

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

Anonymous Referee #1

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Overall, I like this manuscript. It is well written and the discussion is good. Their results show two major conclusions: 1) Caribbean sea surface salinity oscillated between increased glacial salinity and decreased interglacial salinities, and 2) there was a significant shift in the magnitude of interglacial salinity at the MPT. Although the first conclusion is not a new finding and was shown previously by Schmidt et al., (2004), the second conclusion is novel and important. Therefore, I recommend publication after they address my comments.

First, the authors do a nice job of presenting the details of modern climatology at their study site.

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Page 1236, lines 5-10: they state “deeper than 1000 m the Caribbean Sea is filled with NADW.” This is not correct. Between 900-1900 m, Upper NADW mixes with AAIW as well as with Upper Circumpolar Deep water and fills the bottom of the Caribbean from the Caribbean’s deepest passage at a depth of 1900 m. Therefore, bottom waters of the Caribbean are more corrosive during interglacial times as compared to glacial times when Glacial North Atlantic Intermediate water fills the bottom of the Caribbean. This has important implications for Glacial-Interglacial preservational differences (see discussion in Schmidt et al. (2006) in G3). I’m not sure if glacial-interglacial preservation differences affect alkenones, but it makes a big difference in the preservation of calcite in the basin. And because their core site is located at 846 m, it is bathed in very corrosive AAIW today. I would imagine that calcite preservation in their core was much better during glacial intervals when AAIW was replaced by GNAIW. It is worth noting that dissolution can also affect *G. ruber* d18O values, shifting them to more positive values. Therefore, an alternative explanation for the shift to more positive interglacial d18O values before the MPT could simply be the presence of more corrosive AAIW during these earlier interglacials. Do the authors have any evidence of calcite preservation changes before and after the MPT? If so, this data should be presented and discussed in the manuscript.

Why do the authors only use alkenone-based SST estimates? I’m always concerned about combining alkenone-SSTs with foraminiferal d18O_c to calculate d18O_{sw} because it introduces more uncertainty in the results. How do you account for seasonal or depth offsets between when the forams calcify and when the coccolithophorids grow? How can you constrain past changes in the seasonality and/or depth range in each group? It would be nice to see some down-core *G. ruber* Mg/Ca-SST measurements to confirm the core top correlations presented in Section 2.4.

In the Methods, they only used 5-10 individual *G. ruber* shells per d18O analysis. Is this enough to get a solid average for each core interval – I have my doubts. I would think it would take closer to 20-25 individuals to get a good average. Do the authors

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have any data supporting their decision that 5-10 individuals will provide an average of a population of *G. ruber* shells from a single interval?

Also in the Methods, they state they used the Uk37 calibration of Sonzogni (1997) to calculate SST because it was better calibrated for high SSTs. However, I thought the results of Conte et al., (2006) demonstrated the Uk37 index is much less sensitive at higher temperatures because the alkenones are pretty much saturated by 26-27°C, implying that alkenone-SSTs are not ideally suited for calculating past SSTs in the warm pools. So how can simply using a different calibration overcome this problem? This isn't clear to me.

I can't find any discussion of the age model for their records. This needs to be addressed. Age model error is an important consideration when subtracting global $\delta^{18}\text{O}_{\text{sw}}$ change due to changes in continental ice volume. What is the sedimentation rate for this core? This should be clearly stated when they introduce their core site in the manuscript.

On Page 1240, Lines 20-24, they claim previous SST reconstructions for the Caribbean are in agreement with their new alkenone-SST record. However, there are several periods where there is disagreement. For example, during MIS 5b, c and d when the alkenone-SSTs show much warmer temps as compared to Mg/Ca-SST from ODP 999A. Then, during MIS 5e, the Mg/Ca-SSTs are warmer. These temperature differences will have a large impact on the calculated $\delta^{18}\text{O}_{\text{sw}}$ values and need to be considered.

Page 1244, Line 12; "Schmidt et al., 2004" is not an appropriate reference when stating that glacial stages are associated with reduced AMOC.

Page 1245, Lines 12-14, you may want to reword this to say something like, "The ITCZ may have migrated farther northward during the interglacials of the last 451 kyr. . ."

Page 1245, Lines 14-16; an alternate explanation would be that the intensity of the

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ITCZ simply increased after the MPT. Warmer SSTs in the northern tropics could have resulted in more intense atmospheric convection and thus more intense rainfall, resulting in reduced interglacial SSS.

Page 1246, line 16: you may also want to include Schmidt et al. (2006), Nature, v. 443, p. 561-564 in this list of references about rapid climate changes.

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