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Climate of the Past Discussions

# *Interactive comment on* "Sea-surface salinity variations in the Northern Caribbean Sea across the mid-Pleistocene transition" *by* S. Sepulcre et al.

**Anonymous Referee #2**

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Please find below, my review for the manuscript entitled "Sea surface salinity variations in the northern Caribbean Sea across the mid-Pleistocene transition, by Sepulcre, Tachikawa, Rostek and Bard. In general the manuscript is well written, contains a sufficient number of figures of good quality with informative captions. References appear applicable and sufficient. I find the paper suitable for publication after some technical revisions. In particular the manuscript would benefit if the discrepancies between different SST records were discussed and uncertainty on the Dd18O calculation would be taken into account arising using different SST records. In addition some replicate samples of d18O G. ruber should be given to increase confidence in the estimation of the mean d18O. I ask the authors to carefully consider my suggestions and recom-



mendations when producing a revised version. Comments and suggestions on where to improve the manuscript are given below:

Summary This manuscript aims to document the changes in the tropical western Atlantic Ocean, i.e. in the Caribbean, in response to the Mid-Pleistocene climatic Transition (MPT). Alkenone temperature data and planktic foraminifer d18O data are presented of an almost 1 million year spanning marine record, that is core MD03-2628 and used, in combination with published estimates of changes in ice volume, to obtain an estimate of past changes in regional Dd18O, which are interpreted as changes in regional Sea Surface Salinity (SSS). Results obtained are compared to previous d18O and SST estimates from cores in the region and interpreted in the context of changes in the hydrological cycle, shifts in the Inter Tropical Convergence Zone as well as changes in the Atlantic Meridional Overturning Circulation (AMOC). The authors find higher Dd18O values during all glacial periods, indicating higher SSS. Interglacials, over the past 450 kyr, however, appear to have lower Dd18O minima compared to interglacial periods between 940 kyr (base of record) to 650 kyr (500 kyr?). The lower interglacial salinities over the past 450 kyr indicate a northernmost ITCZ location, which, according to the authors, is forced by changes in the interhemispheric temperature gradient associated with the poleward position of southern oceanic fronts. It is suggested that a permanent link existed between the tropical salinity budget and the AMOC over the past 940 kyr.

Comments and suggestions: The introduction and overview of the modern climatology is informative and applicable. Primary information concerning the length of the core, sedimentology, core scanner data etc., however, is largely lacking. Also in the older publication on the same core Sepulcre et al., (2009) there is scarce information on age model, (tie points) and sedimentology. I find it rather surprising such information is not included, especially since such information is rather crucial for a long core like this in tracing for example potential hiatuses. Hiatuses are often clearly visible from the sedimentology or in combination with core scanner data. Why not show core scanner data?

Such data are often produced on the ship after core collection. This would certainly help to increase the confidence in the archive as a whole and age assignment of the record (a figure would be very helpful). Maybe this has been discussed elsewhere and I missed this? If so, references should be included.

Concerning the data presented in this ms.: I do have a bit of a problem with the present paper; that is, the sampling resolution is extremely low. The core has been sampled every 20 cm, and given a total length of the core of slightly less that 2700cm (Sepulcre et al., 2009), the average sed. rate is 2.9 cm/kyr. This means the average time between two successive samples is about 7 kyr. Clearly this coarse resolution has a trade off: Some glacials are covered by only 1 or 2 samples. i.e. stage MIS 8, 10, 12, 16, 18, 20, 22, 24(?), and the age model therefore is likely not too well constrained, especially when it comes to details. Some of the basic information on the age model could be briefly shown (small figure and table) in the present paper. Concerning the Dd18O calculation: I do think quite a large error may be associated with the Dd18O calculation for several reasons: 1) The coarse sampling resolution likely resulted in a relatively poor age assignment, which actually is needed since this determines the size of the ice volume correction at any given point. So how can one be sure that the ice volume correction is indeed correct? Potentially an estimate of the maximum and mininum value for each data-point could be given based on the uncertainty in age. 2) The second source of error in the Dd18O calculation is introduced when applying the alkenone based SST correction. I would have a bit more confidence in the results if other SST data would have given very similar results, but this is not the case at all; the Mg/Ca data from core ODP999A (Schmidt et al., 2006), and in particular the faunal transfer function data from the same core give different results. The transfer function data show rather constant values all over this core down to approx 320 kyr. In the older part of this record there is some variability but this SST record again looks very different from the MD03-2628 alkenone record. The authors use their alkenone SST's to obtain Dd18O, not taking into account the potential effect that calcification temperatures of Globigerinoides ruber could have been quite different from the alkenone temperatures as they



may have been produced during very different seasons and hence temperatures. The authors could potentially show how much the Dd18O estimates would vary if the other SST proxy records (core ODP999A) were used in the calculation. I have been thinking very hard how much of a problem the relatively coarse sampling resolution actually is in the light of the conclusions drawn which are the following: 1) Glacials appear to have higher Dd18O values compared to interglacial periods indicating increased SSS, and 2) younger interglacial Dd18O values (younger than 450kyr) are lower compared to interglacial periods older than 650 kyr (I would say older than approx. 500 kyr judging from panel 7 c). Given that these conclusions are very general they may justify the coarse resolution provided that using the other SST proxy records basically will yield similar results. I think the error presented on the Dd18O reconstruction is actually much larger if age model uncertainty would be included (ice volume correction uncertainty) in combination with error quantification resulting from use of the other SST proxy records. I would be more convinced in the reconstruction if the authors would also take these uncertainties into account and encourage them to quantify this more realistically.

Finally, I think the relationship between the Dd18O changes and the AMOC is speculative; I think the data just appear to be in line with this general view.

P1. Line 19: "At longer time scales…." The start of this sentence suggest the authors aim to say something on the time scale longer than the record. However, the message of this sentence is that younger interglacials, i.e. over the past 450 kyr, however, appear to have lower Dd18O minima compared to interglacial periods between 940 kyr (base of record) to 650 kyr. I suggest rephrasing this sentence.

P1. Line 21: interglacial stages older than 650 appear to have lower Dd18O values compared to interglacials over the past 450 kyr. Judging from Figure 7, interglacial stage 13 and 16 roughly centered at 500 and 580 kyr appear to show also lower Dd18O values compared to the interglacial periods younger than 450kyr? Did I miss something here, or should this be changed?

P2. Line 1: " last five interglacials indicate a northernmost ITCZ location". Although this may offer an explanation, no evidence is presented here. I wonder if an intensification of the ITCZ could be responsible for the observed freshening of the Carribean surface waters for the past five interglacial periods? This may be discussed as an alternative.

P3. Line 1-3. I feel the remark on the purpose of the present study "In this work we sought. . ...", is not well placed here, i.e. in the middle of the literature discussion. Presenting the aim of the study fits better at the end of the Introduction chapter i.e. on page 4 line 7.

P6. Line 8-10. " No clear relationship appears to exist between evaporation and SSS at core location". This statement can be interpreted in two ways, either 1). There are data, which give rise to this statement, i.e. the relationship has been studied and shows that there is no correlation, or 2) there are no data / studies which have investigated this. This should be clarified (in case 1. Refs should be given).

P6. Line 18. I'm not sure if the ITCZ indeed reaches its southernmost position in March (equinox date). Or maybe I should say, I do not understand why this is. I think, the ITCZ generally appears to follow latitude of maximum insolation, which is in December for the southern hemisphere, so I don't get why in March the southernmost position is found as then the sun would be precisely above the equator and, I would expect, the ITCZ is then actively moving southward? Maybe the authors could make a small effort explaining this?

P8. Line 18. "Core MD03-2828. . ." this should be "Core MD03-2628. . ."

P8. Line 24. I doubt if a d18O measurement based on 5-10 specimens results in a robust average especially in the light of this error sensitive application (SSS reconstruction). I would say any opportunity to reduce the error associated with the method should be welcomed. An average of 15-20 specimens or replicate samples would have been better. Given the seasonal range in SST is expected to be relatively small in the Western Atlantic, the authors might have considered the standard error on the d18O



low enough to be acceptable? It would be nice if some replicates could be shown to assess uncertainty in the mean d18Oruber.

Figure 7. Consider stretching this figure a bit, it appears a bit small.

Figure 8. Same comment as for Figure 7.

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Interactive comment on Clim. Past Discuss., 6, 1229, 2010.