Dear Professor Reyes,

I appreciate the patience with the revision process and value the insights from the reviewers. Both reviewers seemed to understand the main goals of the manuscript and supported the main conclusions, and I have tried to follow the spirit of their suggestions to enhance the paper. Their reviews tighten the findings of paper by suggesting new figures, changes in the analyses, and textual clarification.

Regarding review #1, their critiques of the manuscript make sense to me. In considering a revision, I have tried to be more systematic in the naming of the different regions and datasets, and rephrased the wording to make each distinct. I have also developed a site map (new Fig. 1) to show the different regions involved and have updated the text to explain why European data are sometimes included and discussed, such as in line 76 of the Introduction and in line 130 of the Methods. I have replaced Figure 4 (scatter plots) with a table of the main relationships and statistics (Table 1). Finally, Section 4.4 has been revised to better address the broad applicability of the approaches discussed here, including by citing some other recent papers and the methods suggested by review 2. I have also tried to make the minor corrections suggested, although I have also removed some text in response to the reviews and that negated the suggestions.

Regarding review #2, I am very appreciative of the comments from Dr. Hébert. They have been stimulating and helpful. In particular, I am glad to be pointed toward up-to-date methods that can be applied to determine the significant patterns. I had been planning to cite some of these newer papers (e.g., Hébert et al., 2022) even before seeing the review, but I have re-run my analyses incorporating a Gaussian filter in place of the LOESS, developing random surrogate timeseries using 'corit', and used them to test the significance of the correlations as described by Reschke et al. (2019). I had been trying to do a simple version of this approach and was glad to revise with such a well-designed and validated method. Likewise, I re-interpolated the datasets to 100-yr timesteps to reduce concerns about oversampling the underlying data at 50-yr intervals. The changes in the analyses created modest changes to the result, but overall, help to solidify the primary conclusions.

I have also made the following additional revisions:

1) provided a more formal definition of cen-mil variations at the start of section 2 (Methods);
2) removed Figure 4, and replaced it with a new table of statistical results, given the agreement among reviewers about making this figure more concise;
3) updated and clarified Figure 5 (now Figure 6 because of new added figures) by removing the shading and slope uncertainties;
4) adding a new Figure 5 that shows the timescales of significant correlations among the different datasets, which should help address concerns about the meaning of spectral power at the ~500-yr band in the revised spectral analysis figure (Fig. 6);
5) clarified (in lines 343-349) that the raw data clearly show ~500-yr variability at the northeastern coastal sites;

6) removed most references to the standard deviation ratios because I agree that the averaging across records should reduce the signal amplitudes as shown by Hébert et al., 2021;
7) clarified that the spectra in the updated Fig. 6 (previous Fig. 5) were calculated using z-scores to make the temperature and moisture reconstructions directly comparable;
8) simplified Figures 6-7 into a single new Figure 7, which includes a color scale and focuses on the main mid-Holocene shift rather than correlation across the entire pair of timeseries;
9) addressed concerns that arbitrarily selecting any time period and dividing the data into positive and negative change groups (in Fig. 7) would create spurious anomaly patterns by a) showing the patterns of change in selected individual records and b) re-calculating the direction of change using the average difference between two 600-yr periods rather than a fixed change point; by doing so, I tried to minimize the risk of a spuriously constructed abrupt change, which would be smoothed over the 600-yr windows and consequently reduced in amplitude (if it were spurious);
10) further clarified that the time period examined in Fig. 7 was not selected arbitrarily but was found previously to represent the largest rate of change in multiple proxies in eastern North American data over the past 8000 years (line 140 in the Methods) and citing (in lines 375-380) a more in-depth analysis of the spatial patterns of this change in a separate dataset with a greater number of records: Shuman, B. N., Stefanescu, I. C., Grigg, L. D., Foster, D. R., & Oswald, W. W. (2023). A millennial-scale oscillation in latitudinal temperature gradients along the western North Atlantic during the mid-Holocene. *Geophysical Research Letters*, 50, e2022GL102556. https://doi.org/10.1029/2022GL102556;
11) removed references to minor or insignificant changes (e.g., 8200-yr event in line 358) in the datasets and focus on the major cen-mil patterns.

Overall, I have updated the analyses to be more consistent with the state-of-the-art analyses cited by Dr. Hébert, while retaining some of the explanatory discussion of random variations to help illustrate the need to use methods such as the null distributions of correlation coefficients to test for significance of (typically weak) cen-mil variations in paleoclimate datasets. I have also tried to correct all of the smaller issues that were noticed.

Thank you for the opportunity to submit a revision.

Sincerely,
Bryan Shuman