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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.

Anonymous Referee #2

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The paper 'Development of coccolithophore-based transfer functions in the Western Mediterranean Sea: a sea surface salinity reconstruction for the last 15.5 kyr' by B. Ausin, I. Hernández-Almeida, J.-A. Flores, F.-J. Sierro, M. Grosjean, G. Francés and B. Alonso is a valuable attempt at linking coccolith assemblages from surface sediments to salinity in a narrow area of the western Mediterranean Sea. Results are exploited to reconstruct sea surface salinity (SSS) in the western Alboran Sea since the Bølling-Allerød. I feel that the methodology is robust enough to support this original approach and that the paper promotes new efforts in the development of coccolith transfer func-

tions.

The major vulnerability of this paper is the lacking of a distinct oceanographic and paleoceanographic interpretation (i.e. different water masses, surface water currents, water mass dynamics) for both surface sediment coccolith assemblages and downcore SSS.

Surface sediments were recovered in two different hydrological settings, the Gulf of Cadiz/Alboran Sea and the Catalan Balearic Sea. As correctly plotted in Fig. 1, the Atlantic surface inflow (low salinity in the Gulf of Cadiz/Alboran Sea) reach the Catalan Balearic Sea after a long path (northern Africa, Tyrrhenian Sea) and severe evaporation and mixing (and may be upwelling of subsurface and intermediate water). Thus, in your record, you have extreme (36-38 psu) salinity values without intermediate terms (page 3770, lines 25-28). Different salinity means different surface water masses and coccolithophore assemblages should be interpreted in light of the hydrological setting. Even within the Alboran Sea, cyclonic and anticyclonic gyres, the Malaga and the Almeria-Oran fronts may have affected the patchy distribution on the sea floor of some coccolith species.

Also missing is the interpretation of collected data in a wider Mediterranean perspective. The PC1 score, correlated to CO3, salinity, pH and Talk, is impressively similar to Oviedo et al. (2015) results, who investigated living coccolithophores in the whole Mediterranean Sea. CO3, salinity and pH were the main environmental factors to explain living heterococcolithophore distribution. This comparison should be emphasized and strengthen surface sediment results in the present paper. Authors argue that salinity was ruled out as an important environmental factor by Oviedo et al. (2015), because Emiliania huxleyi tolerates a wide salinity range (page 3772, lines 22-26). Indeed in the Oviedo et al. (2015) paper it is stated that salinity reflects different water masses where coccolithophore species and groups may preferentially live (see section 5.3.1, Noelaerhabdaceae as tracers of Atlantic water inflow). It is exactly what can further improve interpretation and discussion.

Specific comments: Page 3762, Line 12: you are referring to Incarbona et al. (2008).

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

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

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.

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).

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).

Page 3772, Line 2: add Knappertsbusch (1993) and Oviedo et al. (2015) among references.

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).

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.

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

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

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 mismatch 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.

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.

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