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Discussions

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 #2 are explained below (Referee's comments also added) and marked in the

final text. Please note that comments not mentioned here have been accepted and changes have been made accordingly to the Referees' suggestions.

-As a general comment, we tried to emphasize our results with those by Oviedo et al. (2015) and to interpret them in more relation to the oceanographic context.

"Page 3763, Line 21: are samples core tops, like stated here and throughout the manuscript, or surface sediments (caption Fig. 1)?"

-Stricto sensu, these are "core-top samples", later referred to as "surface sediment samples" to avoid repetition of a technical term. In any case, we have changed caption in Fig. 1.

"Section 2.3 Micropaleontological analyses: Gephyrocapsa caribbeanica is not included in living coccolithophore species (Young et al., 2003; Jordan et al., 2004). It is an important component of Atlantic Ocean surface sediment assemblages and further analysis/explanation should be nice. For instance electron microscope analysis may provide further taxonomic details and may rule out that they are not specimens of G. oceanica or G. muellerae. Calciosolenia murrayi is in my opinion not discernible by Calciosolenia brasiliensis and Calciosolenia corsellii. Much better Calciosolenia spp. More information is needed for the eliminated 29 samples (page 3765, lines 19-26). Why did you choose the 10% reworked treshold. As later discussed (page 3770, lines 18-21) also coccoliths with a compatible age (still living and long-range taxa) may be displaced or reworked. Why did you not simply rule out reworked specimens from counts and include all samples?"

-The species we found in our samples is a small specimen (< 3 μm) that shares many morphological features under the optic microscope with G. caribbeanica Boudreaux & Hay, 1967. We have already observed this small G. caribbeanica in Holocene sediments from many locations and previous investigations conducted by our group at Salamanca University using SEM pointed it could be an overcalcified small Gephyrocapsa. Nevertheless, we prefer not to lump together these latter species due to a

possible ecological significance. Certainly the name "G. caribbeanica" for this small specimen is not appropriate and it has been changed by Gephyrocapsa cf. caribbeanica in the text. -Most of micropaleontological studies, even those aimed at stratigraphic purposes, have to deal with the possible presence of reworked specimens within the "long-range" taxa and are based on the assumption that such presence, which cannot be quantified, is not significant. As reworked, we could only identify and quantify the obviously reworked specimens (in our case, those taxa pertaining to older stratigraphic levels with no representative in modern oceans and then lacking a relationship with modern surface water conditions). We counted 500 coccoliths per sample. Bellow 450 coccoliths, the statistical representativeness of the sample for studying the main species starts to decline (Fatela and Taborda, 2002), and this further affects minority species. This means that if we removed reworked specimens in those samples where their relative abundance is > 10 % (sometimes up to 80 %), the rest of the sample would not be statistically representative of the "in situ assemblage". Therefore, samples with obviously reworked specimens above 10 %, even considering that these specimens do not necessarily imply that the rest of the association is also allochthonous, were ruled out as a precautionary measure to not compromise the representativeness of the sample. We later explored the spatial distribution of reworked specimens in the remaining samples and observed: i) that samples far from river influence contained very low percentages of reworked specimens (usually < 2 %), and ii) that those samples with the highest % of reworked (but still below 10%) were located close to river mouths. From this we interpreted that higher % of reworked (but below 10%) were linked to terrigenous inputs associated to river discharges and therefore it could be assumed that the rest of the assemblage is autochthonous. For this reason, samples with a 10% of reworked specimens were retained in the modern training set, although of course, the reworked are excluded from the analyses.

"Page 3767, Lines 18-26: I did not understand. Please carefully explain this passage. Photic zone down to 300 metres depth?"

-This analysis was developed by Telford (2013) for planktonic foraminifera. Some species of these organisms present a clear zonation along the upper part of the water column and bloom in a particular season. Therefore, different fossil species preserved in the same sediment layer, might have lived under the influence of different conditions (different SST, SSS, nutrients, etc). However, calibrations in transfer functions are usually performed in a mechanical way using annual-averaged data of the environmental parameters at 10 m depth, therefore assuming that properties at that depth (10 m) and "season" (annual) were the most influential for the assemblage. This should be tested before, since it is possible that, let's say, summer-SST at 70 m influenced the assemblage more than annual-SST at 10 m. For this, several reconstructions are performed, each of them using for calibration data of the water column from a different depth and season. The reconstruction that more variance explains in the fossil assemblage will tell us the depth and season at which the transfer function has to be calibrated. The implementation of this analysis makes more sense when using foraminifera since the majority of coccolithophores inhabit in the upper photic zone, although still some species have a clear preference for deeper parts (e.g. F. profunda) or bloom in a particular season. Nevertheless, one of the aims we pursued with this work was to apply a wide array of new statistical approaches on our data and to show them in a logical series of steps that one should consider when developing a transfer function. This was intended to target the wide public that works on transfer functions, not only based on coccolithophores but on any other nanno/microfossil. We have tried to explain better this analysis on the text. As pointed out by the Referee, the photic zone does not reach 300 m (we took the same depths considered in Telford et al., 2013), and coccolithophores do not inhabit there anyway, so we excluded this depth too.

"Page 3768-3769, Lines 23-25 and 1-13: the description is quite poor and in any case the spanish-african coast comparison is possible just in a very narrow area. In my opinion, Figure 2 is quite self-explanatory and should be complemented by meso-scale oceanographic features (partially plotted in Fig. 1). Then the spatial distribution description of coccoliths on the sea floor will likely follow local hydrology."

-Meso-scale features have been plotted for the reader in Fig. 2 following the Referee's suggestion and G. cf. caribbeanica has been eliminated from this figure in line with previous comments. Despite this section has been rewritten placing more emphasis on the biogeographic pattern, description has been kept brief, since we agree this figure is self-explanatory and the likely relationship with the oceanographic context is discussed further in section 4.1). Regarding this section, it has also been rewritten following Referee's suggestions.

"Page 3770, Lines 18-21: which long-range taxa? Percentage and abundance? Can you rule out them and identify the in situ assemblage (see comment Section 2.3)."

-The yellowish color of this sample had already been noticed and written down when the counts were performed, but it was included in the initial modern training set because it only contains extant species. The fact that both methods (MAT and WA-PLS) pointed this sample as outlier along with its anomalous color made us suspect it had been subjected to diagenetic processes. Consequently, we preferred not to include it within the modern training set. It must be added that any anomaly/observation was written down when the counts were performed, and a revision of these notes let us discard that none other sample presented this color or similar characteristics.

"Page 3771, Lines 5-6: it is not clear from Fig. 5, how much is the SSS error range? How was it calculated? It is important to assess the error given that current geochemical methodologies (temperature corrected d18O) are affected by a huge full propagation error that makes SSS reconstructions unreliable (Rohling, 2007)."

-Sample-specific reconstruction errors are automatically derived for the fossil samples by the C2 software as follows: At each bootstrap cycle we obtain a slightly different value for our estimate. The standard error of these bootstrap estimates for each modern and fossil sample is calculated by C2, being this the prediction error due to errors in estimating species coefficients (i.e. the optima in WA). It is worth to note that we have added to this error a second error component that we had not considered before, and

have plotted errors accordingly in Fig. 5a. This second component is a constant that represents errors in the calibration function, calculated by C2 as the root mean square across all training set samples of the difference between the observed environmental value and the mean bootstrap estimate. Due to an extensive explanation is required to clarify how these errors are calculated, we have indicated the reader where to find detailed information about it in section 2.4.3. The amplitude of the error has been added to the discussion whenever possible.

"Page 3772, Lines 3-13: please re-write. Ecological preferences here speculated need to be formulated taking into account 1) previous reports and 2) a detailed hydrological setting (oceanography once again). So, in my opinion, it is wrong to suppose fresher water preference (low sea surface salinity) for Florisphaera profunda, a deep photic zone taxon (> 50 m depth). Low salinity in surface water is likely linked to anything else that influences and controls species distribution and abundance. Also very striking is the presumed preference for rather saline waters of Helicosphaera. High abundance of Helicosphaera carteri, arguably the main species you found, was often associated to fresher waters in many studies (i.e. Colmenero-Hidalgo et al., 2004; Narciso et al., 2010; Grelaud et al., 2012). Even in Ausin et al. (2015, PALAEO3) Helicosphaera spp. peaks are interpreted 'as being linked to the low-salinity inflowing Atlantic water' (Section 5.1)." -This section has been re-written following Referee's suggestions.

"Page 3772, Lines 14-29 and Page 3773: both discussion and references deal with the influence of salinity on coccolith weight mass, species type (Emiliania huxleyi), coccolith calcification, alkenone production and so on. All them are profoundly different from the influence on coccolithophore assemblages."

-We agree with the Referee and are aware of the differences between the "species" and "assemblage" approaches. We refer to those works for two main reasons: i) we believe that the evidences that exist on the relationship between coccolithophores and salinity (even at a species level) support the salinity influence over the total assemblage, and ii) there is a lack of works dealing with the influence of environmental variables over

the total coccolithophore assemblage using comparable methods. Nevertheless, it has to be mentioned that the work by Oviedo et al. (2015) explores this influence at a species level, but also at a hetero- and holo-coccolithophore assemblage levels, and their results are discussed in this regard. In any case, we have tried to clarify the difference between approaches in the text.

"Page 3776, Lines 2-5: these estimates are however affected by a huge propagation error (see comment above)."

-The propagation error that possibly affects other records and its implications for the comparison with our results have been included in the discussion.

"Page 3777, Lines 1-20: Alpine meltwater may have also had a role in ORL1 lower salinity (Rohling et al., 2015)."

-We thank Referee's valuable comment on Rohling et al. (2015), which has been included in the discussion. We have ruled out mentioning the work by Rogerson et al. (2008), which is already discussed and (the equations used therein) corrected by Rohling et al. (2015).

"Page 3777, Lines 17-20: how the Intra-Allerød Cold Period (IACP) was identified? Apparently there is no basis for the identification of the IACP by oxygen isotopes in Ausin et al. (2015, PALAEO3) and in fact it is not mentioned there. If so, you cannot identify it by the SSS increase. There is also an inextricable confusion about (among others) the IACP in Figure 6 (see comment below)."

-Indeed the IACP was not identified from our records, and consequently, has been ruled out.

"Page 3777, Lines 21-26: the only plot with Younger Dryas (YD) is Figure 6 and I do not understand it. YD and GS-1 are synonyms, simply from different records (the latter from Greenland ice cores) but in Fig. 6 there is a clear mismatch. The Greenland nomenclature from the column is out of phase with the grey shadow of YD. The mis-

match also involves the upper part of the Bølling-Allerød (B-A), the IACP should be in coincidence of GI-1b. There is also an evident problem with the timing of the base of B-A, YD and Holocene that should be respectively at 14.75 (or 14.65) ka, 13 ka and 11.7 (or 11.5) ka. Please check the plot. Without this basic information it is difficult to understand the salinity trend during the YD."

-We thank the Referee for their comment on Fig. 6. Certainly there was a huge mismatch with Greenland chronology, which has been corrected following Rasmussen et al., (2014). According to these authors, the "Oldest Dryas" term has been also excluded since it is poorly defined in literature and it has been changed by Termination 1b, already identified and interpreted in CEUTA10PC08 core by Ausín et al. (2015).

"Page 3778, Lines 13-18: In my opinion the visual inspection of Fig. 6 does not establish a firm correlation between SSS decreases and Alboran cooling (AC) events. In any case why are AC associated with SSS drops? There was not any significant amount of icebergs close to the Iberian Margin like for Heinrich 1 and Heinrich 2 layers (i.e. Bard et al., 2000; de Abreu et al., 2003). Why AC2 does not match with a SST deecrease? Authors should better explain their reasoning providing a mechanism that led to SSS decreases. In my opinion the comparison with terrestrial records (pollen and stalagmites) is extremely difficult and should be significantly shortened or ruled out."

-In order to better explain the SSS drops and its possible relation with AC events in the Alboran Sea, we argue a mechanism based on the Bond events (Bond et al., 1997). We have also included an explanation for the absence of SSS drop at times of AC2. Comparison with terrestrial records has been ruled out.

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Fig. 1.
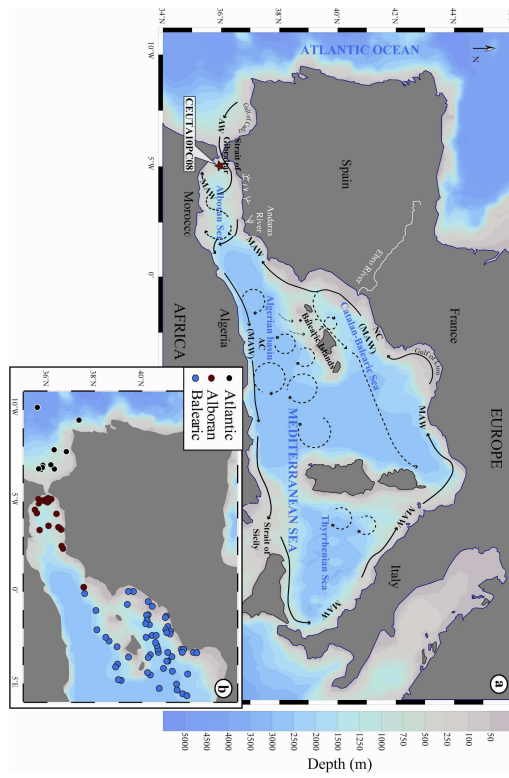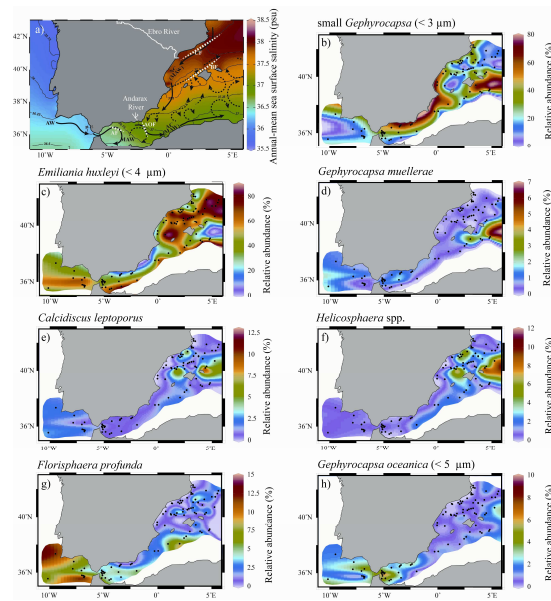
Fig. 2.

Fig. 3.
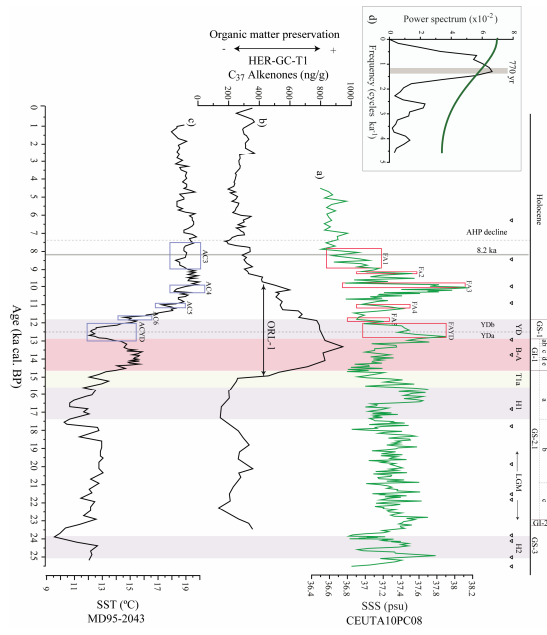
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