

Review of Establishing a monitoring approach for marine heatwaves in the Black and Mediterranean Sea by Lima et al.

The manuscript provides a useful update of MHW indicators for the Mediterranean and introduces new OHC-based diagnostics. The development of indicators for the Black Sea is timely and fills a gap in the study of MHW. The idea of combining SST and upper-ocean OHC into a potential monitoring index is promising and could be published after revision.

However, several methodological gaps and missing justifications weaken the robustness of the results.

Some aspects require clarification and particularly the seasonal dependence of events, the physical meaning of OHC-based MHWs, the calculation of climatologies, and the quantification of uncertainties.

I found some very important points :

- The use of multiple products (regional reanalyses, global reanalysis, and SST L4) even if some discussions are essential on their limitations.
- The computation of an upper-ocean OHC indicator (0–40 m) offers additional insight into the vertical structure of events, especially in the Black Sea. I would advocate for a more in-depth analysis of the propagation of surface MHW into subsurface layer.
- Regional descriptions (Sicily Channel, CIL, Rim Current, etc.) and case studies (2011–2012, 2017, 2024) are relevant and illustrate contrasting conditions.

Here are some thoughts to improve this study :

- **Lack of seasonal decomposition** : it is unclear whether MHW detection is performed separately for summer and winter. In my opinion, this distinction is essential because impacts differ strongly from one season to another. Impacts of MHW are the main justification of establishing a monitoring approach. In addition, upper-ocean stratification varies strongly. I suggest to provide season-specific analyses for all metrics.
- **Unclear physical link between SST and OHC** : The manuscript applies HD2016 method (designed for surface anomalies) to the integrated 0–40 m OHC variable. OHC has different variability, autocorrelation and distributions compared to SST. Furthermore, in strongly stratified summer conditions, the 0–40m layer compiles two different layers. The resulting OHC-MHW mix surface and subsurface processes without clear justification. I recommend to justify why this method is appropriate and can be applied to this layer even in summer.

Maybe normalize OHC anomalies before thresholding. Moreover, SST-OHC relationship is region-specific (e.g., ventilated vs strongly stratified areas) and dependent on the season/stratification/internal variability. Maybe provide a seasonal SST-OHC correlation maps and discuss mechanisms controlling both variables.

- **Insufficiently motivated choice of 0–40 m for OHC:** The choice of 0–40m is justified only briefly but in stratified regions this layer may represent different processes. Moreover, this layer aggregate a lot of different ecosystems and even more in complex basins such as Med and Black Sea. I recommend to test alternative integration depths (0–20, 20–40, 0–60 or 0–100 m) and compare the resulting MHW statistics.
- **Insufficient methodological description :** I found the method to lack from essential details. How is the climatology computed (running mean, harmonic filtering). How do you compute the MLD ? Can you explain why do you start your reference period in 1993? I found the linear detrending to be quite limiting knowing the warming trend in the Med Sea and the regional dependencies (influence of the AMV, aerosol-forced variability, nonlinear internal variability).
- **Limited uncertainty quantification :** Multiple products are compared but there is no formal propagation of uncertainties, no sensitivity testes, no confidence intervals associated with trends or metrics. Some suggestions could be to include bootstrap ranges, product statistics to inform on the spread and sensitivity analyses to percentile choices, detrending methods.
- **Introduction :** The introduction sometimes reads as a list of ideas lacking cohesion, and several important references are missing. Moreover, what is the impact of ENSO on the Med Sea? There is also a strong influence of the NAO and AMV on the western basin.
- **Discussion :** I find the discussion too concise and not really discussing the limitations of the study. It may include explicit methodological limitations (detrending, assimilation influence, vertical resolution, depth choice). As an example, using reanalysys and L4 tend to smooth signal along the coast or in period of important cloud cover due to missing data.

Minor comments :

- in the manuscript, I suggest to include + or – before the anomaly number
- several paragraphs (e.g., the beginning of Section 3.3.1) contain methodological or introductory material and should be moved to their appropriate sections
- L38 : England et al. (2025) is not about the Med/Black nor the disruptions of marine ecosystems.
- L192 : Maybe I'm wrong but it is expected that regional reanalyses perform better partly because they assimilate local data and often use SST relaxation. This may artificially improve skill and reduce variability especially in coastal regions.
- L349-352 : The suggested “combined approach” (SST + OHC intensities) is based purely on a statistical aspect and does not integrate ecological vulnerability or impacts. For a monitoring strategy, impact-based thresholds are essential.
- L453, the main drivers in the Med are now well-known.
- Figures are not self-reading and could be improved.

References to consider :

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