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Climate of the Past Discussions

Interactive comment on "Variations of Mediterranean–Atlantic exchange across the late Pliocene climate transition" *by* Ángela García-Gallardo et al.

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We thank Referee 1 for valuable comments on our manuscript. In the following lines we address the comments and suggestions. Please find our response below each comment. Major concerns in the annotated manuscript have been copied and addressed in this letter.

Referee 1: "I am confused by the biostratigraphy/biochronology you are indicating for Figure 2. I assume the use of "top" and LO are interchangeable? You claim the stratigraphic framework is established based upon indicated biostratigraphic tie points and visual correlation of the d18O records. Are the dates you provide for D. pentaradiatus

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and D. surculus at Site 978 determined from your isotopic correlation or are they based upon published dates for these bioevents? You appear to place more weight on D. surculus than you do on D. pentaradiatus but don't comment on it. There are equally reasonable correlations between the upper part of Site 978 and the Rossello section based upon D. surculus as a tie point and assuming LO of D. pentaradiatus to be diachronous. I think you need to provide more detail on how the biochronology relates to the stratigraphic framework, otherwise why show those events?"

Related comment in the manuscript: "You show other biostratigraphic events at 978 and the Rossello section but don't mention them in the text. Either D. pentaradiatus or D. surculus is out of place in one of the sites based upon Figure 2. The correlation between the isotope records at the upper part of 978 is not unambiguous and other visual correlations could be made. I think you need to talk more about the age model for this upper part of 972 and how potential alternative correlations might effect your paleoceanographic interpretation".

Author's response: We agree Referee 1 about the discrepancy between datums from D. surculus and D. pentaradiatus. In our case, while the LOs of D. surculus and D. pentaradiatus are distinguishable at the Rossello section (separated by 30-40 kyrs), this is not the case at ODP 978, although a narrow range of ca. 150 cm was specified in Comas et al. (1996). Due to problems with reworking, Bukry (1973) did not distinguish between the disappearances of both species in DSDP Leg 32 sites. A similar case was reported by Backman et al. (2012), who found a series of problems with the LO of D. surculus in the discussion of biozone CNPL5. In our case, we interpret that D. pentaradiatus is authochthonous while D. surculus has been reworked in ODP Site 978. Thus the LO of D. pentaradiatus appears in general more reliable than the LO of D. surculus and the LO of D. pentaradiatus corresponds to what we observe at ODP 978.

For this reason, we do not agree with the comment from the referee stating that the correlation of the upper ODP 978 is not unambiguous. The top of both records is

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clearly delimited by the LO of D. pentaradiatus, while the lower part is delimited by the LO of D. tamalis at 2.78 Ma in ODP Site 978 and the FO of the foraminifer N. atlantica (sin) at 2.72 Ma in the Rossello Section. Thus there is a period of 0.06 Myrs which should not be correlated to the Rosello section. If ODP Site 978 goes from 2.78 to 2.52 Ma, we are looking at 2.6 Ma in around 50 m of core, which would be equivalent to 0.05 Ma/10 m. That means that the visual correlation should start around 10 m topward from the D. tamalis event at 2.78 Ma. Given the similar number of cycles of both records, not many more solutions are possible. We tried different possibilities and this one is the most likely and best in agreement with the LO of D. pentaradiatus.

Certainly, this issue was not clarified in the text and will be addressed in the revised manuscript.

Referee 1: "Your interpretation of the pollen data indicating warmer and wetter conditions in the Mediterranean during most of the Pliocene is at odds with several previous studies. My understanding of these data is that the NW Mediterranean and Europe were wetter than present day but the SW Med was basically the same as or drier than the region is today. I've provided further comment on the annotated manuscript."

Related comment in the manuscript: "There are different conditions in the North and south. NW Europe and NW Mediterranean were wetter than today but data from the SW Mediterranean were drier or maybe the same as today. I also think you need to look at/cite Fauquette 1999 and 2007."

Author's response: Fauquette et al. (1999) found large amounts of Quercus pollen at middle elevations of Andalucia (S Spain). This genus indicate humid conditions (Combourieu-Nebout and Vergnaud-Grazzini, 1991). However, Fauquette et al. (1999) exclude the Quercus data from the Andalucia dataset resulting in an indication of similar or drier conditions compared to the present-day in the Alboran Sea, to which reviewer 1 is referring. Fauquette et al. (2007) show a record extending only until 3.5 Ma, out of our interval of study (3.33-2.60 Myrs) reaching similar conclusions.

Nevertheless, in spite of those two particular cases in which the Quercus data were extracted, warm and very humid conditions have been reported in the W Mediterranean during the late Pliocene (e.g. Fauquette et al., 1998; Béthoux and Pierre, 1999; Bertini, 2010; Combourieu-Nebout et al., 2015) which it is in turn corroborated with the presence of sapropels in the W Mediterranean linked to precession minima/monsoon maxima (Rohling, 1994). The occurrence of sapropels has been highly related to shifts toward lighter surface water d18O values due to enhanced runoff (Vergnaud-Grazzini et al., 1977; Williams et al., 1978; Thunell and Williams, 1989). Therefore, the most reasonable explanation for our depleted-d18O signature in the Alboran Sea during interglacials is that they may likely reflect this general trend toward humid and warm conditions in the W Mediterranean.

Referee 1: "I would avoid using 4-letter genus abbreviations. I'm not sure of CP guidelines on this but it is definitely non-standard and not necessary with the taxa you are using."

Author's response: We will decline using the 4-letter abbreviation but we keep a distinction between both genera Globigerina and Globigerinoides using Gs. in the last case. The 2-letter genus abbreviation has been extensively used in previous studies.

Referee 1: "Make sure all values have units associated. For instance, you report SST as °C but salinity with no unit at all."

Author's response: The issue about units is controversial and could result confusing or not appropriate depending on conventions. However, we consider that salinity is a dimensionless parameter according to UNESCO (1985) since it expresses the ratio between electrical conductivity of a seawater sample and electrical conductivity of a standard. For instance, the salinities indicated in the manuscript have been adopted from previous research (Millot, 1999, 2013; Rhein, 1995) in which no units were assigned to salinities, or expressed in psu (Bryden et al., 1994) and ‰ (Wüst, 1961), which are dimensionless scales. Hence we propose to keep the values without units.

Referee 1: "The use of Ma and My has gotten confused in recent years. Whatever the CP preferences are you need to follow them consistently. You sometimes use Ma for duration and other times Myrs. This needs to be fixed throughout the text and tables."

Author's response: We consider that we are following the same rule consistently. According to international conventions (see Christie-Blick, 2012), we are using Ma to indicate a point in time, and Myrs to indicate a time span (e.g, intervals) throughout the manuscript.

Referee 1: "The supplementary data are inadequate as provided. A list of ages and values is not sufficient for others to be able to critically assess your findings or reproduce the data. Provide the sample information in standard ODP format in addition to your ages."

Author's response: The referee is right, the table will be corrected.

Referee 1: "Unless CP has changed the instructions to authors, journal names should be abbreviated in the References section."

Author's response: The journal names will be revised in the manuscript.

In relation to comments and minor revisions on the annotated manuscript, corresponding changes will be incorporated in the revised version accordingly.

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