mu\text{m}$ and $> 4 \mu\text{m}$) with different ecological and biostratigraphic significance already demonstrated in the study area (Colmenero-Hidalgo et al., 2002; Colmenero-Hidalgo et al., 2004). *G. oceanica* size distinction follows a classical size criteria (medium: $> 3 < 5 \mu\text{m}$; large: $> 5 \mu\text{m}$). Despite there is no evidence to our knowledge that this separation has a demonstrated ecological significance, we usually carry out it due to a potential use in Ecology. A short explanation for this has been added to the text. Nevertheless, it has to be noted that abundance of *G. oceanica* $> 5 \mu\text{m}$ is very low, 0.24 % on average and 2 % as maximum.

"Throughout the manuscript: the term “range of SSS values” is often used, but might refer to either the amplitude of SSS changes within a particular event, or to the complete range of measured (modern data set) or reconstructed (downcore CEUTA10PC) SSSs. The authors should better define what they mean by “range off SSS values”. The same question applies to the frequent and sometimes incorrect use by the authors of “SSS gradients”."

-We concur with the Referee that the terms “range of SSS values” and “SSS gradients” might be confusing. We have clarified those terms where necessary.

"Section 4.1, lines 358-360: The authors recall a finding by Knappertsbuch (1993) of a positive relationship between *G. oceanica* abundance (in modern samples) and Atlantic-derived waters (and thus SSSs) in the western Mediterranean Sea. The

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authors should comment this finding later-on in section 4.3 and 4.4 based on the downcore distribution of *G. oceanica* within core CEUTAP10PC (Ausin et al; Paleo3, 2015). A visual comparison of the downcore SSS patterns and *G. oceanica* abundance changes within this core indeed shows a high degree of similarity between these two datasets. Such a comparison would therefore work in favor of both reliable SSS reconstructions, as well as the main influence of Atlantic water inflow upon SSS in the Alboran Sea."

-We appreciate the Referee's comment on the finding by Knappertsbusch (1993). However, we find this good visual comparison is a logical consequence of the "use" of this (but not only) relationship between *G. oceanica* and low SSS by the transfer function to reconstruct SSS, causing a circular argument.

"Section 4.3, lines 383-391. The reference to Bollmann and Herrle (2007) is not accurate. Bollmann and Herrle' modern micropaleontological dataset does not include any coccolith of *E. huxleyi* larger than 4 microns. Only their LGM dataset contain coccoliths close to but < 4 microns for higher latitude sites (north of 35_N). Rather than discussing Bollmann and Herrle (2007), the authors should relate the non-analog situation (linked with large coccoliths of *E. huxleyi*) to input of cold polar-subpolar derived Atlantic waters, based on the modern distribution of extant population of *E. huxleyi* type B (coccoliths larger than 4um) in subpolar environment such as in the southern Ocean (e.g. Poulton et al, Mar. Ecol. Progr. Ser., 2011)."

-We agree with the Referee reference to Bollmann and Herrle (2007) is not entirely correct. Certainly their modern dataset does not include any specimen of *E. huxleyi* > 4 μm , so this has been corrected in the text. However, we still believe their findings are worth mentioning and discussing briefly. Their LGM dataset does include *E. huxleyi* > 4 μm (see Fig. 6 and section 4.2 in that study), where specimens of *E. huxleyi* of 4-6 μm length in samples located in the Canary Island region led to an overestimation of the salinity values compared to values published by other authors (Table 2 in that study). Their results (and also interpretation) suggest that the large morphotype in

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ancient sediments represents an extinct morphotype. To be the case, *E. huxleyi* > 4 μm would lack an analog in modern sediments, as shown by the results of the present paper and therefore supporting our interpretation.

"Section 4.4.1 and 4.4.2: the authors often compare paleo SSS changes with pollen based changes in aridity/precipitation, hereby suggesting that precipitation changes might explained to a high extent the surface salinity changes (ie. line 414: "This change is not supported by the findings of Fletcher....."). This connection between paleo SSSs and paleo-precipitation is definitively ruled out by the authors in section 4.4.3 (Younger Dryas and Holocene) based on modern observations (incoming AW drive SSS in the Alboran Sea) and a comparison off SSSs reconstructions with Uk37- derived paleo SSTs. The comparison of reconstructed SSSs with paleo-precipitation records over the nearby continent should therefore only recall previous works such as Fletcher et al (Clim. Past 6, 2010) who illustrated and thoroughly discussed the phasing of high latitude cooling (cool inflowing Atlantic water through Gibraltar strait) with dryness of the western Mediterranean climate. A thorough investigation of the ocean-atmosphere coupling in the western Mediterranean region throughout the last deglaciation and Holocene is out of scope of B. Ausin's manuscript. Section 4.4: Keeping-on the discussion on comparing paleo-precipitation records with paleo SSS reconstructions (which, according to my previous comment should be drastically reduced) : Although very similar to the pollen-derived record of Combourieu-Nebout et al. (2009), the dataset by Fletcher et al. (2010) was produced from the same sediment core as the one from which the illustrated SST record is derived, core MD952043 (Cacho et al., 2001). Also, beside TMF (%) variability, Fletcher et al. (2010) provided additional indexes of interest such as quantitative paleo-precipitation reconstructions and/or pollen-based precipitation index (Ip). The authors might therefore consider illustrating Fletcher et al. (2010) record (rather than the ODP site 976 record), although mentioning that both MD952043 and ODP 976 pollen-records are highly coherent."

-The discussion about atmospheric conditions in the study area has been ruled out as

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suggested by Referee#2. Accordingly, any pollen record has been removed from Fig. 6.

"Section 4.4.3: a lot of emphasis is put on brief periods of low SSSs. No discussion is made on the exceptionally high values of SSS (higher than during late glacial and YD) during the short time interval of ca. 10-10.5 cal. ka BP. This event should definitely be discussed by the authors."

-A brief discussion for the SSS increase that occurred from 10.5 to 10 ka cal. BP has been added to the text.

Interactive comment on Clim. Past Discuss., 11, 3759, 2015.

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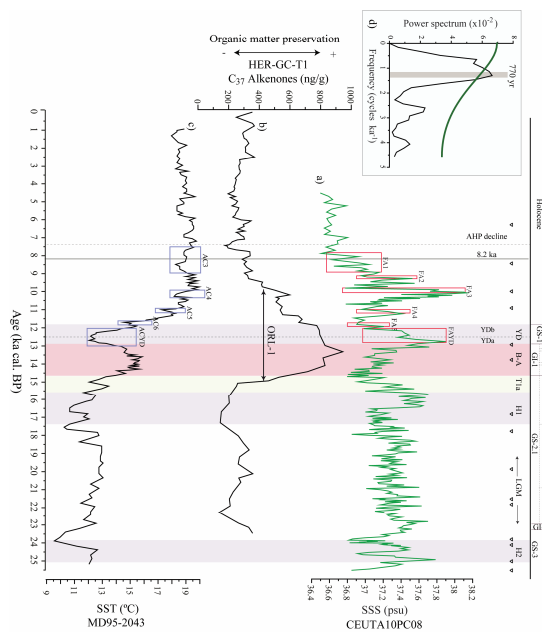


Fig. 1.

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