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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 #1

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This manuscript introduces a new method of sea surface salinity reconstruction in the western Mediterranean Sea based on coccolith assemblages. Within this area (the Alboran Sea), changes in Atlantic and Mediterranean inflow and outflow, respectively, determine nowadays to a high extent the horizontal and vertical distribution of salinity in the upper and intermediate water layers. The resulting transfer function is applied to a high resolution micropaleontological time-series previously published by the main author (Ausin et al., paleo3, 2015), in order to infer past SSS variability in the Alboran Sea throughout the last 25 kyr. The reconstructions are discussed in view of other quantitative and semi-quantitative reconstructions of oceanic (SSTs) and hydrological (precipitations) conditions from nearby sediment archives. The core of the manuscript is about the setting-up and evaluation of the transfer functions. This is by no doubt the strength of this paper, given the thorough use by B. Ausin of a wide array of statistical techniques to validate the transfer functions. Most of the statistical tests are well detailed, relevant to the purpose of critically assessing the reconstruction technics, and sustained by the ad-hoc literature. Although not a specialist on the various transfer function technics, I am rather convinced by the approach used by the authors to relate the environmental variables to the coccolith assemblages. The weighted average (WA-PLS2) and best analog technics (MAT) have been widely and successfully applied in previous works on late Quaternary planktonic faunal or floral assemblages but never, to my knowledge, to coccolith assemblages. The amplitude of reconstructed SSS changes (core CEUTA10PC08) across the deglaciation and early mid-Holocene as well as the range of SSS values across the whole studied time period look more accurate that those reconstructed previously using alternative technics such as coupled d18O – Uk37 data. The use of a regional surface sediment dataset distributed on both side of Gibraltar strait seems relevant given the period investigated and the oceanic circulation at play , and partly explains the non-analog situation which hampers quantitative reconstructions for the LGM ,H1 and H2 intervals of the sedimentary section, as shown by the authors.

I have few comments/questions, mostly on the downcore SSS reconstructions (chapter 4.3 and 4.4) which need some answers from the authors, if not a substantial rewriting of these final sections. I must also emphasize that English language should be checked by a native English speaker, mostly within chapter 4.

- 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)?

- 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”.

- 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 Meditteranean Sea. The 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.

- 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).

- section 4.4, lines 410-412. Useless (and poor English wording). Rather conclude section 4.3 with a s sentence stating that the 25-16 ka part of the record will not be discussed further.

- 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.

- 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 definitively be discussed by the authors.

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Interactive Comment