RC2: 'Comment on egusphere-2025-4807',

Anonymous Referee #2, 07 Jan 2026 reply

Review comments on “Sea-surface temperature variability and climate drivers in Cuba’s Jardines de la Reina National Park (2003–2022)” by Castillo-Alvarez et al.

This work examines the physical drivers of sea surface temperature (SST) trends and variability in an area encompassing the Gulf of Ana Maria, the Jardines de la Reina National Park, and the Caribbean Sea off the southern coast of Cuba. The authors find differences between the shallow gulf waters and the much deeper Caribbean Sea, which appears to be primarily a result of the mixed layer depth. They correlate modes of local SST variability with large-scale climate modes, finding (potentially) significant correlations with the North Atlantic Oscillation and the El Niño-Southern Oscillation. The authors note that understanding the mechanisms that cause SST trends and anomalies (and hence marine heatwaves) can be used to better predict the occurrence of marine heatwaves for coral ecosystems in this region.

The paper is well written and the analysis is presented in a thorough manner such that the authors’ conclusions are supported by the data (with a few relatively minor exceptions; see below). The analysis is well done, although perhaps not particularly novel and the conclusions reached may not be broadly applicable outside of the study region. Still, I would recommend the paper for publication if the comments below can be addressed in a satisfactory manner.

We thank the referee for the careful reading and constructive assessment of our manuscript. We appreciate the positive evaluation. We agree that the focus is mainly regional. We were motivated by the ecological value of the Jardines de la Reina National Park and the strong shelf - offshore contrasts created by the Gulf of Ana María and adjacent deep Caribbean waters.

Line 246-247: The results of this sensitivity test should be shown in supplementary material.

R: Regarding to the EOF analysis, those lines said: “It should be noted that a sensitivity test that removed a linear trend produced similar leading modes (not shown herein).”

We have added to the supplementary material the EOF analysis computed after removing the linear trend (new Figures S3) and updated the manuscript text to explicitly refer to this figure. The detrended EOF spatial patterns are very similar compared to the no detrended EOFs, indicating that our interpretation of EOF1–EOF2 is not sensitive to the presence of the long-term trend Accordingly, The new lines (275-276) in the revised manuscript (RM) now read: “For comparison, we also include EOF results computed after removing the linear trend (Figures S3).”

Lines 270-271: It would be nice to see a supplementary figure with the cross-correlation vs. lag to support this sentence.

R: We agree, and we have added a supplementary figure showing the cross-correlation between the regional-mean SST and the regional-mean air–sea heat flux as a function of lag (new Figure S5). For completeness, Figure S5 also includes the standardized time series over 2003–2022, highlighting the dominant seasonal co-variability. The cross-correlation exhibits a clear maximum at a lag of approximately +2 months (with heat flux leading SST; see lag convention in the caption), consistent with mixed-layer heat storage and the expected delayed SST response to surface forcing. We have updated Lines 270–271 accordingly to reference Figure S5.

Lines 298-299 and Fig. 6: In the text the significance test is written as a p-value (“p <0.05”) and in the figure it is as the equivalent percentage level (“95% level”). I recommend using the percentage level for both as it seems to be more common in our field.

R: Following the reviewer comment we modified the text, and we changed to percentage level.

Lines 308-313 and Fig. 7: Has the determination of the transition points of the piecewise/step fits been done empirically? If so, by what method? See Reeves, et al. (2007). A Review and Comparison of Changepoint Detection Techniques for Climate Data. Journal of Applied Meteorology and Climatology, 46(6), 900–915. https://doi.org/10.1175/JAM2493.1

R: Thanks for this comment. Following the reviewer’s suggestion, we apply a changepoint analysis using a two-phase regression with a common trend (XLW; Reeves et al., 2007). So, we include the following text in the RM:

“To formally assess whether the apparent transition toward a warmer state is statistically significant, we applied a changepoint test based on a two-phase regression with a common trend (XLW), which is designed to detect a step-like shift in the mean level while allowing for an underlying linear trend. The test was performed on the monthly SST anomaly series for the two representative subregions (GAM and CS; Fig. 7a,c), scanning all admissible changepoint times and using the maximum F statistic (Fmax; see Reeves et al. (2007)); significance was estimated via Monte Carlo simulation under the null with serial autocorrelation accounted for in the residual structure. The analysis identifies an optimal changepoint in December 2012 in both subregions, consistent with the step-like transition highlighted visually in Fig. 7b,d. The estimated mean-level increase after the changepoint is 0.67 °C for CS (Fmax = 53.45; 98% confidence) and 0.84 °C for GAM (Fmax = 35.75; 95% confidence). These results provide quantitative support that the late-2012 shift represents a statistically significant transition toward persistently warmer conditions in both shelf and offshore environments.”

Lines 353-366 and Fig. 10: The results of the significance testing of the correlation coefficients appear to be missing despite the mention of the method of significance testing. Additionally, are the effective degrees of freedom calculated before or after low-pass filtering. adjusted for the low-pass filtering? This is especially important for the PC1-NAO correlation because after low-pass filtering both time series have very few degrees of freedom left (perhaps around N=5 judging by the number of peaks and valleys).

R: In our analysis, the effective degrees of freedom (N*) were computed after applying the low-pass filter, and the significance of correlations was assessed using a two-sided Student’s t-test with N* adjusted for serial autocorrelation, following the lag-1 approximation (e.g., Bretherton et al., 1999; Thomson and Emery, 2024). We acknowledge that this was not clearly stated in the original manuscript and have now clarified it in the revised version (see section 2.5 in RM and our response to reviewer 1). We also added, for every reported correlation, the corresponding N* and significance level (Table S1). For the interannual correlations (2-yr low-pass filtered), N* is typically ~14. For the decadal correlation (5-yr low-pass filtered) between PC1 and the NAO index, N* = 6; despite the reduced degrees of freedom, the correlation remains significant at the 95% confidence level ($p < 0.05$; Table S1).

In the RM we have included the following lines: “The effective degrees of freedom (N*) and associated two-sided significance levels for all correlations (computed after low-pass filtering) are summarized in Table S1”

Fig. 10c: The text in the correlation box should be “PC1-NAO,” not “PC2-NAO.”

R: The legend of Figure 10c was fixed.

General comment: It is a matter of personal taste, but I think that a more standard first-person voice (e.g., “We diagnosed…”) may improve readability compared with third-person voice (e.g., “The authors diagnosed…”).

R: We thank the reviewer for this helpful stylistic suggestion. We have revised the manuscript to consistently use the third person plural.

Bibliography

Bretherton, C. S., Widmann, M., Dymnikov, V. P., Wallace, J. M., and Bladé, I.: The effective number of spatial degrees of freedom of a time-varying field, Journal of climate, 12, 1990-2009, 1999.

Reeves, J., Chen, J., Wang, X. L., Lund, R., and Lu, Q. Q.: A review and comparison of changepoint detection techniques for climate data, Journal of applied meteorology and climatology, 46, 900-915, 2007.

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