

We would like to thank the two reviewers for their constructive feedback and comments, which have greatly helped to improve the clarity, focus, and overall quality of our manuscript. In response to their comments, we have expanded the discussion to include a synthesis of the drivers associated with 2024 MHWs in the Mediterranean, as well as a discussion of the different methods used. We believe that these revisions have strengthened the manuscript and enhanced its contribution.

Below, we address each of the reviewer's comments, along with the corresponding changes made in the manuscript. Reviewer comments are shown in black with the author's [response in blue](#). The lines refer to the clean manuscript to which the revisions have been applied.

Revisor 1:

This short paper provides an assessment of SST evolution throughout the Mediterranean Sea in terms of 1982-2024 trends, and in terms of 2002-2024 Marine heatwave (MHW) statistics (duration, days per year, intensity) computed with 3 different methods. A special focus is put on year 2024, which is shown to have been exceptional since 2002 in terms of number of MHW days, as well as in terms of mean event durations and intensities in the Eastern basin. The other main conclusion concerns the sensitivity of the results to MHW detection methods.

The paper is well written and clearly reports about most diagnostics that were produced, although it provides barely any hypothesis (or discussion) about the Mediterranean dynamics or possible MHW drivers in 2024 (or before). I would thus suggest to discuss the potential reasons (atmospheric state, ocean preconditioning, etc) that may explain the 2024 MHW events (duration, intensity, location), on which the paper is focused.

We have expanded the Discussion section to include a synthesis of the atmospheric and oceanic conditions associated with the 2024 MHWs, including the role of atmospheric heatwaves, persistent blocking patterns, reduced wind speeds, and anomalous surface heat fluxes [L. 178–199].

Additional references have been included where appropriate (for instance: Androulidakis et al., 2024 [L. 178]; Napolitano et al., 2025 [L. 186, 189]; Tolika et al., 2025 [L. 190]; Aboelkhair et al., 2023 [L. 191]; Paredes-Fortuny et al., 2025 [L. 191, 198] and Bonino et al., 2025 [L. 195]).

Some readers might also regret that this study is not innovative in terms of methodology: the exact same 3 detection methods (fixed baseline, moving base line, detrending) were compared on a smaller region and on a slightly shorter dataset in the Ocean Science 2025 preprint by the same authors, and the present study basically provides the same methodological conclusion. The main added value of the present study over the latter is thus its extension to the whole Med Sea and the inclusion of year 2024.

While we agree with the reviewer that this manuscript does not introduce a new methodology, we consider that this was not the objective of this study. Rather, the aim was to characterise 2024 MHWs with these three methods in the context of the last two decades in the whole Mediterranean, assessing differences in the Western and Eastern regions. We acknowledge that this focus was not very clear from the original manuscript, and we have improved the objective in the new version:

"In this study, we aim to analyse Mediterranean Sea surface temperature (SST) trends from 1982 to 2024 and marine heatwaves (MHWs) in 2002–2024, using three detection approaches: a fixed baseline (1982–2011), a 20-year moving baseline, and detrended SST data." [L.48–50]

Regarding methodology again, I am also a bit surprised that the authors do not prioritize their 3 detection methods, nor comment about their own purposes, their strengths and weaknesses. Instead, the expressions "the increase is real" (line 1120), "ensemble perspective" and "robust metrics" (line 177) suggest that the 3 methods are implicitly considered as equally valid or adequate for the study. However, they are quite different by construction in the way they deal with long-term SST evolution (in particular the fixed baseline approach compared with the other two). A good part of the differences between the results of these methods is expected a priori (based on the existing literature about them), but this is not commented. In order to improve the paper, better anchor it in the existing methodological MHW literature, and enrich it with respect to the Ocean Science preprint, I would suggest to document, explain and discuss the known/expected properties and strengths of each approach. It would be also be very valuable to provide a posteriori recommendation about the 3 methods for the present purpose, and discard the least (or designate the most) adequate, if any.

We appreciate this comment. We have added a new section in the Discussion that explicitly discusses the physical interpretation, strengths, and limitations of each MHW detection approach in the context of the Mediterranean Sea [L. 205–242].

Here are a few other remarks :

- L 62: I would suggest to write ".. remove the mean seasonal cycle" and to explain how this is done.

The text has been revised to state that the mean seasonal cycle is removed, and we now explicitly explain that this is done by estimating and subtracting the annual harmonic (yearly frequency) from the time series:

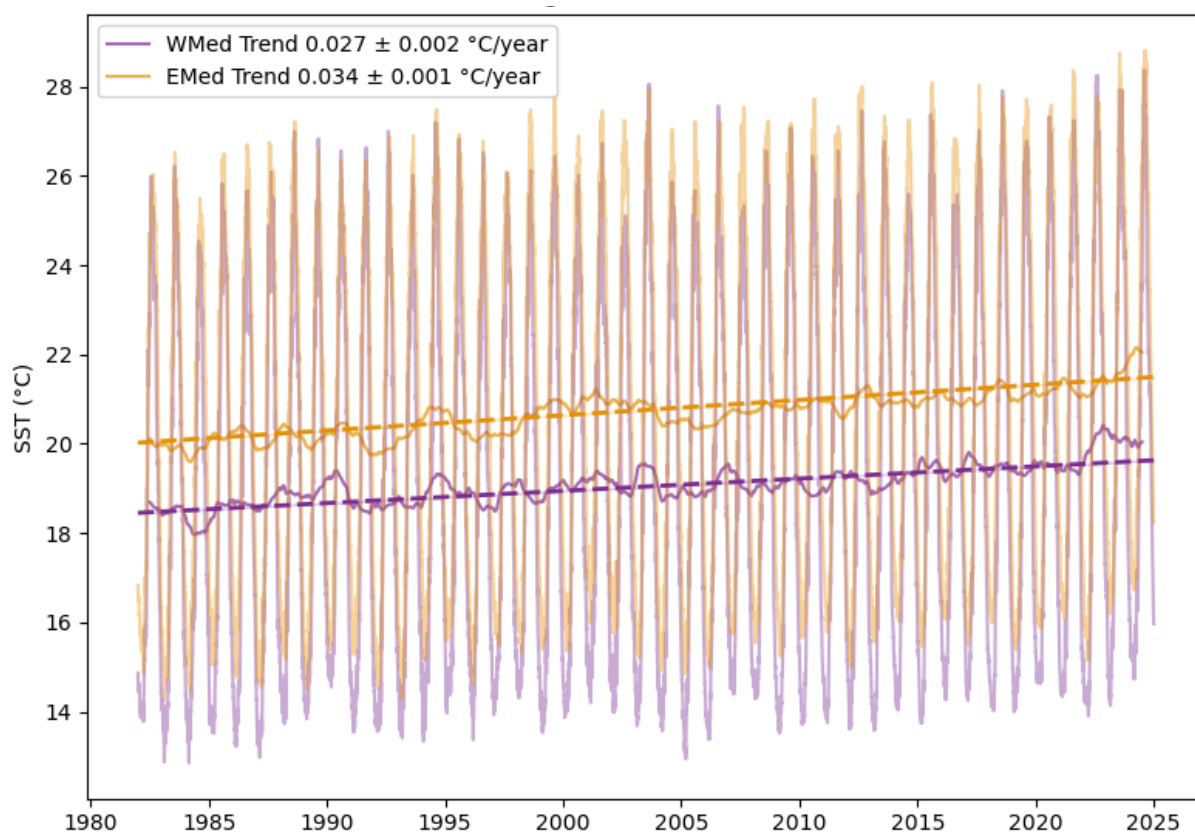
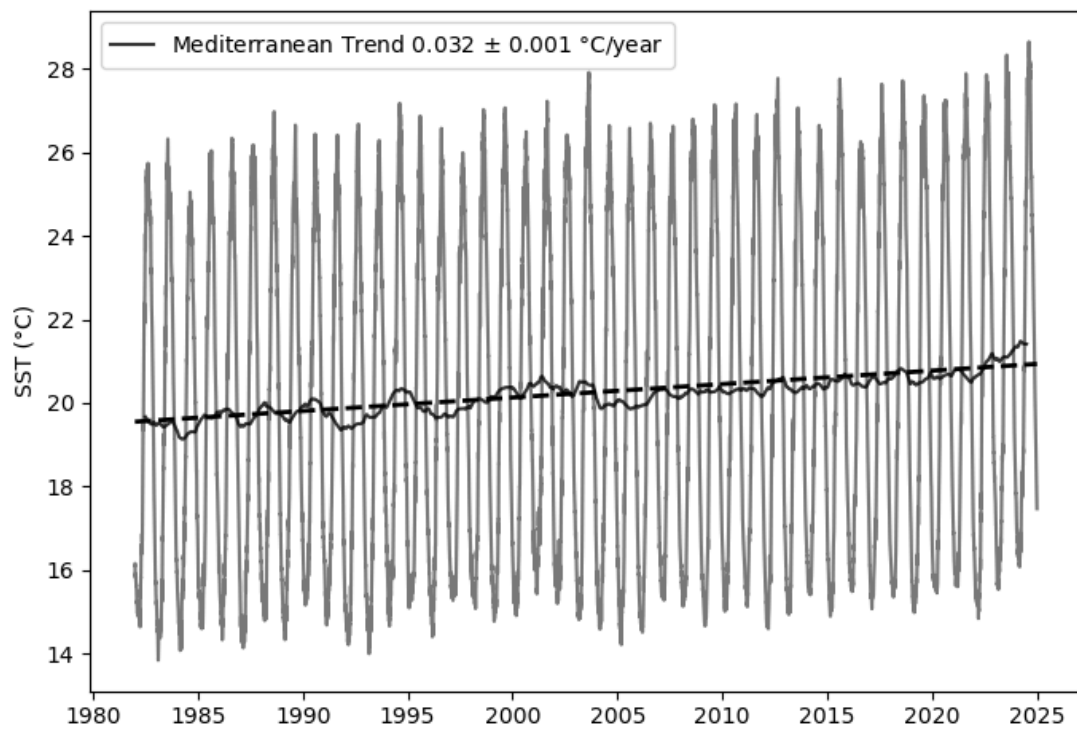
"For the SST trend estimation, we first remove the mean seasonal cycle, estimated by fitting a harmonic function at each grid point and then compute the long-term SST trend using the Theil–Sen slope estimator (Fernández-Álvarez et al., 2025)." [L. 71–72]

- Fig 1: is the 365-day moving average (L 107) applied on Fig1d as well? Please clarify. Would it be possible to see the original (unfiltered) SST timeseries as well in the background of panels c and d?

We have clarified in the caption of Figure 1 that the 365-day moving average is applied to all time series:

"The time series (c, d) have been smoothed using a 365-day moving average for clearer visual representation." [L. 122–123]

We considered including the original SST time series in the background of Figures 1c and 1d. However, the amplitude of the seasonal cycle is large (mean amplitude of $\sim 11^{\circ}\text{C}$), which makes it difficult to clearly visualise the longer-term signal (see figures below). For this reason, we show only the 365-day moving average time series in Fig. 1. Here are the figures with the original and filtered temperatures for the whole basin and by subregion:



- L 149: I suggest to split this long sentence after "(respectively)".

Done.

- L 150: It indeed seems to me that "... the moving baseline its total was surpassed by 2022", but not by 2023.

We thank the reviewer for pointing this out. This was an error in the original manuscript. The 2024 MHW days in the Western Mediterranean using a moving baseline were higher than in 2023, as can be seen in the table below.

MHW days			
Year	Fixed-baseline	Moving-baseline	Detrended
2003	74	84	68
2022	154	115	83
2023	144	93	55
2024	155	96	37

The text has now been corrected accordingly:

“... 2024 was the year with the most MHW days since 2002; however, using the moving baseline, its total was surpassed only by 2022, and with detrended data by 2003, 2022, and 2023 (Fig 3a).” [L. 166–167]

- L 161: Are panels g,h,i shown for 2024? Please clarify.

This has been clarified in the caption:

“(g-i) Maps of the annual total number of days with MHWs (MHW days) in 2024 detected using a fixed baseline (g), moving baseline (h) and detrended data (i).” [L. 203–204]

- L 184-187. Is this paragraph the end of the conclusion? It repeats statements that are given before...

This paragraph was included by mistake and has now been removed.

- Please consider providing the link toward the actual dataset that is used, and to the tools that were used for this study.

We now include the link to the dataset in the Data Availability section. Similarly, the code used in this study is linked in Code Availability section.

Revisor 2:

General Comments

The paper presents an assessment of SST and SST-based MHW trends (1982–2024) in the Mediterranean Sea, with particular focus on the significant 2024 MHWs. The authors apply three MHW detection approaches considering fixed, moving, and detrended baselines. The study examines how the three detection approaches affect MHW descriptions (in terms of total MHW days, duration and mean intensity of MHWs) and highlights the exceptional character of 2024 MHWs.

However, the manuscript would benefit from a deeper discussion of the physical meaning and interpretative implications of the three methods. In particular, I encourage the authors to expand the discussion beyond the comparison between fixed and alternative baselines and explicitly address the conceptual difference between the moving and detrended approaches. Although both reduce the imprint of long-term warming, they describe different physical frameworks. Moving baselines yield extremes relative to the contemporary climate state, while detrending isolates variability independently of the long-term trend. Clarifying this distinction is essential for discussing the implications and limitations of each method for interpreting MHW trends and associated impacts, and thus would considerably strengthen the paper.

We agree with the reviewer and have added a section that discusses the implications as well as the strengths and disadvantages of each approach in the context of the Mediterranean [L. 205–242].

Another concern is the lack of discussion of the 2024 MHWs which are meant to be an important part of the paper. Apart from comparisons with previous years in terms of the three basin-averaged metrics, the study lacks a comprehensive description of MHW conditions during this exceptional year (which areas were most affected, when did the most severe MHW conditions occur?) and a basic discussion of potential drivers. There is definitely enough space for such an addition, as the paper now includes figures (Fig. 3g-i) that are not even mentioned in the text. The inclusion of results should be balanced according to their relative importance

We agree that the original manuscript did not sufficiently discuss the spatial characteristics and drivers of the exceptional 2024 MHWs, despite their central role in the study. In response, we have expanded the Discussion section to include a dedicated subsection describing the 2024 MHWs drivers, including the ocean and atmospheric conditions and the role of heat fluxes in the onset and development of the 2024 MHWs [L. 178–199].

The study should also be enriched by a broader integration of the existing literature in several parts of the paper. With an enriched discussion of methods interpretation and associated limitations, clearer rationale for certain choices, and some textual clarifications (all listed in specific comments below), the manuscript will provide a much improved contribution to the MHW literature.

We have reviewed recent publications to enrich the discussion throughout the manuscript. In particular, we expanded the discussion to the drivers of the 2024 marine heatwaves and strengthened the interpretation of the implications and limitations associated with each baseline approach. To support and contextualise these additions, we incorporated the following recent references:

Aboelkhair, H., Mohamed, B., Morsy, M., and Nagy, H.: Co-Occurrence of Atmospheric and Oceanic Heatwaves in the Eastern Mediterranean over the Last Four Decades, *Remote Sensing*, 15, 1841, <https://doi.org/10.3390/rs15071841>, 2023.

Androulidakis, Y., Kolovoyiannis, V., Makris, C., and Krestenitis, Y.: Evidence of 2024 Summer as the Warmest During the Last Four Decades in the Aegean, Ionian, and Cretan Seas, *Journal of Marine Science and Engineering*, 12, 2020, <https://doi.org/10.3390/jmse12112020>, 2024.

Bonino, G., McAdam, R., Athanasiadis, P., Cavicchia, L., Rodrigues, R. R., Scoccimarro, E., Tibaldi, S., and Masina, S.: Mediterranean summer marine heatwaves triggered by weaker winds under subtropical ridges, *Nat. Geosci.*, 18, 848–853, <https://doi.org/10.1038/s41561-025-01762-9>, 2025.

Denaxa, D., Korres, G., Darmaraki, S., and Hatzaki, M.: Insights into sea surface temperature variability and the impact of long-term warming on marine heatwaves in the Mediterranean Sea, *State of the Planet*, 6-osr9, 1–11, <https://doi.org/10.5194/sp-6-osr9-10-2025>, 2025.

Hayward, A., Dasgupta, N., McAdam, R., Payne, M. R., Raj, R. P., Bonino, G., Chatterjee, S., Combes, V., Denaxa, D., De Rovere, F., Englyst, P., Haapaniemi, V., Hargous, P., Høyer, J., Joseph, K. A., Lopes, B., Oliveira, A., Paixão, J., Silva, F., Surendran, S., Zegna-Rata, A., and Olsen, S.: Marine Heat waves - Multiple Analysis / Definitions (MHW-MAD): A Multi-Definition Global Marine Heatwave Dataset from Satellite Sea Surface Temperature data, *Earth System Science Data Discussions*, 1–21, <https://doi.org/10.5194/essd-2025-590>, 2025.

Marcos, M., Amores, A., Agulles, M., Robson, J., and Feng, X.: Global warming drives a threefold increase in persistence and 1 °C rise in intensity of marine heatwaves, *Proceedings of the National Academy of Sciences*, 122, e2413505122, <https://doi.org/10.1073/pnas.2413505122>, 2025.

Napolitano, E., Carillo, A., Iacono, R., Struglia, M. V., dell'Aquila, A., Palma, M., Marullo, S., De Sabata, E., Bordone, A., and Borzelli, G. E.: Unprecedented Mediterranean Sea warming in 2024: analysis of the driving mechanisms, *Front. Mar. Sci.*, 12, <https://doi.org/10.3389/fmars.2025.1668204>, 2025.

Paredes-Fortuny, L., Pastor, F., and Khodayar, S.: Concurrent atmospheric heatwaves intensify marine heatwaves through air-sea heat flux change in the Mediterranean Sea, *Commun Earth Environ*, 6, 638, <https://doi.org/10.1038/s43247-025-02633-2>, 2025.

Tolika, K., Manios, E. M., Zachos, A. P., Velikou, K., and Anagnostopoulou, C.: Can we predict the extreme? Assessing the skill of seasonal forecasts in capturing the 2024 European heatwave, *Environ. Res. Lett.*, 20, 124050, <https://doi.org/10.1088/1748-9326/ae21f7>, 2025.

I would like to kindly encourage the authors to submit a revised version of their work taking this review as constructive feedback aimed at improving their already interesting work.

Specific comments:

Abstract:

Line 1-2: Here you say that the study includes analysis of SST trends for 1982-2024 and MHWs in 2024, which suggests that only 2024 events are analysed; please, rephrase

Done. Now it reads:

"...marine heatwaves (MHWs) for the period 2002 to 2024, with a special focus on the last year, using three MHW detection approaches..." [L. 7]

Line 10: Why "In contrast"? No contrast reported for the WMed MHW days in this sentence.

We removed this connector.

Overall, please ensure the abstract reflects the revisions suggested in this review.

We modified the abstract according to the suggestions.

Sect. 1 - Introduction

Line 19: “sea surface” should be removed from the definition, MHWs are not necessarily surface events

Done.

Line 20: Marine ecosystem disruption is not only by altering species distributions and triggering mass mortality events. I suggest including additional examples.

We modified this sentence to include more examples. Now it reads:

“...disrupting marine ecosystems by altering species distributions, modifying individuals' behaviour and physiology and, in extreme cases, triggering mass mortality events (Garrabou et al., 2022; Smith et al., 2023; Wernberg et al., 2025).” [L. 21–23]

Line 24: “on the event’s spatial and temporal scale and distribution” what do you mean by distribution here? Please, clarify.

By “distribution” we meant the geographical location of the event, acknowledging that the dominant processes and dynamics can differ substantially between regions (e.g. the Mediterranean Sea versus the northeastern Pacific). To avoid ambiguity, we have reworded this sentence in the manuscript:

“A range of physical processes contribute to the development and persistence of MHWs, and their relative importance often depends on the event’s spatial and temporal scale and its location (Holbrook et al., 2019).” [L. 25]

Line 28: Consider rearranging and enriching (recent literature) the info now included in this paragraph, separating Mediterranean-specific information from general MHW background. Also, “complete review” on drivers?

We added recent literature such as Bonino et al. (2025) and Paredes-Fortuny et al. (2025).

The introduction is structured with a paragraph dedicated to general MHWs, another to the Med and another to the definitions of MHWs and the aim.

Line 33: This paragraph should better be enriched and split into smaller ones. A short introduction on previously applied alternative detection methods should be included, ending with the gaps this study aims to fill.

We agree that alternative MHW detection methods and their limitations are an important topic. However, we believe that the current Introduction already provides sufficient context by summarising the most widely used approach (fixed baseline), briefly introducing alternative baseline definitions, and motivating why these have been proposed in the literature [L. 35–43]. Since the primary objective of this study is not to introduce or develop new detection methodologies, but rather to analyse the 2024 MHWs in the Mediterranean using established approaches, we consider that further expanding this methodological discussion in the Introduction would detract from the main focus of the paper. Instead, we have prioritised enriching and expanding the Discussion section [L.205–242], where we now more thoroughly address the physical interpretation, strengths, and limitations of the different detection methods in the specific context of the Mediterranean Sea. We believe

this structure better balances background information with analysis and interpretation, and more clearly highlights the contribution of the present study.

Line 41: adapt to what? Explain better the purpose of employing alternatives, and then describe these alternatives. Also, refer to studies that apply such methods explaining the corresponding objectives.

We now explicitly state that these approaches have been proposed to better separate extreme events from the long-term warming signal [L. 45–46].

Line 47: Why the 2 past decades? The rationale should be explained in Methods.

We have added an explanation in the methods regarding the use of the last two decades:

“The MHWs are calculated from 2002 to 2024, which is the common period for all the baselines. Due to the nature of the moving-baseline approach, the first year for which MHWs can be consistently computed is 2002, once the initial 20-year reference window is available.” [L. 67–70]

Sect. 2 – Dataset and Methodology

Line 53: Please specify the detection algorithm used (if not developed by the authors). Providing this reference allows you to omit some standard processing steps (e.g. threshold smoothing).

Now we explicitly state that we only change the baseline to construct the climatology, and the rest of the detection algorithm is as proposed in Hobday et al. (2016):

“...SST time series, and then compute and apply a fixed-baseline (1982–2011) threshold on the detrended data. In all cases, the remaining steps of the detection algorithm follow Hobday et al. (2016).” [L. 67]

Line 55: I would suggest: “..using three different methods, as in...”

Done.

Line 62: Change paragraph

Done.

Line 64: What do you mean by “real” here? Maybe choose another wording

We have rephrased the sentence, and now it reads as:

“Previous studies have shown that a linear fit is a good approximation of the trend in the Mediterranean Sea (Pisano et al., 2020; Simon et al., 2023).” [L. 72–74]

Line 67: The term cumulative intensity in MHW studies is usually considered to be the product of intensity and event duration. Mean cumulative intensity per year or the sum of the cumulative intensities over a year may then be examined, depending on the objective. Please, clarify what you compute here.

I do not see results for this metric in the study (same for maximum intensity). Is there a reason to introduce these metrics in Methods?

This was left here from an earlier version of the manuscript. Since this metric is no longer discussed, we removed this part. Attending to your question, we were doing the mean cumulative intensity (defined as the product).

Sect. 3 – Results and Discussion

Line 74: Acronyms EMed/WMed should be used consistently; note that the EMed–WMed boundary on the map includes the Adriatic in the EMed, but here you separate it. Please ensure consistency.

Done. As we describe it, the Adriatic Sea is part of the EMed, and this is included across the manuscript.

Line 79: I suggest enriching this part with more recent studies based either on the same or other SST datasets reaching closer to present.

We added more recent references. For instance: Denaxa et al., 2025 [L. 89] and Ibrahim et al., 2021 [L. 113].

Line 85: The increase is gradual throughout the entire study period; please, rephrase.

We rephrased the sentence to indicate that the years 2022–2024 have temperatures above the trend line:

“Regarding the time series of the averaged SST data for the whole basin, the smoothed temperatures from 2022 onwards fall above the trend line (Fig. 1c).” [L. 95–96]

Line 92: Have these studies considered exactly the same EMed/WMed separation? If not, rephrase accordingly.

Juza et al. (2022) apply a similar division to ours, although they separate the basin using a meridional boundary rather than through the Sicily Strait. Pisano et al. (2021) use the same division line as we do, but treat the Adriatic Sea as a separate region. Despite these differences, we think that what we have stated and our point still apply. We have rephrased the text to clarify that it is not an exact match. For instance:

“...by Pisano et al. (2020), noting that these studies apply comparable but not identical definitions of the Western and Eastern Mediterranean sub-basins.” [L. 101–102]

and

“...found that the EMed (excluding the Adriatic) had a warming trend of 0.033 ± 0.004 °C/year from 1982 to 2020, similar to the value reported...” [L. 114]

Line 112: Why this reference period? So far, 1982–2011 is used for the climatology computation and the entire 1982–2024 period is used for computing 2024 anomalies. Why is this most recent period used here for averaging MHW days? This should be explained for clarity. If possible, keeping consistency within the study would be ideal to avoid confusions and allow for inter-comparing your different findings that are relevant to deviations from the same state.

The period 2002–2024 is used for averaging MHW metrics because it represents the longest common interval over which MHW statistics can be directly compared across all three detection methods. We have clarified this point in the manuscript to avoid confusion [L. 67–69].

Line 116: Maybe remove this, this sentence compares findings between the last decade and the rest of the dataset, inter-annual is not much accurate here

We changed the wording of the sentence.

Line 117: MHWs

Done.

Line 117: I do not understand the selection of this specific year, it does not appear as a changing point.

The year 2011 is pointed as it corresponds to the final year of the fixed reference period (1982–2011) used to construct the climatology for the fixed-baseline MHW detection method, rather than indicating a physical regime shift or change point in the SST record [L. 60]. The selection of this year is according to the baseline proposed in Rosselló et al. (2023).

Line 120: In the fixed baseline method we detect events relevant to a past, cooler period in order to account for the long-term warming. Therefore, the observed increase in MHW days is not an artifact, or unreal. It is associated with the physical meaning of choosing to detect events that account for the long-term warming.

Yes, we agree. We have rephrased accordingly. This part now reads as follows:

“This tendency can also be observed to a lesser extent with the moving baseline and is reduced but still present with the detrended method, suggesting this increase might be due to a mechanism other than just the increase in mean warming (Fig. 2a).” [L. 133–134]

Line 128: It would be interesting to discuss this differentiation of mean intensity. Do these results potentially suggest a weaker role of the long-term warming in shaping the magnitude of this metric? In this respect, check also Marin et al. 2021 (<https://doi.org/10.1029/2020JC016708>) and Denaxa et al. 2025 (<https://doi.org/10.5194/sp-6-osr9-10-2025>) which report a more complex behaviour (from different aspects) of the mean MHW intensity compared to other MHW metrics.

We mentioned this aspect in the discussion:

“In particular, Denaxa et al. (2025) show that [...] trends in MHW duration and MHW days are mainly driven by the long-term trend. MHW mean intensity [...] is generally dominated by interannual variability....” [L. 158–161]

“...modifications in seasonal amplitude [...] have implications for attributing detected MHW trends either to long-term mean warming or to interannual variability; in particular, the authors showed that the spatial distribution of trends in seasonal amplitude is closely related to patterns in mean MHW intensity.” [L. 232–235]

Line 134: Better keep MHW days consistently throughout the text

Done.

Line 137: See previous comment

Both changed.

Line 138-9: Here you compare results for the moving baseline against what? It is not clear, please rephrase.

Response in the next comment.

Line 138: Not sure what “still” means here.

We have rephrased the part that includes this comment and the previous one. Now it reads as:

“Under the moving baseline, similarly to the fixed-baseline, EMed has the most MHW days (47 versus 43 days), while WMed exhibits the longer mean duration out of the two subbasins (13.6 versus 13.5 days) (Fig. 3a-b, d-e).” [L. 152–153]

Line 140: This is also true for the WMed, so better rephrase to support your point in the rest of the paragraph

We have rephrased this line:

“In both the WMed and EMed, most years with above-average MHW days occur in the latter half of the record; however, this behaviour is more pronounced in the EMed.” [L. 154–155]

Line 140: the EMed

Done.

Line 148: This sentence needs to be rearranged/split.

Done.

Line 154: MHW days, see previous comments

Done.

Line 156: As noted in previous comments, I suggest expanding this discussion and compare (further supporting) your findings with recent studies (several satellite- and model-based findings report stronger increases in MHW durations in the eastern basin).

Thank you, we expanded the discussion [178–242] and added new references:

Aboelkhair, H., Mohamed, B., Morsy, M., and Nagy, H.: Co-Occurrence of Atmospheric and Oceanic Heatwaves in the Eastern Mediterranean over the Last Four Decades, *Remote Sensing*, 15, 1841, <https://doi.org/10.3390/rs15071841>, 2023.

Androulidakis, Y., Kolovoyiannis, V., Makris, C., and Krestenitis, Y.: Evidence of 2024 Summer as the Warmest During the Last Four Decades in the Aegean, Ionian, and Cretan Seas, *Journal of Marine Science and Engineering*, 12, 2020, <https://doi.org/10.3390/jmse12112020>, 2024.

Bonino, G., McAdam, R., Athanasiadis, P., Cavicchia, L., Rodrigues, R. R., Scoccimarro, E., Tibaldi, S., and Masina, S.: Mediterranean summer marine heatwaves triggered by weaker winds under subtropical ridges, *Nat. Geosci.*, 18, 848–853, <https://doi.org/10.1038/s41561-025-01762-9>, 2025.

Denaxa, D., Korres, G., Darmaraki, S., and Hatzaki, M.: Insights into sea surface temperature variability and the impact of long-term warming on marine heatwaves in the Mediterranean Sea, *State of the Planet*, 6-osr9, 1–11, <https://doi.org/10.5194/sp-6-osr9-10-2025>, 2025.

Napolitano, E., Carillo, A., Iacono, R., Struglia, M. V., dell’Aquila, A., Palma, M., Marullo, S., De Sabata, E., Bordone, A., and Borzelli, G. E.: Unprecedented Mediterranean Sea warming in 2024: analysis of the driving mechanisms, *Front. Mar. Sci.*, 12, <https://doi.org/10.3389/fmars.2025.1668204>, 2025.

Paredes-Fortuny, L., Pastor, F., and Khodayar, S.: Concurrent atmospheric heatwaves intensify marine heatwaves through air-sea heat flux change in the Mediterranean Sea, *Commun Earth Environ*, 6, 638, <https://doi.org/10.1038/s43247-025-02633-2>, 2025.

Tolika, K., Manios, E. M., Zachos, A. P., Velikou, K., and Anagnostopoulou, C.: Can we predict the extreme? Assessing the skill of seasonal forecasts in capturing the 2024 European heatwave, *Environ. Res. Lett.*, 20, 124050, <https://doi.org/10.1088/1748-9326/ae21f7>, 2025.

Line 159: Figs 3g-i are not presented at all in the text, therefore they should either be removed or you should adjust the results section accordingly. Spatial differences for MHW days across the basin are now only discussed based on the EMed-WMed time-series. Also, if you decide to show spatial distributions of metrics, why limit this discussion to the MHW days? In any case, the selection of the figures to be shown and corresponding discussions should be based on the importance of your findings.

The inclusion of Figs. 3g–i was intended to illustrate the spatial extent and heterogeneity of the areas affected by MHWs, particularly during the exceptional conditions observed in 2024. We agree that these figures were not sufficiently introduced or discussed in the original version of the manuscript. In response, we have revised the Results section to explicitly refer to and describe the spatial patterns shown in Figs. 3g–i:

“The spatial distribution of areas with higher and lower numbers of MHW days obtained with the moving-baseline (Fig. 3h) method is consistent with that derived using the fixed baseline (Fig. 3g), although the overall magnitude is reduced by approximately 40 days. In contrast, when using the detrended approach, MHWs are largely restricted to the EMed, particularly its southern part (Fig. 3i).” [L. 174–177]

Sect. 4 Conclusions

Line 171: They do not account for the long-term warming, please correct and rephrase this conclusion

We have modified the wording:

“...methods that reduce the influence of the long-term warming on MHW detection.” [L. 251]

Line 176: The paper lacks discussion on the three methods. The last paragraph suggests different approaches as next steps (ensemble approach, absolute temperature thresholds) but the section skips discussing the methods being the focus of this study. I suggest discussing differences in the physical meaning especially

between the moving vs detrending methods (this should also be mentioned in the introduction of these methods as noted before) and consequent suggestions for applying these methods.

An important advancement would also be including critical reflection on limitations associated with the physical interpretation of results from the different methods. For instance, whether, and to what extent, we can assume that detrending corresponds to removing the climate-change warming signal, and what assumptions are implied for the remaining variability such as what should be considered internal/physical or interannual variability. Moreover, the implications/limitations of applying linearly detrending should be discussed in the context of warming basins that also exhibit strong multi-decadal variability, such as the Med Sea.

We have addressed this comment by substantially expanding the Discussion section. We added a dedicated discussion comparing the three detection methods and critically examining their physical interpretation and associated limitations. In particular, we now discuss the ecological motivation and sensitivity to baseline length of the moving-baseline approach, as well as the assumptions underlying the detrending method, including the extent to which linear detrending can be interpreted as removing the climate-change warming signal. Together with the limitations of the fixed-baseline approach already stated in the Introduction, these additions provide a clearer framework for interpreting the results obtained with each method.

Line 184: This paragraph is likely accidentally located here. It also largely overlaps with the beginning of this section and should be removed.

This paragraph was included by mistake and has now been removed.