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Climate of the Past
Discussions

Interactive comment on "Enhanced western Mediterranean rainfall during past interglacials driven by North Atlantic pressure changes" *by* Yama Dixit et al.

Anonymous Referee #2

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The manuscript submitted by Dixit et al to the Climate of the Past journal is a revised version of a manuscript that I reviewed for another journal almost two years ago, and I must say that several of my concerns have not been suitably addressed. Dixit et al. present Mg/Ca and Ba/Ca-based reconstructions of sea surface temperature (SST) and salinity, respectively, for the Tyrrhenian Sea during the last three terminations (TI, TII en TIII) and peak interglacials of MIS 1, MIS 5e, MIS 7e and MIS 7c. From these reconstructions the authors infer changes in Golo River runoff that they interpret to indicate changes in winter rainfall over the northern Mediterranean basin. They observe that the long-term amplitude of the salinity decrease tightly follows eccentricity. They also find that during SST warming putative increases in winter rainfall

coincide with increases of African monsoon, increase of Nile River runoff in summer, both developing well-stratified column waters, periods of anoxia and sapropel layers. A comparison of these results with model simulations for the mid-Holocene allows the authors to support the idea of an increased southwesterly moisture transport into the western Mediterranean from the North Atlantic. The first observation is new, and it is interesting to know the factor that may modulate the amplitude of long-term changes in salinity in the Mediterranean Sea; the second finding is not new but supports previous studies explaining sapropel formation (Toucanne et al., 2015, QSR; Grant et al., 2016, QSR) and the origin of the western Mediterranean rainfall during the mid-Holocene (Brayshaw et al., 2011, The Holocene). Therefore, I do not see the real contribution of this manuscript. Moreover, interpreting changes in runoff as direct evidence of changes in seasonal (winter) rainfall seems to me to be inappropriate. Changes in runoff can be the result of changes in vegetation cover with increased runoff during late summer/early autumn in the Mediterranean associated with more erosion, i.e. less forest cover (Durán Zuazo and Rodríguez Pleguezuelo, 2008, Agronomy for Sustainable Development). Additionally, changes in salinity can also result from changes in precipitation-evaporation balance. These issues are not sufficiently discussed in the manuscript. Based on previous research in the Mediterranean region, they state that other proxies (pollen and speleothems) are "unable to offer direct insights on the variability in winter rainfall". Contrary to this statement and as far as pollen studies are concerned, we know that present-day changes in Mediterranean forest cover depend on the North Atlantic Oscillation shifts, i.e. on the position and intensity of the westerlies, that in turn control winter precipitations in Europe (Gouveia et al., 2008, Int. J. of Climatology). Therefore, pollen-based Mediterranean forest cover changes are direct evidence of changes in winter precipitation as repeatedly shown by data (Fletcher and Sanchez Goñi, 2008, Quat. Res.) and model–data comparisons for different interglacials of the last 800,000 years (Peyron et al., 2017, Climate of the Past; Oliveira et al., 2018, Climate Dynamics). Moreover, some of these records have allowed for quantitative reconstructions of winter precipitation for TI and the peak of MIS 1 (Fletcher et al., 2010, Climate of the Past; Peyron et al., 2017). I am surprised by the fact that the authors refer to some of these papers in the Discussion section to support their interpretation after criticizing such an approach in the Introduction. Moreover, they justify their work by the inability of this proxy to reconstruct winter rainfall. Throughout the manuscript the authors are not consistent when they refer to the region of precipitation. Sometimes they refer to northern Mediterranean rainfall, at other times to western Mediterranean rainfall, and they discuss records coming from the east and to the west of this region. This inconsistency is problematic as several studies show that climate during the Holocene in the Mediterranean region presents west-east and north-south gradients (e.g. Dormoy et al., 2009, Climate of the Past). I am also concerned by how the authors deal with the timing of Terminations. Terminations are intervals from glacial to interglacial states that generally last a few thousand years and are not events (midpoints) as suggested by Dixit et al. (dashed line in Figures 2 and 3). Terminations should be identified from the δ18O of benthic foraminifera, and they are triggered by a combination of ice volume and orbital parameters (Parrenin and Paillard, 2012, Climate of the Past). Cheng et al. (2009, Science) established the timing of marine oxygen-isotope terminations (δ18O of benthic foraminifera) by correlating North Atlantic ice rafted debris (IRD) to radiometrically dated oxygen-isotope cave records from China. The timings of the onset and end of Terminations I, II and III are 18-11 ka, 138-129 ka, 251-243 ka, respectively, and the timing of the midpoint terminations are 14.5 ka, 131 ka, 247 ka. These accurate measurements of the timing of terminations do not coincide with the dates given by Dixit et al. The authors say that the three terminations are centered on 11, 129 and 243 ka, but they do not specify how they established them. With respect to this issue, I invite the authors to look at the recent paper by Barker et al. 2019 in Paleoceanography and Paleoclimatology. Overall, the organization of the manuscript and the order of the figures should be changed, and the English improved. For instance, the environmental setting and the studied material are explained twice: at the end of the introduction and at the beginning of the Material and Methods. The introduction should be more focused and clearly explain the gap this work aims to fill and

justify the interest of working and comparing the MIS 1, MIS 5e and MIS 7e and MIS 7c warm periods and the related Terminations. Adding model simulations of precipitation changes during contrasting MIS 5e, MIS 7c and MIS 7d interglacials could be relevant. The subsection "Proxy systematics" should be moved to the Material and Methods section. Furthermore, I do not understand the meaning of "systematics" in this context. Figure 3 in which all the results from this study are shown should be Figure 2. Figure 2 is only displaying different records covering TII and MIS 5e in the Mediterranean. Additionally, I have found many inconsistencies throughout the manuscript, sentences difficult to understand, and several typographic mistakes (see below other comments). In the conclusion section I have one major concern related to the following sentence: "Proxy data placed on a globally synchronous timescale demonstrate that the intensity of the precession-controlled wintertime rainfall. . .": What do the authors mean by "globally synchronous timescale"? How have the authors harmonized the different paleoclimatic records presented in the work: GDEC-4-2, ODP sites 975 and 976, Corchia and Tana Urla speleothems, and the Greek pollen record? The Chronology section is confusing and focuses on how Marino et al. (2015) have dated ODP site 975. The authors only provide in Table S3 (supplementary information) the common age control points between GDEC-4-2 and ODP 975 for TII and MIS 5e, but they do not refer to the related stratigraphic events. What are the control points for dating TI, TIII and the MIS 1, MIS 5e, MIS 7e, and MIS 7c warming peaks? Based on the issues detailed above, I recommend major revisions before any potential publication.

Other comments Page 2, line 25 – But Toucanne et al. (2015) suggested that the enhanced rainfall in the western Mediterranean during warm periods of the last interglacial was regional and due to the intensification of winter Mediterranean storm tracks.

Page 9, line 14 - The authors describe the paleoclimatic data of MIS 6/5 and MIS 2/1 transitions related to HS11 and HS1, respectively, but not those of MIS 8/7 and MIS 7d/7c that, although not related to Heinrich events, are associated with IRD pulses as shown by ODP 980 (McManus et al., 1999, Science). In Figure 3 (to be renamed

Figure 2) the authors should add the intervals of IRD deposits.

Page 3, line 5; Page 4, lines 25-26 – Why do the authors cite Raislback et al., 2015 for MIS 5 and MIS 5e and not for MIS 7c and MIS 7e?

Page 3, lines 5-6 – Fletcher and Sánchez Goñi (2008) article presents a marine pollen sequence. Therefore, it is not a lacustrine record as indicated by Dixit et al.

Page 4, lines 1-11 – The authors should improve Figure 1 by representing the hydrography affecting GDEC-4-2 site.

Page 5, line 11 – Add a "," after "sporadically" and "to" before "obtain".

Page 5, line 26; legend of Figure 3 – Replace "precession" with "precision".

Page 6, lines 22-23 – Delete this sentence. It is a repetition of lines 19-21.

Page 6, lines 23-25 – Water temperatures can be also decoupled from local atmospheric temperatures during periods of ice-growth (Sanchez Goñi et al., 2013, Nat. Geosci.), not only during periods of high river discharges.

Page 7, line 12 – Modify the following sentence ". . .a reduced concentration of atmospheric concentrations. . .".

Page 7, line 19 – Replace "isotopes" with "record".

Page 8 – lines 10-12 – Rephrase this sentence, and move all section 3.1 to section 2 "Material and Methods".

Page 9, line 4 – Add "," before "respectively".

Page 9, lines 5-6 – Delete "Ocean Drilling Program" and "s" from "Sites".

Page 9, line 12 – Replace "ice-sheet" with "iceberg".

Page 9, line 22 – Figure S4 is not necessary. The same information is presented in Figure 3.

Page 10, lines 13-14 – Contrary to authors' statement, d18Osw values are not shown for site LC21 in Figure 2g. Only d18O values of G. ruber are represented.

Page 10, line 13 – Delete "s" from "sites".

Page 10, lines 14-16 – Add "through the Nile river".

Page 10, line 18 – Delete "at".

Page 10, lines 21-22 – Delete "and sea level changes".

Page 10, lines 22-26 – Please rephrase this sentence.

Page 11, lines 3 and 27 – Replace "Fig. 3f" with "Fig. 3e".

Page 12, line 2 - Replace "Fig. 3f" with "Fig. 3e".

Page 12, line 16 – Add "during winter" after "North Atlantic region".

Page 12, line 24 – Amore et al, 2012 is not listed in the references.

Pge 12, line 27 – The "increased precipitation" of the mid-Holocene occurred in winter?

Page 14, line 3 – Delete "direct". The geochemical evidence presented by the authors is not a direct evidence for winter precipitation.

Page 15, line 4 – Replace "analysis" with "analyses".

Page 26, line 3 – Replace ". . .for the last MIS 5e" with "for TII and MIS 5e".

Page 26, lines 7-8 – From where do pollen records come?

Page 26, lines 9-10 – Vertical orange and yellow bars do not indicate "warmer substages of the last interglacial". Both bars are within the Marine Isotopic Substage 5e.

Page 27, lines 7-8 – Contrary to the authors' statement, vertical orange and yellow bars do not indicate stadials during MIS 1 and MIS 5. Also there is no orange bar. Please harmonize the colors between figures 2 and 3.

In Figure 2, the blue band indicating the HS1 should be enlarged and start at 18 ka and not at 17 ka as shown.

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