



EGUsphere, referee comment RC3
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Comment on egusphere-2022-787

Sarah Metcalfe (Referee)

Referee comment on "Millennial hydrological variability in the continental northern Neotropics during Marine Isotope Stages (MISs) 3–2 (59–15 cal ka BP) inferred from sediments of Lake Petén Itzá, Guatemala" by Rodrigo Martínez-Abarca et al., EGU sphere, <https://doi.org/10.5194/egusphere-2022-787-RC3>, 2022

This paper presents the record from the PI-2 core from Peten Itza which extends back to 59 cal ka BP. It is compared with the previously published records from core PI-6 taken slightly further to the west. The results are interpreted primarily in terms of changes in the position of the ITCZ and changes in the North Atlantic that are recorded as Greenland Stadials (generally dry here) and Interstadials (generally wetter). Overall, it is noted that the PI-2 record replicates that from PI-6. The paper is generally thorough and well written, but there isn't really a clear case for why this was worth doing or what, if anything, new was learnt from work on this core which had a higher sedimentation rate than the PI-6 sequence. The last sentence of the Conclusions could, for example come earlier as this is an important point about the value of the higher resolution sequence. The point is made in lines 521-22 about the confirmation of lake response to hydroclimate across sites, but this is not elaborated on. The Conclusions make no reference to anything new. I do recommend that the authors consider making a clearer case for the significance of this paper. I would have thought that this further record of the high variability of MIS3 was also worthy of more comment (again, somewhat mentioned in passing).

5.1 is devoted to comparing this Peten Itza sequence with a number of other records across the region. As noted above, the real focus is on the ITCZ, but there is little recognition that some of the sites used in this comparison are more under the influence of the North American Monsoon than the ITCZ (it is well established that although the ITCZ and NAM are related, this is not a direct relationship). I think this does matter, as does the greater influence of mid-latitude systems (potential sources of winter precipitation) at sites such as Babicora and Patzcuaro (this is mentioned later in lines 446-447). The more westerly sites are also likely to see more influence than the Pacific than the more easterly sites. I just think that the variations across the wider region need more acknowledgement. This also comes in to play in relation to Fig. 5. I found the interpretation of the Babicora record (currently based on Roy et al., 2013) and Patzcuaro (based on Bradbury, 2000) odd, as there is clear evidence that conditions at both sites were still wetter than present around the LGM. At Babicora marked shallowing did not apparently occur until around 15 ka and at Patzcuaro wet conditions persisted in to the

early Holocene, although the diatom flora changes markedly (a change in moisture source has been suggested). There are other references that could be explored. Please review text on this in lines 415-417, possibly drier than earlier, but definitely not dry. Could also reflect on these differences in climatology re the observation in lines 384-386.

There seems to be an inconsistency in the text in 5.1.1. which refers to a more northerly location of the ITCZ, then suggests drier summers and more winter rain and then more anoxia due to deeper water. There is a drier period noted at 55.1 – 53.8 ka. How do you get drier summers if the ITCZ is further north? Is this where the NAM comes in to play? (although there are suggestions that the monsoon was quite strong, at least during interstadials in MIS3). I think part of the problem here is the interpretation of the Bradbury (1997) paper – thinking has moved on quite a lot since that was published.

The text notes that the lithostratigraphic units of Mueller et al. (2010) are applied here (lines 105-106, 172), but I wondered whether the application of these units had been tested independently in any way. There is a suggestion in the Discussion (lines 236-237) that some sort of independent work was done, but this is not explained.

I was not convinced that Fig. 7 was in the right place, it would seem more logical for it to come before the current Fig. 6 which makes the comparisons of millennial change with other records.

Minor corrections:

Line 42 'a drop in mean...'

Line 253 Missing a few words at the end of the line 'the end of MIS3 and the start of MIS2' (as earlier in the text).