

## **Review: Attribution of the 2025 Mediterranean Marine Heatwave to Climate Change Using Analogues:**

The authors present an analysis of the 2025 Mediterranean marine heatwave (MHW). Using an analogue-based approach, they examine SST anomalies and synoptic conditions in a past (1950-2024) and a present (1987-2024) period, in order to assess the atmospheric conditions conducive to the development of similar events in these two periods, in comparison to the conditions associated with the 2025 event. While the results are broadly consistent with the growing body of literature documenting the intensification of Mediterranean MHWs and their atmospheric drivers, the current manuscript does not yet provide sufficiently robust evidence to support its main attribution-related claims.

A key concern is the methodological interpretation. Although the analogue method is a useful tool for identifying historical precursors and exploring variability of atmospheric drivers, it cannot on its own provide formal attribution of events to anthropogenic climate change. At several points in the manuscript, the results derived from the analogue framework appear to be treated as a direct proxy for the influence of anthropogenic climate change on the occurrence of the event. In addition, many of the results rely primarily on qualitative comparisons and visual inspection rather than on a clearly defined quantitative framework, which leaves parts of the analysis more descriptive than substantiated. Also, the study focuses on the atmospheric drivers of the 2025 event, with the role of oceanic processes being largely neglected in the analysis and only briefly acknowledged in the limitations of the study.

To strengthen the manuscript, I suggest reconsidering the framing of the conclusions. In particular, the authors may wish to avoid drawing definitive inferences regarding the causality or likelihood of the event under climate change. Such questions are typically addressed through formal attribution studies using climate model ensembles, and the influence of anthropogenic climate change on the increasing likelihood and intensity of Mediterranean MHWs is already well established in the literature. As a result, attempting to infer this within the current framework, without a formal attribution approach, does not substantially advance existing understanding of the topic. Instead, the study could focus more on the specific insights that the analogue approach may provide in comparison to existing methods. Clarifying whether and in what ways, this approach may offer additional physical understanding or diagnostic value specifically for the 2025 event, beyond established analyses would, perhaps, help better define the contribution of the study. At present, however, this added value (if any) is not yet clear.

Overall, a major revision that addresses the detailed comments below and refocuses the manuscript on the methodological and physical insights provided by the analogue analysis (rather than on causal attribution) would substantially improve the clarity and impact of the study. In addition, the manuscript would benefit from improvements in structure and clarity. For instance, the introduction remains quite general and is not sufficiently aligned with the specific objectives of the study, while the Results section at times blends results with discussion. A clearer organization and a more cohesive, logically structured narrative would strengthen the presentation of the work

## **Abstract:**

**Line 14:** *"...raising questions about the influence of human-induced climate change."* This is not a novel question as the relationship between climate change and the increasing occurrence and intensity of Mediterranean MHWs is already well documented in the literature.

**Line 19:** It is unclear how this result provides new insights compared to existing studies that have addressed similar questions using other methods.

**Line 25:** "...method for assessing extreme events, ~~also including marine heatwaves,~~ in a warming Mediterranean context"

## **Introduction:**

**Line 29:** A MHW is not an extreme weather event, it is an extreme marine event.

**Lines 40-45:** This literature background on global MHW impacts is already known and less relevant to the 2025 Mediterranean event. The paragraph could be condensed to focus on Mediterranean MHW impacts and the specific insights the study seeks to gain using the analogue method.

**Line 51:** ...The Mediterranean **Sea...**

**Lines 51-53:** How is the series of record-breaking marine heatwaves (why not MHW by the way?) in the past two decades is different from the immediately previous sentence on frequency, intensity and duration of MHWs? These two sentences convey the same information.

**Lines 56-60:** This is not a new regime. MHWs are already known to be becoming more frequent and intense as a results of the mean warming. Especially in the Mediterranean Sea. It is unclear what is novel here. The sentence is also too long: terms like "conditioning synoptic circulation" need clarification. What exactly is meant by these phrases?

**Lines 61-64:** These two sentences are redundant. The question raised here and the effect of anthropogenic climate change on individual extreme events is already well established globally and regionally. It would be more useful to focus on the analogue method itself and clarify how it complements or differs from previous studies addressing similar questions in the Mediterranean Sea.

**Line 75:** The role of atmospheric dynamics in Mediterranean MHWs has already been highlighted multiple times in previous observational and modeling studies. It would be helpful to clarify what (or if) the analogue method adds beyond these earlier investigations of atmospheric forcing, for the specific 2025 event.

**Lines 86-90:** This reads more like a Methods section than an Introduction. Consider moving the detailed description of the analogue approach to Methods and in the Introduction focus on clearly stating the main goal of the paper: investigating the 2025 MHW using the analogue method, with only a brief descriptive mention of its core concept.

## **Data:**

There should be a section describing the method and datasets together. Not separately.

**Line 109-110:** It is unclear what is meant by "average" here. Is this a spatial average over 20-30 June, a temporal average, or both? the daily SST anomalies calculated first and then averaged, or was the (temporal) mean SST over 20-30 June computed before subtracting the climatology ( in space)? Do these approaches yield similar results?

Are these the actual dates (20-30 June) of the 2025 MHW? If so, the Introduction should include a brief description of the event, rather than just listing the dates, to explain why this period was selected.

**Line 115:** This appears redundant with the previous sentence. Additionally, the term “distances” is introduced for the first time here and is unclear. Please clarify its meaning by describing the method where the method section is.

**Lines 126-132:** The dataset details here are excessive; providing just the link is sufficient. The term “refer to” is unclear and it should be explained why this observational dataset was chosen.

**Lines 133-135:** Do you mean the daily Mediterranean-mean SST from the satellite dataset? If so, please clarify. It is also unclear how the timing and magnitude of the 2025 MHW can be compared using a 1991–2020 baseline when the ERA5 anomalies discussed in the study are based on 1950–2025. Can similar anomalies be expected with different climatologies? Since the satellite observations cover 1991–2020, shouldn’t the “present-climate” period be the same to allow a proper comparison? Additionally, please clarify which years are considered as “previous years.”

**Lines 136-137:** I do not understand how OISST is being used. What is meant by “descriptive purposes”? If Level-4 AVHRR data are already used, what is the added value of including another observational dataset? Please clarify how this dataset is applied and which reference period was used to compute anomalies.

## **Methodology:**

**Line 143:** maybe you meant *analogue* events? Also the description feels a lot like repeated from the introduction.

**Line 144:** The phrase “dynamical situation” may be unclear. Here, it would be more precise to refer specifically to the relevant atmosphere state or circulation pattern that drives the observed SST anomalies, rather than implying the entire coupled system, since no ocean variables are included.

**Line 148:** Since this refers to an SST anomaly pattern, it should pertain only to the sea, including coastal regions, unless another aspect is intended, which should be clarified.

**Lines 152-155:** In this description It is unclear whether the closest analogues at each grid point occur on the same days, or how analogue days are selected. Is a spatial average of the distances across grid points computed for each day to identify candidate analogue dates? Please clarify how analogue days are determined consistently.

**Lines 155-159:** What is meant by “fix the number of analogues,” ? You also mention that earlier sensitivity analyses provided robust results. Please specify which analyses were performed and how they support this conclusion.

**Lines 162-163:** You have already stated that in the beginning of the paragraph. Maybe merge?

**Lines 167-168:** what do you mean by “avoid introducing biases from model simulations”? ERA5 is a reanalysis product that assimilates observations and therefore inherently includes biases and errors, so the purpose of this statement is unclear.

**Lines 170-171:** It is still unclear why 42 events were selected, why this number and not another? Additionally, the manuscript has already emphasized multiple times that the analysis compares different background climate conditions.

**Line 178:** main drivers of MHWs during the summer perhaps?

## **Results:**

Section 4 and onwards is the results of the paper:

**Lines 215-216:** Before presenting the Mediterranean-averaged SST anomaly timeseries, it would be more informative to show a map of the event's spatial pattern. This would justify focusing on the Western Mediterranean rather than the entire basin, as the choice is presumably due to the event being more intense there? Currently, the paragraph implies the 2025 MHW spanned the entire basin, but no explanation is provided for narrowing the analysis in the west.

**Line 217:** It is unclear why the 1991-2020 climatology was chosen. According to the Methods, the "present climate" period is 1987-2024, so it would be more consistent to calculate the MHW SST anomalies relative to the same period. If there is a specific reason for using 1991-2020, it should be explained; otherwise, using 1987-2024 would be more appropriate.

**Lines 221-223:** would these results still hold if a climatology of 1987-2024 was used?

**Lines 225-227:** What is meant by "*in the context of recent warming*"? Which period does "recent warming" refer to? Also, would this statement still hold if the climatology were shifted to 1987-2024?

**Lines 228-230:** The reader does not see a map of SST anomalies for the entire basin. Fig. 2a shows only the Western Mediterranean. Later, a west-east gradient is mentioned, but it is unclear what this refers to. Presumably it is a basin-wide gradient across the Mediterranean rather than confined to the Western part. This should be clarified.

**Lines 233:** The phrase "*embedded in a basin-wide warming*" is misleading for two reasons: (1) the 2003 and 2022 events exhibited basin-wide warming except in the Aegean Sea, which was cooler than normal (Darmaraki et al., 2024); (2) a MHW cannot be "embedded" within another MHW, as each event is defined by its own spatial extent.

**Line 237:** This belongs in the Discussion rather than the Results. Additionally, the cited paper discusses co-occurrence of atmospheric and marine heatwaves in general and does not specifically address the 2025 event.

**Lines 237-240:** The figures label the variable as MSL, not MSLP, this should be made consistent throughout. Also, the description of the event characteristics is somewhat disorganized, starting with Fig. 2i and then providing separate comparisons with the literature for each atmospheric variable. It would be clearer to first describe all atmospheric variables for the 2025 event and then discuss past, present and  $\Delta$  differences for each field. Comparisons with previous studies should be moved to the Discussion section, as the current order makes the narrative confusing.

**Line 242:** We are still in the Results section, so it is premature to compare with the literature. In particular, there is no evidence supporting the claim that this synoptic pattern corresponds to a northward shift of the subtropical ridge. Such a statement should be framed as a hypothesis or possibility and addressed in the Discussion section.

**Line 244:** Again, this is Results section, the focus should be on presenting the findings. Discussion of how these results relate to other studies belongs in the Discussion section.

**Line 245:** According to the caption of Figure 2.1, the data represent the average over 20–30 June, so no figure shows the specific conditions on 30 June as mentioned in the text.

**Line 246:** The analysis does not include cloud cover, only wind is shown. Please be precise about which variable is being discussed and refer to the specific figure. Currently, results are mixed with general theory about synoptic conditions associated with Mediterranean MHWs, which reduces clarity.

**Line 248.** If this is the case, it should be addressed in the Discussion section. The Results section on the 2025 atmospheric conditions should first present a clear, coherent description of all relevant variables and only afterward should the writers discuss the likely interpretations or implications of each, in the Discussion section.

**Lines 253-254:** This result is not novel and has already been documented in previous Mediterranean studies. This discussion belongs in the Discussion section. Currently, theoretical knowledge about the co-occurrence of atmospheric and marine heatwaves is mixed with the presentation of your results. While the observed 6 °C air-temperature anomaly may suggest a local atmospheric heatwave, this has not been formally demonstrated, as no climatology or definition of atmospheric heatwaves was applied here. As such, the statement remains speculative.

**Line 255:** Again, I did not see anywhere the cloud variable been presented.

**Line 259-261:** What is the purpose of Figure 1 without a map showing the full spatial pattern of the event across the entire basin? The figure shows anomalies relative to other years, but the reader cannot see how the 2025 MHW was distributed across the Mediterranean.

**Line 263:** ...record-breaking

**Line 265:** Did the studies you compare your results with use the same climatology to define the 2022 events? If different climatologies were used, it is unclear how these comparisons are valid. In any case this belongs to the Discussion section.

**Lines 266-267:** There is no information on the spatial extent of the event or how it differs from previous events. From Figure 2a, it is not clear that >2 °C anomalies cover a larger area than in prior events. The statement that the spatial extent is “larger” (larger than what?) is unclear and unsupported. The figure only shows a concentration of warm anomalies in the Western Mediterranean, which has been observed in past events in this region. The claim, as currently stated, is not substantiated. The authors should clearly demonstrate the validity of their results.

**Lines 268-269;** Again, comparisons with previous studies belong in the Discussion section. Moreover, have you verified whether the anomalies in those studies were calculated using the same climatology as yours? Without this, the comparison is not meaningful.

**Line 269:** which event? the 2003 or 2025? clarify.

**Line 270:** It is unclear which figure this description refers to. Please clarify where the reader is expected to look.

**Lines 273-275:** This statement is expected given the mean warming of the basin. it is unclear what is novel about this argument.

**Line 278:** ...*described* in Section 2..

**Lines 281-287:** This content does not present results. It corresponds to a figure caption and is not relevant to the main text.

**Lines 289-292:** Aside from the warmer background and increased MHW frequency, these results are largely expected: the 2025 event falls right after the present period, so SST patterns

and atmospheric conditions are naturally more aligned with present-day climatology than with past conditions. Again, comparisons with the literature belong in the Discussion section.

**Lines 292-294:** Which figure does this refer to? If it refers to SST in Fig. 2d, it only shows the Western Mediterranean and the Adriatic. Why do the event figures focus solely on the Western Mediterranean, while the atmospheric variables cover the entire basin? For consistency, the full basin should be shown for all variables.

**Line 294-295:** Co-variability of what? Clarify.

**Lines 295-297:** In Figure 2h, only parts of southern Europe and North Africa are shown, so it makes sense that the text refers to values in these regions. Additionally, the statement that the “actual event exceeds both composites” is unclear. Which composites are being referred to, and what type of exceedance is meant, given that Fig. 2a-f shows SST over the Mediterranean, while the other variables cover land and atmosphere? This needs to be clearly explained.

**Lines 297-301:** These belong to the Discussion section again. Why do the MSL patterns suggest a subtropical-ridge warming? The basis for this statement is unclear and not sufficiently substantiated here.

**Line 304:** Since the study does not demonstrate that the SSR patterns are actually linked to anticyclones, this should be framed as a hypothesis rather than a fact and discussed in the Discussion section.

**Lines 308-309:** Has it been demonstrated that higher absolute humidity causes the weaker and more spatially mixed SLHF patterns? If this is a hypothesis rather than a proven result, it should be moved to the Discussion section.

**Line 309:** The differences in SSHF shown in Fig. 2a-b are actually negative around the Mediterranean basin and near zero over the Mediterranean Sea. It is unclear where the authors see positive sensible heat flux differences.

**Lines 313-314:** What is meant by “modest MSL differences”? Fig. 2l shows values around zero or negative, and  $\Delta WSPD$  exhibits both positive and negative variability, so it is unclear how this is consistent with the MSL differences.

What does “*indicating similar dynamics*” also refer to? Similar to what? Please clarify. The description does not clearly explain how the thermodynamic background is shifted.

In general, the current Results section mixes figure description with speculative interpretations based on literature. Most claims about the background thermodynamics belong in the Discussion, and the Results should present a clearer, fact-based account of what actually occurred.

**Line 325:** What defines an “anomalous circulation pattern”? Did the writers expect that the atmospheric thermodynamics would change the sign of their influence (positive or negative) on the ocean during a MHW? The rationale behind this statement is unclear and should be clarified.

**Lines 327-329:** Based on Fig. 2d,h,l,p,t,x,ab,af, the results show:

1. Higher SST in the Western Mediterranean, but not across the rest of the basin (Fig. 2a).
2. Warmer 2m air temperatures over land surrounding the Mediterranean, while over the Sea the anomalies are near zero (Fig. 2h).
3. Increased shortwave radiation input (Fig. 2p).

4. Reduced net longwave loss only over North Africa. The Mediterranean Sea shows zero or positive STR changes (Fig. 2t).
5. Smaller sensible heat flux mainly around the basin, with no clear change over the Mediterranean Sea itself (Fig. 2ab).

These observations indicate that the statement in the text is inconsistent with the figures.

Additionally:

6) What about latent heat flux and wind (Fig. 2x, af)? These variables are relevant in the analogue framework, and latent heat flux (decrease from the ocean to the atmosphere) has been shown to play an important role in previous studies. Here, only small increases are seen over limited Mediterranean regions. How do the authors explain this discrepancy?

**Lines 329-331:** I am not convinced by this analysis of “*coherence across independent variables*”, as it does not provide clear or robust evidence to support the attribution of the 2025 event’s severity to thermodynamic changes. Also the role of the ocean, such as advection and mixing processes, which are important for Mediterranean MHWs, has been entirely neglected.

**Lines 335-343:** This whole paragraph belongs to the Methods section. The methodology for using these large-scale climate modes is entirely absent from the manuscript. The authors do not specify which indices were used for each mode, their source, whether they are 2D fields or simple time series, or their resolution. As a result, the current analysis relies on datasets that the reader cannot trace or understand. The indices should be fully introduced in the Methods section before presenting the analysis. Otherwise, this section lacks clarity and context.

**Lines 360-363:** The discussion of the positive AMO phase describes its influence on the regional summer climate, but does not explain how these conditions specifically affect the Mediterranean basin. Since no evidence is provided for the mechanism through which these large-scale climate modes to Mediterranean MHWs, the claims remain hypotheses rather than substantiated results. The same applies to all other climate modes discussed in this section.

Additionally, “pin-down” is not appropriate for a scientific paper and should be rephrased. Finally, the transition from AMO in the first sentence to ENSO in the next is unclear; the relationship between these two statements should be clarified.

**Lines 368-372:** Figure 3 only compares the distributions of climate mode indices during analogue dates in past and present periods. There is no direct analysis linking these indices to the 2025 MHW, no correlation, regression, mechanism, or attribution is presented. Therefore, any claim that these large-scale modes “explain” or directly drive the 2025 event is unsubstantiated. Such statements should be framed as hypotheses and placed in the Discussion, where a potential mechanism connecting these modes to the event could be outlined, unless the authors provide evidence demonstrating a causal link.

**Lines 373- 380:** This paragraph is not clear or well explained. Rephrase.

**Lines 382-383:** This is expected, since 2025 falls within the present period rather than the past. It is unclear what is novel about this claim or why a different outcome would be anticipated.

## **Discussion:**

**Lines 386-387:** The manuscript does not demonstrate that the 2025 MHW is amplified by anthropogenic climate change. Both the past and present periods used to identify analogues contain contributions from internal variability and climate change. What has been shown is

simply that the 2025 MHW has closer analogues in the present period than in the past, which is naturally anticipated and primarily reflects a warmer background climate rather than isolating the cause of the warming. The differences between the two periods as they are examined here based on ERA5, arise from both anthropogenic forcing and internal variability, or changes in observational/reanalysis datasets. The analogue method detects a shift but does not provide formal attribution to climate change. Even without the analogue method, by using observational datasets alone, (whether a single or multiple datasets), one cannot isolate the effect of climate change properly, because each provides only a single/the same realization of internal variability. Robust attribution of an event to anthropogenic climate change requires the use of model ensembles designed to separate the influence of internal variability from external forcing.

**Lines 401-402:** I do not agree with the statement that these changes are “*purely from the climate shift.*” The study provides no evidence for this, as it relies on a single observational-based dataset, which, as already underlined in previous comments, cannot isolate the climate signal from internal variability. Furthermore, the role of the ocean has not been considered, representing a major limitation in attributing the causes of the event or its intensity. Such statements should be made with caution.

**Lines 402-404:** It is well established from previous studies using formal attribution methods that Mediterranean MHWs are increasingly likely due to climate change. However, the present study does not perform such an attribution. The method only compares synoptic conditions in the “present” versus “past” climate for summer, 2025-like events, which demonstrates thermodynamic amplification during the 2025 MHW, a result that is expected and already documented. Therefore, it is unclear what the novel contribution of this study is: the method, the results, or both.

**Line 406:** The statement “*Here we can say the same for the marine heatwave of 2025*” is not justified, as the study has not performed a formal climate change attribution. The analysis only shows thermodynamic amplification under present conditions, which makes the 2025 event appear more intense than past events, a result that has already been demonstrated in previous studies using other approaches. The authors should focus on how their method provides unique insights into the characterization of the event, rather than claiming to have quantified the effect of climate change, which has not been done.

**Lines 420-423:** Is the discussion here still about MHWs or more generally about atmospheric or compound heatwaves? Please clarify. Also, what is meant by a “warmer ocean-atmosphere baseline”? Are you referring to sensible heat fluxes? These showed little difference between present and past periods, similar to MSL, so it is unclear which atmospheric field this claim is based on.

**Line 426:** The claim that a northward shift of the subtropical ridge influenced the 2025 event has not been demonstrated here. It appears to be inferred visually from MSLP patterns and is therefore unsubstantiated. The authors should either provide evidence to support this claim or reframe it as a hypothesis.

**Lines 436-438:** The manuscript discusses the assumption of stationarity in the analogue approach and argues that pattern similarity in Mediterranean SST anomalies can be meaningfully compared across climates because the spatial structures are largely governed by stationary factors, such as basin geography and recurrent wind systems (e.g., Mistral-driven cooling in the western Mediterranean). While it is true that the Mediterranean basin geometry and dominant atmospheric forcings impose persistent large-scale spatial structures (e.g., west-east contrasts and sub-basin variability), the statement that this likely ensures the validity of the stationarity assumption may be somewhat too strong.

A growing body of literature shows that Mediterranean SST trends and variability have evolved over recent decades, with spatially heterogeneous warming rates (often stronger in the eastern basin) and shifts in basin-scale gradients associated with large-scale modes of variability and long-term warming (e.g. Pastor et al., 2018, Kubin et al 2023). These changes do not necessarily imply that entirely new SST spatial structures appear, but they can modify the magnitude and relative gradients of SST anomalies across the basin. As a result, analogue methods based on historical pattern similarity may still face limitations due to the non-stationarity of the background climate state, particularly when comparing early-period events (e.g., 1950–1986) with recent extremes.

I suggest slightly moderating or clarifying this discussion by acknowledging that, although Mediterranean SST anomaly patterns are indeed constrained by basin geography and recurrent atmospheric forcing, climate change may still alter anomaly magnitudes and spatial gradients in ways that reduce the representativeness of historical analogues. This would provide a more balanced description of the stationarity assumption underlying the analogue framework

**Line 444:** The argument that *moderate analogue distances justify using analogues for attribution* is not valid. Moderate distances only indicate that the 2025 event resembles present patterns; they do not provide evidence that the method can isolate or quantify the effect of climate change. Attribution requires separating the influence of external forcing from internal variability, which analogues alone cannot do. Using analogue similarity as a “justification for attribution” conflates pattern resemblance with causal attribution, so this claim is not substantiated. Why is this considered sufficient justification for using analogues for attribution, when other established methods for climate change attribution exist? Please clarify.

**Lines 447-450:** That claim may apply mainly to summer MHWs. In other seasons oceanic advection often dominates MHW development, and even in summer advection can be important in coastal areas while mixing frequently controls MHWs in regions with strong seasonal wind forcing. Thus the statement is incomplete. A more explicit discussion of the potential contribution of ocean dynamics (which are completely omitted here) would provide a more balanced interpretation of the results.

**Lines 471-472:** This point is valid but already noted in the Introduction, so there is no need to repeat it here.

**Lines 473-474:** It is unclear from the text which sources or studies support the statement that the 2025 event likely caused or will cause significant ecological stress and where. The authors should specify the evidence or references underlying this claim.

**Lines 474-475:** As with many previous events and studies, higher SSTs naturally lead to more intense MHWs. Defining each new event relative to a fixed past baseline means that increasingly extreme events are expected every year. Therefore, it is unclear what is novel in these results.

**Lines 476-477:** Once again, this is not a climate change attribution. The study only applies a method showing an amplified thermodynamic background for SST anomalies. It is unclear whether the same result would hold if the climatology reference period were changed.

**Lines 480-482:** You have not demonstrated how the ocean or the atmospheric variables have responded specifically to climate change. The analysis only shows how atmospheric variables have changed between the past 30 years and the present 30 years, without isolating the effect of climate change.

**Lines 480-491:** The authors' claim that the 2025 event would have been "extremely unlikely" under pre-industrial conditions is not supported by the methods used here. The analogue approach identifies historical cases with similar synoptic patterns to characterise variability. However, it does not provide the framework required to isolate the climate change signal. Without a formal framework (e.g., model ensembles) the study cannot demonstrate the event's probability in a pre-industrial climate. Inferring causation from the absence of historical analogues conflates rarity with attribution and is circular. I recommend removing or substantially rephrasing this claim unless a proper attribution analysis is performed; Alternatively, present it only as a hypothesis and move the discussion to the Discussion section. Using a single, or even multiple, observational datasets cannot isolate the climate change signal from internal variability. Proper separation of these effects typically requires the use of climate model ensembles. This should be mentioned somewhere as a limitation of this study,