

Response to the Editor's comments

Dear Dr Mohamed,

The two reviewers are satisfied with the revised version of your manuscript, except for a small typo to fix. However I have noted a few points in the third review that we unfortunately misplaced. These are minor but I think that addressing them will improve the quality of the manuscript. Please consider the following points (the line numbers refer to the newest submission, the one with tracked changes):

A: Once again, we would like to thank you and all the anonymous reviewers for their willingness to improve the quality of our manuscript. We have corrected this typo to be 2013 instead of 2023. In the following, we address all comments/clarifications suggested by the third reviewer point by point. The responses are shown in bold text.

- Lines 135-138, reviewer 3 said : "The method applied is not the "shifting baseline" presented by Amaya et al. (2023). The shifting baseline involves a redefinition of the climatology over shifting 30-year periods. Please refer to the paper by Smith et al. 2025 (<https://www.sciencedirect.com/science/article/pii/S0079661124002106>) for a more comprehensive discussion of MHW definitions in the presence of a trend, and their implications."

A: Although most of the previous publications refer to this approach as "shifting baseline", including the proposed article by Smith et al. 2025 (please see the note at the end of the definitions of "detrended baseline" and "shifting baseline" in Table 1 in their article), in the revised version of our manuscript, we have used the term "detrended baseline" instead of "shifting baseline" to distinguish between the two cases, as suggested by Smith et al. 2025.

- Line 178, reviewer 3 notes: "Why is the approach applied twice? This sentence seems incomplete.". Indeed the sentence seems incomplete to me as well, maybe it is just a question of style.

A: That's right, the word twice could be confusing, so we rewrote the whole sentence as below, especially since we already mentioned in the same paragraph that we reapplied the test again for the second period (2001 to 2024) to investigate whether a new regime shift has recently occurred in the North Sea SST.

- The result of this test, based on the entire study period (1982–2024), reveals that a significant change point in SST occurred between 1996 and 2001 (Fig. 1A, top left panel).

- lines 320-324 (beginning of section 3.3), reviewer 3 noted: "We already know that the trend is likely nonlinear given the two detected climate shifts and the trend intensification after 2013. The rationale for and implications of considering a linear trend should be discussed." I got the same impression as the reviewer when I read the paragraph. Maybe it is something you would want to address?

A: This is one of the questions that still has a lot of debate among scientists, for example, the above-mentioned article by Smith et al. (2025) demonstrates that “Often, assumptions about the nature of warming (e.g., linear, or higher-order trends) are made, with the risk that higher-order trends may inadvertently remove important low-frequency natural variability along with the anthropogenic warming signal”. In our manuscript, the decision to include a linear trend analysis in this section was motivated by the need to provide a baseline quantification of long-term changes over the full study period. Linear trends are commonly used in most previous studies as a first-order approximation to facilitate comparison across regions and time frames. In the revised MS version (lines 377-383), we have added and highlighted the justification and limitations of using the linear approach.

- Another remark for reviewer 3: "Figure 6. The AMO clearly shows a positive trend encompassing the whole study period. How much does the AMO evolution contribute to the estimated linear trend? It seems that associating the linear trend to climate change may be incorrect as that trend may also include internal variability. This aspect should be carefully discussed." I find this remark relevant. Maybe a caveat should be added to the discussion in lines 369-374? Or elsewhere in the manuscript?

A: That's a good point. However, in this figure, we have not removed the trend before the EOF analysis of cumulative MHW intensity, as the aim here is to analyze the total variability (i.e., without removing the trend) and not to separate the role of trend and internal variability (this point has been addressed in the same section based on the TAR). Therefore, we used the non-detrended AMO index (i.e., including internal variability and trend). This figure suggests that the observed SST regime shift and associated MHW are attributed to both the long-term warming trend and the shift to the warm phase of the AMO mode. In the revised version, we emphasized in the caption of this figure that the EOF was applied based on the original SST.

I hope you will find these comments relevant; taking them into account should not take much of your time. The manuscript will not be sent out to the reviewers again.

A: Thank you again for your time. We really appreciate it. All comments are relevant, and we consider all of them as mentioned above.

We also consider the editorial board's comment on the use of the color blindness simulator scheme for Figures 4, 6, 7, 10, and 12.

Yours

sincerely,

Anne Marie Treguier