

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

Anonymous Referee #1

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The paper from Dixit et al present trace elements (Ba/Ca and Mg/Ca) and stable oxygen isotope composition from planktic foraminifera (*G. bulloides*) from the previously published marine core GDEC-4-2 from the Corsica margin. The data cover the Holocene, the interglacials MIS5 and MIS7 and the glacial termination TI, TII, TIII. Sea surface temperature were obtained from Mg/Ca data and used together with $\delta^{18}\text{O}$ calcite data to calculate $\delta^{18}\text{O}$ of sea water. Ba/Ca is used as a proxy for Golo River discharge and, through calibration using the modern sea surface salinity (SSS)-Ba/Ca relationship, the authors attempt to quantitatively reconstruct past SSS. Using these data, and by comparison with several records (both marine and continental) from the Mediterranean, the authors suggest that the three interglacial were characterized by

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an increase in winter precipitation driven by changes in North Atlantic storm tracks trajectories, in turn modulated by changes in precession and eccentricity. To support their hypothesis, the authors also analysed outputs from modelling experiments (PMIP3) for the pre-industrial Holocene. Authors also suggests that these increases in precipitation contributed to trigger basin anoxia and sapropel deposition. The presented data are of interest and their interpretation as paleo-rainfall variability proxies is reasonable. However, main text, figures and supplementary material are rather confused and misleading in some points, references are not updated and often messed-up, the comparison with the model almost useless and the whole discussion is a bit inconsistent and not fully supported by the data. Moreover, the main findings of the paper add very few to what was already proposed in the original paper on the same core (Toucanne et al., 2015). Thus, I suggest publication in Climate of the Past only after careful major revision.

Main Points: One of the main claims of the paper is that variations in winter precipitation are modulated by eccentricity changes. I found this claim a bit obvious. Indeed, it is well known from both data and modelling experiments that winter precipitation in the Mediterranean are mostly modulated by precession changes (e.g. Tzedakis et al., 2007; Milner et al., 2012; Toucanne et al., 2015; Regattieri et al., 2015; Bosmans et al., 2015, just to quote some, but there are others). The intensity of the precession forcing relate to changes in eccentricity, with higher eccentricity inducing higher precession forcing and lower eccentricity reducing the influence of this orbital parameter. Honestly, I do not see any reason to decouple the forcing related to precession to that related to eccentricity. Also, the importance of obliquity changes is not mentioned at all, while several works (se e.g. Bosmans et al., 2015 and references therein) have showed that it has an impact on the Mediterranean hydroclimate. I think that authors have to largely re-focus the discussion, better explaining the relationship between eccentricity and precession and taking into account the influence of obliquity changes. To this end, I suggest that they have a detailed look to Bosmans et al (2015) results. Another very weak point is the claim that the findings of the paper are supported by modelling results. I found this part confused and even misleading. First because the

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model output is related only to the mid-Holocene, so results cannot be extended to others interglacials where the boundary conditions were so different, second because the whole discussion about changes in storm tracks trajectories and NAO like atmospheric patterns are not supported, to me, neither by the data or by the modelling experiment that they present. I can agree that the Mid-Holocene experiments shows a southern shift in westerly trajectories which can resemble a NAO- pattern, but I do not see any reason to extend this interpretation to the other considered periods. The most likely mechanism for increased precipitation during precession minima, basing on available data and literature, is related to changes in Mediterranean-sourced precipitation due to increased Med heat content. Indeed, during precession minima hydrology in the Med is influenced by low-latitudes atmospheric patterns: the northward shift of the ITCZ causes stronger summer drought related to the descendent branch of the Hadley cell. It causes an increase in the Mediterranean summer heat content. High summer temperatures lead to elevated sea-surface temperatures and associated high evaporation levels persisting well into the year, contributing to the formation of depressions across the northern borderlands, strengthening cyclogenesis within the basin and causing an increase in autumn-winter precipitation. This is what has been proposed also basing on GDEC data by Toucanne et al just few years ago, and I do not understand why authors now invoke a completely different mechanism. . . . The whole discussion about the model experiment is very confused, do not add nothing to the interpretation and do not support what the paper claims. I suggest to largely modify section 4.2 trying to explain the mechanisms more relying on the presented data and on previous literature. They should briefly review mechanisms proposed by e.g. Tzedakis et al. (2007) or by Milner et al. (2012) or by Bosmans et al. (2015) and especially by Toucanne et al., 2015, trying to better highlight which one best fits with their results. To me, this whole part about modelling is an, almost failed, attempt to add something new to the -good- explanations already proposed by Toucanne et al. Authors should be more "honest" with that in the sense that they should clearly state that this work is an update of the previous one and that the new data and results strengthen the previous interpretation, without striving

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to introduce new and confused mechanisms for that. Specific points: Abstract: P. 1 line 22: North Atlantic climatic processes is rather vague, do the authors refer to atmospheric patterns? or to oceanic circulation? P1 line 23: (but also elsewhere, see above and below) Summer monsoon rainfall does not reach directly the Mediterranean Basin. As this sentence stands now, it seems that monsoon directly contribute to Mediterranean precipitation. I agree that monsoon rain contribute to Mediterranean Sea water through Nile (and fossil river system from N Africa) discharge, but it has to be clearly explained. p1 line 25 across the past 1-Introduction: P2 line 16 Hydrological not hydrologic p3 line 1 There is a typo in interglacials p3-line 3-9 this part reads odd. Please rephrase. I guess the words between "Mediterranean" and "for" should be moved after "(Railsback et al., 2015)", also it is not clear which papers refer to Holocene and which to the LIG (e.g. Zanchetta et al., 2007 is Holocene, not LIG). p3- line 13 because THE of a lack p3 line 20 to the end of the section: it should be moved in a paragraph of site description or in material and methods, it is not introduction. IMPORTANT: a sentence clearly explaining the aim of the paper is missing from the introduction, please add it at the end.

2 Material and methods p4 line 14 GDEC is WAS RECOVERED from P4 line 18-20 Rephrase, a sediment core cannot capture variation in storm track (sediment properties yes, but it should be better explained). 2.1 Stable isotope analyses Which was the previous resolution of stable isotope analyses? which is the new one? There are not enough details about analytical method (i.e. which calibration method has been used?, which is the reaction time? If analytical methods were the same as in the Toucanne et al paper, it should be stated clearly. 2.2 Trace elements analyses Add the resolution (spatial) at which these analyses were done. p5 line 12 proxy data OBTAINED FROM IT are representative. . . . p5 line 13 30 m, and are reflect THUS REFLECTING surface. . . . p5 line 25 TO CHECK FOR INTERNAL CONSISTENCY recurrent analyses. . . . p5 line 26 precession PRECISION p6 line 13 "comparable" to the TO THAT OBSERVED IN previous studies p6 line 15 Here it is stated that others core top in the Mediterranean have lower Ba/Ca values so it is not clear if Ba/Ca values observed here are compa-

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rable or not with previous studies. . . . Also the sentence about calibration for used to infer temperature from Mg/Ca (line19-21) should be better separated by the discussion about Ba/Ca and better motivated. It is the same calibration used in previous studies in the region or not? (i.e. from where the McConnell calibration comes from?) p6 line 22-23 for this region of the Mediterranean P6 line 24 generally p6 line 26 records TO for our core site that constrain river runoff AT OUR SITE 2.3 PMIP3 model simulation See general comments, it is a non-sense to use a mid-Holocene simulations to infer mechanisms working for other interglacials characterised by different boundary conditions. I would almost remove this part. . . .please instead consider modelling results from Bosmans et al. 2015 paper. 2.4 Chronology As the GDEC record has been published already and now the chronology is updated by aligning to the Marino et al. (2015) curve I suggest the authors to quantify the difference with the previously published record. Also, associated uncertainty to the new chronology has to be stated clearly. Last, at the end of this paragraph authors should insert the resulting temporal resolution for both the stable isotope and the trace element records. p7 line 25: to exploit EXPLOITING

3 Results 3.1 Proxy systematics p.8 line 10 “ $\delta^{18}\text{O}$ OF in foraminifera” and “and BY $\delta^{18}\text{O}_{\text{sw}}$ ”. Also, you should put a reference here and also quote Fig. S3. p.8 line 14. Sentence not clear. Also the $\delta^{18}\text{O}$ of the river water is related to P/E ratio, not only the $\delta^{18}\text{O}$ of the sea water. At the end of the page you should quote the relative supplementary text and figure. 3.2 Sea Water oxygen isotope and Mg/Ca based SSTs p.9 line 2 highER, not high and also quote a figure after periods p.9 line 4 and THESE INTERVALS ARE also characterized p.9 line 5-6 This is not results but discussion already (see also comments to Figure) p.9 line 16 BP, with AND BY lowest values Authors should briefly comment here about MIS7 temperatures and removing the relative paragraph, which is really confused, from the supplementary material (see specific comments to Supp Mat). 3.3 Precipitation and salinity changes inferred from foraminifera Ba/Ca p.9 line 27 Why the increase abundance of benthic foraminifera indicates an increase of OM transportation to the bottom? 4 Discussion p10 line 11 Last Interglacial (here and after) p10 line 15 which delivered DELIVERING p.10 line 16 As above, you should

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specify that the monsoonal rain is delivered by the Nile and by -now fossil- river system in the North Africa p10 line 20 waters FROM WHICH the foraminifera calcite FORMS p10 line 25 increased LOCAL precipitation p11 line 4 put a comma after Ba/Ca and another one after MIS5e p11 line 5 synchronous TO WETTER CONDITIONS INFERRED p11 line 7 as above, Zanchetta et al., 2007 is Holocene and not LIG, I guess Regattieri et al., is 2014 or 2017 and not 2015. Lines 9-10 are a repetition of lines 5-6. p11 line 8 The source effect on continental calcite in the Mediterranean has suggested to be strong during glacial to interglacial transition, but less important during interglacial period (see e.g. Tzedakis et al., 2018 or Regattieri et al., 2019) p11 line 14 What does “regional sedimentary signal means”??? p11 line 17 Tzedakis et al, 2007 does not report any Holocene pollen record showing higher seasonality and for should be FROM sites In general, what is new in this paragraph with respect to the Toucanne et al paper????? The whole discussion about eccentricity influence is a non-sense, as the amplitude of the precession forcing is directly related to eccentricity!

4.2 Proposed mechanism for high Mediterranean winter precipitation during interglacial See general comment. I would largely remove this part. . . .

4.3 Contribution of western Mediterranean precipitation in sapropel deposition (in should be TO instead of in) Toucanne et al paper's speaks about an increase of western Mediterranean storm track, not about an increase in North Atlantic sourced precipitation during period of sapropel deposition. I agree that wMed precipitation play a role in triggering anoxia and sapropel deposition and I do not support as well the Rohling hypothesis. However, this part is very confused and I do not see any reason to invoke an increase of moisture transport from the North Atlantic. This claim is not supported by the references provided in lines 19-20, nor by the new presented data, and is in contrast with what already proposed basing on GDEC data. p14 line 1 there's a typo in supported (or proposed?) p14 line 12 how mid-latitude storm tracks can contribute to organic fluxes? this sentence has no sense. Conclusion: they need to be largely rewritten following provided comments. Figures They are all rather poorly constructed

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in my opinion and need to be largely modified. I suggest to prepare a proper results figure showing only the results from GDEC for all the period discussed (this should be fig. 2 not 3), then to make others figures with the three intervals separated and where the records used for comparison have to be shown. Please enlarge all the figure and be sure that axes's values are appropriated. Figure 4 is useless in my opinion, all the mentioned sites needs to be shown in fig. 1 If you want to show Corchia data please use the updated record provided by Tzedakis et al., 2018 Fig. 1 The line indicating the Mediterranean storm tracks has no sense, this line may resemble the major trajectory of North Atlantic storm track, but it seems to me an over simplification. Argentarola cave is not mentioned in the text, why it is mentioned here? From where the position of the ITCZ comes from? again it seems poor and over simplified. Please put all the reference for the terrestrial and marine sites in the caption of Figure 1, this would avoid the whole first paragraph of supplementary text, which is really confused and not useful at all. Fig.2 It should report only the results from GDEC, whereas all the other records used for comparison should be moved to another figure (fig.3)

Supplementary information The first two paragraph (regarding the records used for comparison and the one regarding the MIS7 temperature, should be shorten and accommodated in the main text and in figure captions as indicated in previous comments. Fig. S5: Why there are only 3 points if in table s5 five sampling points are reported? The high correlation coefficient reported is simply an artefact due to the very limited number of points!

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