

Concurrent atmospheric heatwave drives marine heatwave intensification through air-sea heat flux change

Corresponding Author: Ms Laura Paredes-Fortuny

This file contains all editorial decision letters in order by version, followed by all author rebuttals in order by version.

Attachments originally included by the reviewers as part of their assessment can be found at the end of this file.

Version 0:

Decision Letter:

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Dear Ms Paredes-Fortuny,

Your manuscript titled "Concurrent atmospheric heatwave drives marine heatwave intensification through air-sea heat flux change" has now been seen by 2 reviewers, and we include their comments at the end of this message. They find your work of interest, but some important points are raised. We are interested in the possibility of publishing your study in Communications Earth & Environment, but would like to consider your responses to these concerns and assess a revised manuscript before we make a final decision on publication.

In your revised manuscript, you must address the following editorial thresholds:

1. To ensure clarity and consistency, all methodological details, particularly event definitions, composite event counts, and key terminology must be clearly stated in the methods section. Additionally, the type of statistical test used to assess significance should be explicitly reported. Please take a look at comments from both the reviewers
2. Clearly state the novel contributions of this study and distinguish them from previously published findings. If a key conclusion has been reported in earlier work, the manuscript must explicitly highlight the new insights or advancements presented.
3. Place the findings within a broader global climate change context. Incorporating comparisons with other regions—emphasizing shared trends or unique characteristics of the Mediterranean AHWs and MHWs will strengthen the study's relevance and overall impact.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response that takes into account the points raised by both the reviewers. Please highlight all changes in the manuscript text file.

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Please use the following link to submit your revised manuscript, point-by-point response to the referees' comments (which should be in a separate document to any cover letter), a tracked-changes version of the manuscript (as a PDF file) and the completed checklist:

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We hope to receive your revised paper within six weeks; please let us know if you aren't able to submit it within this time so that we can discuss how best to proceed. If we don't hear from you, and the revision process takes significantly longer, we may close your file. In this event, we will still be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

Please do not hesitate to contact us if you have any questions or would like to discuss these revisions further. We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

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REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

This study used DRA5 reanalysis data to understand the air-sea heat flux drivers of concurrent atmospheric and oceanic marine heatwaves in the Mediterranean Sea and identified that the latent heat flux played a key role in terminating these events. This effect is suggested to be strengthening under the influence of climate change. While this is an interesting study, there are need for further improvement to the study, so I can't recommend the publication in its current form. Here are some main comments.

Some of the method descriptions should be brought up upfront, such as how the events are defined and how many events are in the composites, what's a concurrent and non-concurrent day etc. Otherwise, it is hard to follow in the result section.

As stated by the authors, the conclusion that the concurrent marine heatwaves are more intense was published in their previous work (citation 40). So the authors need to present what's new in this study on this aspect.

In the schematic, Figure 9 (similarly in Figure 7), the marine heatwave is always longer than atmospheric events. Is this always the case?

If the latent heat flux increase is the key driver to terminate the concurrent event, should the marine heatwaves be terminated, instead of the atmospheric heatwaves?

Fig. 1: the daily mean intensity doesn't reflect the peak distribution, so maybe showing the median values is a better option. It appears that the distributions have a larger spread during the concurrent events, which needs to be explained.

There are plenty of statistical analyses, however, there is a need for more mechanism understanding. Such as what's the driver of marine heatwaves prior to the onset of the concurrent events? What's the typical driver of atmospheric heatwaves?

In Fig. 7, cloud cover and atmospheric circulation variations should be included.

It could be useful to expand the 2003 event analysis as a case study.

Some of the English language can be better polished.

Reviewer #2 (Remarks to the Author):

I read this manuscript with great interest as it offers a novel comprehensive analysis of the heat fluxes involved in the compound occurrence of atmospheric and marine heatwaves from 1949 to 2022, particularly before, during and after their co-occurrence. This topic is of high importance to the scientific community and has not been extensively explored before. Upon review, I have some comments for the authors that can be further addressed before publication and could help clarify the content and improve interpretation.

1. Introduction:

a. Lines 118-123 could be more effectively integrated into the end of the current paragraph (lines 136-138 for instance) to create a more cohesive and focused paragraph dedicated exclusively to the impacts of marine heatwaves on atmospheric processes. This restructuring would help improve the organization of the text by ensuring a more logical progression of ideas.

b. Lines 138-140: The phrase regarding the amplified impacts of concurrent marine heatwaves could be effectively incorporated into or immediately following paragraph 84–95 to enhance the coherence of the text. Additionally, and in parallel, referencing also other types of amplified impacts that arise from the co-occurrence of such events would offer a more comprehensive understanding of their broader implications. Incorporating a paragraph focused on this theme would contribute valuable information to the introduction.

2. Methodology choices

- a. Lines 403-405: The authors refer "ERA5 temperature data has been found to compare well with observations from station data for AHW analysis in the Mediterranean region." A similar supporting reference justifying the use of ERA5 data for the study of marine heatwaves, as opposed to satellite data, for instance, should also be included.
- b. Lines 407-409: The authors state at the beginning of the methodology section that they analyze Mediterranean marine heatwaves during the May to September period. This information is repeated in lines 440–441, where it is accompanied by a justification that was not initially given. I recommend mentioning this detail only once—preferably by removing the first paragraph (or at least the reference to the study being limited to the extended summer period from the beginning of the methodology section), as it is more clearly and effectively presented later.
- c. Line 424: The reference of the WMO that justifies using the reference period 1961-1990 should be mentioned in the references list.
- d. The authors use Pastor et al. (2024) and Hobday et al. (2016) definitions to determine HW events. But according to these studies, HW events separated by only one day not meeting the HW conditions are considered the same event. Did the authors also apply this detail to their study? Both to AHWs and MHWs? If so, this should also be added to the methodology section.
- e. Lines 428-430: This phrase does not appear to be well contextualized within this section, its relevance or connection to the surrounding content is unclear.
- f. Lines 431-432: "Previous definitions are applied to SST and T2m data to obtain a spatiotemporal tracking of MHWs and AHWs, respectively." What references? Please show examples presented in previous investigations, justifying the choice of using the definition of concurrence given by Pastor et al. (2024).
- g. Lines 434-438: "This definition ensures a coherent spatial analysis as at least 75% overlapping area between two consecutive concurrent days in most concurrent MHWs in the Mediterranean Sea". This sentence is unclear, please clarify its meaning.
- h. Line 439: Could the authors clarify what they mean by the term "marine heatwave catalogue"?
- i. Figure 8: From what I understand, the concurrent points correspond to the dark blue points shown over the ocean. If this is the case, the authors should consider adding a legend or explanatory note to clarify this detail. Additionally, it would be helpful to specify whether the AHWs occurring over land are utilized in any part of the study since they are identified as well in this figure.
- j. Line 475: Why did the authors choose a five-day period for the before and after concurrence phases?
- k. Lines 510-517: Dividing the reasoning in this paragraph into distinct bullet points would help the reader follow the authors' analysis more clearly and gradually.
- l. Lines 521-522: Please consider including this last phrase of the paragraph in the results section corresponding to this topic.
- m. Lines 523-525: The authors refer to the "selected interval" in this section. Would you please clarify whether this is equivalent to the "interest interval"? It would be helpful to define the term more explicitly to avoid confusion.

3. Results section:

- a. Lines 166-169 would be more appropriately placed in the discussion section rather than the results section.
- b. It would be helpful to include a small legend above the graphics in the results section indicating "Concurrent" and "Non-Concurrent" periods. Although this distinction can be inferred by reading the legends, explicitly labelling it would offer clearer guidance and make interpretation easier for the reader.
- c. Figure 1: I had some difficulty understanding why areas with a higher percentage of concurrent points (orange coloured) appear in regions of the graphic that correspond to non-concurrent points. If my interpretation is incorrect, I would appreciate clarification from the authors and suggest that this explanation could be added to the manuscript.
- d. Lines 249-251: The phrase "the LH flux change between the penultimate concurrent day and the first day after concurrence end [...]", along with similar expressions throughout the manuscript, could be replaced with the term "interest interval", as this term was clearly defined in the methodology section. Doing so would improve readability by avoiding repetitive descriptions.
- e. Paragraph 265-268: This sentence is unclear and appears to have structural issues that cause difficulty in its comprehension. Please review it.
- f. Paragraph 299-307: This part of the methodology, in contrast to the others, was not explained in the methodology section. To maintain consistency, consider including this information in the methods section. Please also specify which type of statistical test was used to infer the results of significance.

4. Discussion and contextualization

- a. A more comprehensive contextualization would enhance the paper by situating its findings within the broader global framework of climate change. Drawing comparisons between the Mediterranean region and other areas worldwide could outline shared patterns or distinct challenges.

5. Minor comments/corrections:

- a. Line 81: the terms "MHW and AHW" should be "MHWs and AHWs".
- b. Lines 296-297: "equal 0" should probably be "equal to 0", and "(a)" and "(b)" would be better fitted before, correspondingly, "positive" and "negative" words.
- c. Lines 310-314: Please add to the legend of Figure 6 that the analysis is focused solely on T1.
- d. Line 322: Replacing the term "influence" with "may influence".
- e. Lines 401-403: I believe the reference to (C3S-CDS, 2023) is not included in the reference list. If it is not, it should be added, as it is mentioned in the text. Additionally, the inclusion of the HTTP link is unnecessary if it is added to the

references section.

- f. I suggest that the distinct subtopics within the first part of the methodology section could be separated into individual paragraphs for improved clarity and readability—for example: one paragraph for the definition of MHWs, a separate paragraph for the definition of AHWs, and another for the elements common to both MHWs and AHWs (concurrence).
- g. Line 447: “An MHW” should be “a MHW”.
- h. Line 460: The title of this section could be revised to something more specific, such as “Marine Heatwave Intensity and Heat Flux Analysis During Concurrent Periods” or something like this to better reflect the content discussed.
- i. Lines 484-486: the sentence “As MHWs have different durations, and on each grid point, concurrence may start and lasts for different times, and thereafter. it is difficult to compare different full concurrent MHW events.” would read more clearly if “and thereafter” was removed. The word “lasts” should be replaced by “last”.
- j. Paragraph 494-499: please consider adding a reference to Figure 9 within this paragraph to reinforce the connection between the discussion and the visual representation. Alternatively, you could merge this paragraph with the preceding one.
- k. Line 507: “detail” should be “detailed”.

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Decision Letter:

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Dear Ms Paredes-Fortuny,

Your manuscript titled "Concurrent atmospheric heatwave drives marine heatwave intensification through air-sea heat flux change" has now been seen by our reviewers, whose comments appear below. In light of their advice we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment.

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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We hope to hear from you within two weeks; please let us know if you need more time.

Best regards,
Alice

Alice Drinkwater, PhD
Associate Editor
Communications Earth & Environment
Consulting Editor
Communications Sustainability

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

The authors have made an effort to address the previous comments, which have resolved many of my concerns. There are still areas that need clarification before the manuscript can be considered for publication in the journal.

1. The title of the manuscript is "Concurrent atmospheric heatwave drives marine heatwave intensification through air-sea heat flux change". However, this point is barely mentioned in the abstract, except in a phrase, "heat flux disruption". Instead, the focus is on the sharp increase of latent heat "at concurrence end". These should be better clarified.

2. Line 146-147: "predictability" is low?

3. line 153-155: This sentence reads awkwardly.

4. line 162-163: Need a brief introduction of the method. The figure implies that MHW is longer than AMW, which needs to be stated. Or you have only looked at those cases.

5. Line 239-240: I think there needs to be some explanation of why this is the case. There should be a delayed effect of air-sea heat flux anomalies on SST. This is the key point highlighted in the title.

Reviewer #2 (Remarks to the Author):

I have a few minor comments for the authors that could be further addressed before publication:

1. Regarding my previous comment "Line 424: The reference of the WMO that justifies using the reference period 1961-1990 should be mentioned in the references list.", I think the citation is not mentioned in the references list, although it is justified in the text. A comma "," is missing between words "organisation" and "for analysis" in line 443.

2. Fig. 8 is missing latitude and longitude identification (eg:N,E).

3. Line 548, "selected interval" should be "interest interval".

4. Figs. 1 and 3 of the results section are missing the point.

5. Line 555, "student t distribution" should be "student's t-distribution".

6. Lines 317-321: Please add to the legend of Figure 6 that the analysis is focused solely on T1.
7. Line 150, "unknown" instead of "runknown".
8. Lines 424-434, "an MHW" should be "a MHW".
9. Line 440, "a AHW" should be "an AHW".
10. Line 193, "no-MHW" should probably be "non-MHW days" for consistency.

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Reviewer #1 (Remarks to the Author):

This study used ERA5 reanalysis data to understand the air-sea heat flux drivers of concurrent atmospheric and oceanic marine heatwaves in the Mediterranean Sea and identified that the latent heat flux played a key role in terminating these events. This effect is suggested to be strengthening under the influence of climate change. While this is an interesting study, there are need for further improvement to the study, so I can't recommend the publication in its current form. Here are some main comments.

We would like to thank the reviewer for their thoughtful evaluation of our study. We appreciate the valuable comments and suggestions, which we will carefully consider as they will undoubtedly enhance the scientific rigor and clarity of our results.

Regarding the main conclusion of our study on latent heat fluxes, we would like to clarify that we do not claim changes in latent heat flux are the driver of concurrence termination. Rather, we indicate that this is a physical process observed during that time. By definition, concurrence occurs when both AHW and MHW conditions are simultaneously present over a widespread area. Once either event ceases, whether due to atmospheric or oceanic processes, or shifts spatially, the concurrence naturally ends.

- Some of the method descriptions should be brought up upfront, such as how the events are defined and how many events are in the composites, what's a concurrent and non-concurrent day etc. Otherwise, it is hard to follow in the result section.

Thank you for your feedback. We appreciate your observation regarding the clarity of the results section in the absence of a more detailed methodology description. In accordance with the journal's guidelines, we have provided the full methodological details in the Methods section. To enhance clarity and facilitate readers' understanding, we have now added a reference—“(see section Marine Heatwave Catalogue in Methods)”—at line 163, directing readers to the relevant methodological details.

- As stated by the authors, the conclusion that the concurrent marine heatwaves are more intense was published in their previous work (citation 40). So, the authors need to present what's new in this study on this aspect.

We appreciate your comment. As indicated by the reviewer, our previous results showed a marine heatwave intensification in the presence of atmospheric heatwaves. The novelty in the present study is that here we show that the intensification is progressive on a two-day scale, and that the MHW intensification symmetrically descends after the end of concurrence. The present study builds on our previous results to go a step further in the search for the physical/energetic processes

responsible for the intensification of the marine heatwaves. Again, ERA5 data have been used in the analysis of energy and heat fluxes at the atmosphere-ocean interface, with the key role of latent heat fluxes in MHW intensification being the main, and novel, result obtained.

We have added a clarification in lines 356-359: “The present analysis shows a MHW intensification during concurrence, in agreement with previous studies ³⁶. Our results show, for the first time to our knowledge, that this MHW intensification in concurrence with AHW occurs gradually during the first days after concurrence onset.”.

- In the schematic, Figure 9 (similarly in Figure 7), the marine heatwave is always longer than atmospheric events. Is this always the case?

Yes, the MHW duration is usually longer than the AHW duration in the Mediterranean region. There are two relevant factors that explain these differences. First, the intrinsic differential thermal response of the atmosphere compared to the sea surface. The sea has a greater heat capacity than the air which hinders heat dissipation and maintains temperature values above the considered threshold for a longer time in the ocean. Consequently, the sea surface temperature anomalies tend to vanish slowly than that of the 2m air temperature. Second, there are differences in their dynamics. The speed and temporal scales of the oceanic circulation are in general slowly than in the atmosphere, which leads to more stationary and persistent MHWs compared to AHWs. Another relevant factor is the different temperature threshold in their definitions, being the 95th for AHW and the 90th for MHW. A higher temperature threshold implies that there are less days with a temperature anomaly. Consequently, the chances of having consecutive temperature anomalies are lower for the AHWs than for MHWs, limiting the AHW duration.

- If the latent heat flux increase is the key driver to terminate the concurrent event, should the marine heatwaves be terminated, instead of the atmospheric heatwaves?

AHW and MHW concurrence is not a single, distinct or isolated event but the spatiotemporal coincidence of two different extreme heat events. In this sense, the end of the concurrence in one area does not imply that the concurrence has not moved to another nearby area, nor does it imply the end of the independent events necessary for the concurrence to appear.

In our previous study (Pastor et al 2024) we showed that during concurrence MHW are more intense, and that this intensification subsides when concurrence with AHW ends. The processes documented in the present study do not imply the end of an event. Instead, we show the processes related to heat fluxes that are observed when the condition of concurrence –not the end of AHW or MHW –finalises.

- Fig. 1: the daily mean intensity doesn't reflect the peak distribution, so maybe showing the median values is a better option. It appears that the distributions have a larger spread during the concurrent events, which needs to be explained.

Thank you for the comment. We understand that the mean is not completely representative of the MHW intensification although reveals a 0.25°C intensification. Figure 1 displays the full distribution of the intensity at each time step complementing the mean and showing the daily variability. We have rephrased lines 169-171 to explain the spread around the mean: “We observe a MHW intensification on concurrent points on the concurrence onset of about 0.25°C on average (Figure 1a) but with a significant variability around the average”.

- There are plenty of statistical analyses, however, there is a need for more mechanism understanding. Such as what's the driver of marine heatwaves prior to the onset of the concurrent events? What's the typical driver of atmospheric heatwaves?

The different drivers of independent AHW and MHW events have been extensively studied in scientific literature, in the Mediterranean and at global scales (see references in Capotondi et al 2024). Our work does not aim to investigate the drivers of the single extreme heat events, or even of the concurrence of both. The purpose of the present study is to assess the processes that take place in situations of concurrence at the air-sea interface in terms of energy/heat flux/balance. Nevertheless, in the analysed period (May-September) most Mediterranean AHWs are associated with an anticyclonic stable situation with strong stagnation and subsidence. This persistent atmospheric stability is usually found to be a driver of MHWs through an increase of solar radiation (clear skies) and calm winds that, in turn, favours upper marine levels stratification, starting or reinforcing MHWs.

We have added the following phrases to describe AHW and MHW drivers in lines 113-118: “Most Mediterranean AHWs are associated with an anticyclonic stable situation with strong stagnation and subsidence¹⁴. This persistent atmospheric stability is usually found to be a driver of MHWs through an increase of solar radiation (clear skies) and calm winds that, in turn, favours upper marine levels stratification, starting or reinforcing MHWs¹⁵”.

Capotondi, A. *et al.* A global overview of marine heatwaves in a changing climate. *Communications Earth and Environment* vol. 5 at <https://doi.org/10.1038/s43247-024-01806-9> (2024).

Zschenderlein, P., Fink, A. H., Pfahl, S., & Wernli, H. (2019). Processes determining heat waves across different European climates. *Quarterly Journal of the Royal Meteorological Society*, 145(724), 2973–2989. <https://doi.org/10.1002/qj.3599>

- In Fig. 7, cloud cover and atmospheric circulation variations should be included.

We appreciate the reviewer's comment and acknowledge that incorporating additional factors into Figure 7 could provide a more comprehensive overview. However, this would also increase its complexity and extend beyond the primary scope of our study, which focuses specifically on the behaviour of heat fluxes during concurrent AHW and MHW events.

It is important to note that cloud cover is indirectly represented in Figure 7 through the depiction of the SWRnet flux. Regarding atmospheric circulation, we fully agree with the reviewer on its critical role as a driver of both AHWs and MHWs, as well as their concurrence. This influence is reflected in Figure 7 by illustrating the reduction in wind intensity, which is typically associated with the anticyclonic stability characteristic of AHWs. We have added a clarification on that in lines 363-364: "Synoptic blocking—characterized by increased shortwave radiation, associated with reduced cloud cover, and reduced wind speed—leads to a shallower mixing layer that limits the entrainment of colder, deeper waters, thereby enhancing heat accumulation".

Ultimately, the main objective of our study was to analyze heat flux dynamics during concurrent events. Figure 7 is intended to highlight the key processes related to MHW intensification in these situations, rather than to provide an exhaustive representation of all possible contributing factors.

- It could be useful to expand the 2003 event analysis as a case study.

Thank you for the comment. We agree that expanding our analysis with a case study could be useful. In fact, we want to explore several case studies to cover the most frequent types of AHW-MHW concurrence in a future work.

- Some of the English language can be better polished.

Thank you for the comment, we reviewed the English language and improved the writing.

Reviewer #2 (Remarks to the Author):

I read this manuscript with great interest as it offers a novel comprehensive analysis of the heat fluxes involved in the compound occurrence of atmospheric and marine heatwaves from 1949 to 2022, particularly before, during and after their co-occurrence. This topic is of high importance to the scientific community and has not been extensively explored before. Upon review, I have some comments for the authors that can be further addressed before publication and could help clarify the content and improve interpretation.

We are grateful for the reviewer's positive feedback on our work and its relevance, particularly given the Mediterranean's status as a climate change hotspot. We have carefully considered and incorporated all of the reviewer's comments into the revised manuscript.

1. Introduction:

a. Lines 118-123 could be more effectively integrated into the end of the current paragraph (lines 136-138 for instance) to create a more cohesive and focused paragraph dedicated exclusively to the impacts of marine heatwaves on atmospheric processes. This restructuring would help improve the organization of the text by ensuring a more logical progression of ideas.

Thank you for your comment. We shortened the paragraph previous to the heat flux impacts on marine heatwave to improve the organization (lines 128-133).

b. Lines 138-140: The phrase regarding the amplified impacts of concurrent marine heatwaves could be effectively incorporated into or immediately following paragraph 84–95 to enhance the coherence of the text. Additionally, and in parallel, referencing also other types of amplified impacts that arise from the co-occurrence of such events would offer a more comprehensive understanding of their broader implications. Incorporating a paragraph focused on this theme would contribute valuable information to the introduction.

Thank you for the comment, we moved the phrase as suggested. We agree on the relevance of an additional paragraph focused on other impacts of atmospheric-marine concurrence; however, to the best of our knowledge, there are no other references at the moment.

2. Methodology choices

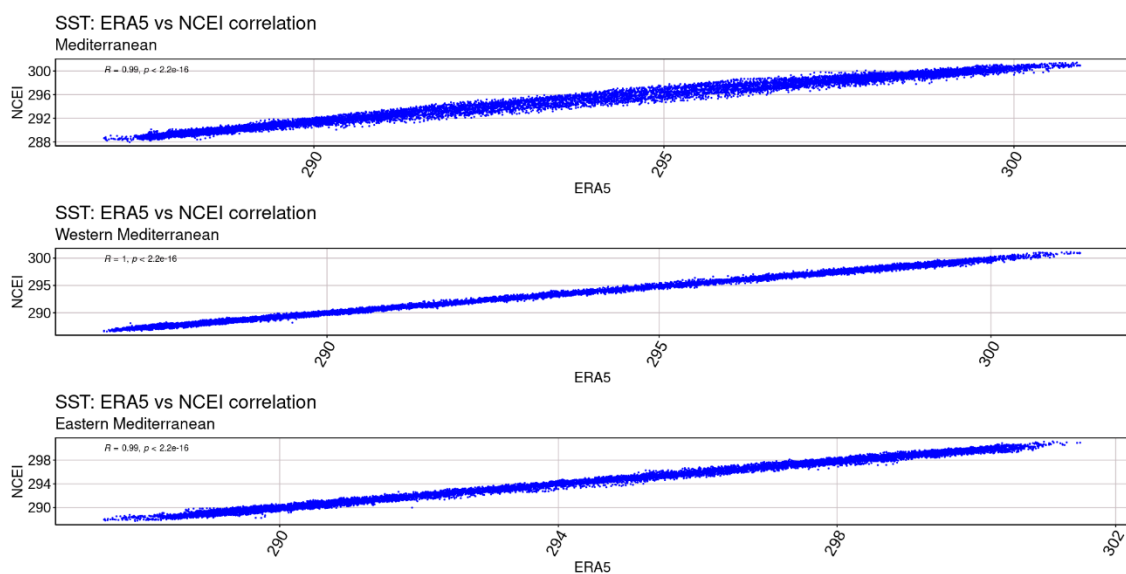
a. Lines 403-405: The authors refer “ERA5 temperature data has been found to compare well with observations from station data for AHW analysis in the Mediterranean region.” A similar supporting reference justifying the use of ERA5 data for the study of marine heatwaves, as opposed to satellite data, for instance, should also be included.

We agree with the reviewer that the feasibility of ERA5 SST data use should be supported by references from other authors. We have included in the manuscript a reference (lines 414-417) to two different evaluation studies comparing ERA5 SST with observational data from different sources.

Luo, B. & Minnett, P. J. Evaluation of the ERA5 sea surface skin temperature with remotely-sensed shipborne marine-atmospheric emitted radiance interferometer data. *Remote Sensing* **12**, (2020).

Yao, L., Lu, J., Xia, X., Jing, W. & Liu, Y. Evaluation of the ERA5 sea surface temperature around the pacific and the atlantic. *IEEE Access* **9**, 12067–12073 (2021).

Besides, in our previous work (Pastor et al 2024) we calculated the correlation between ERA5 and satellite data from GHRSSST Level 4 AVHRR_OI Global Blended Sea Surface Temperature Analysis (GDS2) from NCEI (AVHRR_OI-NCEI-L4-GLOB-v2.1) that has the same spatial resolution (0.25°), finding a strong agreement for the mean Mediterranean SST in the 1982-2021 period, see figure (not included in the manuscript).



ERA5 has also been used in the analysis of AHW in the Mediterranean in references already included in the manuscript.

b. Lines 407-409: The authors state at the beginning of the methodology section that they analyze Mediterranean marine heatwaves during the May to September period. This information is repeated in lines 440–441, where it is accompanied by a justification that was not initially given. I recommend mentioning this detail only once—preferably by removing the first paragraph (or at least the reference to the study being limited to the extended summer period from the beginning of the methodology section), as it is more clearly and effectively presented later.

Thank you for the comment, we removed the mention of the extended summer in lines 407-409 and added it in lines 440-441.

c. Line 424: The reference of the WMO that justifies using the reference period 1961-1990 should be mentioned in the references list.

Thank you for your noticing the lack of this reference, we have added it to the reference list.

The WMO climatological normal (<https://community.wmo.int/en/wmo-climatological-normals>) states that, although the most-recent 30-year period finishing in a year ending with 0 should be used in recent climate analysis, “the period from 1961 to 1990 has been retained as a standard reference period for long-term climate change assessments”. As we were interested in the concurrence of atmospheric and oceanic events and their long-term changes, we applied the same reference period for both phenomena.

d. The authors use Pastor et al. (2024) and Hobday et al. (2016) definitions to determine HW events. But according to these studies, HW events separated by only one day not meeting the HW conditions are considered the same event. Did the authors also apply this detail to their study? Both to AHWs and MHWs? If so, this should also be added to the methodology section.

Yes, we have applied this detail in AHWs and MHWs. Two AHWs separated by two days are the same event, and two MHWs separated by two days or less are the same event. Thank you for the comment, we have now added this information in lines 433, and 441.

e. Lines 428-430: This phrase does not appear to be well contextualized within this section, its relevance or connection to the surrounding content is unclear.

Thank you for the comment, we have eliminated this phrase.

f. Lines 431-432: “Previous definitions are applied to SST and T2m data to obtain a spatiotemporal tracking of MHWs and AHWs, respectively.” What references? Please show examples presented in previous investigations, justifying the choice of using the definition of concurrence given by Pastor et al. (2024).

Thank you for the comment. In lines 431-432 we were referring to the definitions described in the previous paragraphs. We have rephrased lines 447-449 to clarify this “The AHW and MHW definitions described in the previous paragraphs are applied to SST and T2m data to obtain a spatiotemporal tracking of MHWs and AHWs, respectively.”.

To the author’s knowledge, the concurrence definition given by Pastor et al. (2024) has not yet been used in other studies. We have added a phrase of the justification in Pastor et al. (2024) to use this definition (lines 444-446): “This baseline was tested for AHW and MHW analysis in the Mediterranean concurrent HW, defined as the spatiotemporal co-occurrence of MHW and AHW for at least three consecutive days⁴¹, which coincides with the minimum duration of AHW.”.

g. Lines 434-438: “This definition ensures a coherent spatial analysis as at least 75% overlapping area between two consecutive concurrent days in most concurrent MHWs in the Mediterranean Sea”. This sentence is unclear, please clarify its meaning.

Thank you for the comment, we have rephrased it: “The consecutive days that were identified as concurrent days had at least 75% of their area overlapping in most of the cases, ensuring spatial coherence”.

h. Line 439: Could the authors clarify what they mean by the term “marine heatwave catalogue”?

Thank you for the comment, by “marine heatwave catalogue” we mean “marine heatwave list”. We have rephrased it to clarify it: “The present analysis is based on the MHW catalogue described in Pastor et al.⁴¹, which is a list of the 169 identified MHW events in the extended summers (May to September) from 1940 to 2022.”.

i. Figure 8: From what I understand, the concurrent points correspond to the dark blue points shown over the ocean. If this is the case, the authors should consider adding a legend or explanatory note to clarify this detail. Additionally, it would be helpful to specify whether the AHWs occurring over land are utilized in any part of the study since they are identified as well in this figure.

Thank you for your suggestion to improve this figure. At the first version, concurrent points were not intentionally marked with a darker blue colour. Instead, the overlapping of purple and light blue points created the effect of darker blue points in the overlapping area. We agree that this could be confusing for the reader. We have now marked the concurrent points with a distinct colour (black) to improve the figure's readability, and we have accordingly modified the legend and caption.

Regarding AHW over land, we have not constrained the AHW identification process over the sea but have included the Mediterranean Basin. However, in the present study, we only have analysed the AHWs over the sea, excluding their land areas, but in a future work we will use the AHW over land to investigate AHWs in coastal areas.

j. Line 475: Why did the authors choose a five-day period for the before and after concurrence phases?

In our analysis we tested different periods for the before and after concurrence phases. Periods from 1 day up to 10 days were tested and no significant differences were found for periods longer than 5 days while shorter periods could mask some relevant intensity variations.

In addition to the sensitivity tests, we tested extending Figure 1, Figure 3, and Figures S1, S2, S3, and S4 up to 10 days. These figures show the daily probability distribution of the MHW intensity and heat flux components around the onset and end of concurrence. The extended figures revealed that before/after day 5, there were no major changes in the variable examined.

Therefore, we chose the 5-day period because it is in this period where we observe the major changes. Thank you for the comment, we have now included a clarification "Based on these tests, we use a length of five days for the before/after-concurrence phases because the major changes were observed during the first 5 previous/posterior days to concurrence onset/end."

k. Lines 510-517: Dividing the reasoning in this paragraph into distinct bullet points would help the reader follow the authors' analysis more clearly and gradually.

Thank you for the comment, we have added two bullet points.

l. Lines 521-522: Please consider including this last phrase of the paragraph in the results section corresponding to this topic.

Thanks for the comment, we have added this phrase in the latent heat flux variation section lines 263-266: “We only show this analysis for the *interest interval* on T2 (from the penultimate concurrent day to the first day after concurrence ends) because of its bigger magnitude with respect to T1 (the day before concurrence onset to the second concurrent day).”.

m. Lines 523-525: The authors refer to the "selected interval" in this section. Would you please clarify whether this is equivalent to the "interest interval"? It would be helpful to define the term more explicitly to avoid confusion.

Thanks for the comment. The correct term is “interest interval”, we have corrected it now.

3. Results section:

a. Lines 166-169 would be more appropriately placed in the discussion section rather than the results section.

Thanks for the comment, we have moved it to the discussion section (lines 356-359).

b. It would be helpful to include a small legend above the graphics in the results section indicating "Concurrent" and "Non-Concurrent" periods. Although this distinction can be inferred by reading the legends, explicitly labelling it would offer clearer guidance and make interpretation easier for the reader.

Thanks for the suggestion. However, Adding the titles “concurrent” or “non-concurrent” would be confusing because there are concurrent and non-concurrent days on each subfigure. Please note that we have rephrased the Figure legend to clarify its interpretation (see the following comment).

c. Figure 1: I had some difficulty understanding why areas with a higher percentage of concurrent points (orange coloured) appear in regions of the graphic that correspond to non-concurrent points. If my interpretation is incorrect, I would appreciate clarification from the authors and suggest that this explanation could be added to the manuscript.

Thanks for the comment, we see that the figure description should be clearer. Figure 1 shows an analysis of the intensity at concurrent points of MHWs. Concurrent points of MHWs are the points that are concurrent at some time, and we track their intensity before, during, and after concurrence. In Figure 1a, the x-

axis ranges from the 5 days prior to concurrence onset to the 5 days after concurrence onset. Figure 1a shows the intensity probability distribution on each day, which is the percentage of points (color scale) with a given intensity (y-axis). Figure 1b is analogous to Figure 1a but with the x-axis ranging from the 5 days prior to concurrence end to the five days after concurrence end. We have changed the color scale title to “Points (%)” and we have rephrased the legend “MHW intensity on AHW-MHW concurrent points of the MHW from May to September 1940 to 2022. Probability distribution of the MHW intensity (°C) around the a) concurrence onset (from the five prior days to concurrence onset to the first five concurrent days, T1) and the concurrence end (from the five last concurrent days and the five days after concurrence end, T2). The coloured area represents the percentage of MHW points with a given intensity. The dotted black line shows the daily mean MHW intensity, and the shadowed area between thin black lines depicts the daily standard deviation.” to clarify the description of Figure 1. Additionally, we have applied the same changes in Figures 3, and S1 to S4.

d. Lines 249-251: The phrase “the LH flux change between the penultimate concurrent day and the first day after concurrence end [...]”, along with similar expressions throughout the manuscript, could be replaced with the term “interest interval”, as this term was clearly defined in the methodology section. Doing so would improve readability by avoiding repetitive descriptions.

Thank you for the comment, we have applied your suggestion on lines 257, 259, 264, 265, 271, 280, 283, 287, and 288.

e. Paragraph 265-268: This sentence is unclear and appears to have structural issues that cause difficulty in its comprehension. Please review it.

We appreciate your comment. This sentence was part of a sentence that was removed in the manuscript version that we submitted to the journal and should have been deleted too. We have now removed it.

f. Paragraph 299-307: This part of the methodology, in contrast to the others, was not explained in the methodology section. To maintain consistency, consider including this information in the methods section. Please also specify which type of statistical test was used to infer the results of significance.

Thanks for the comment, we have now included this explanation in the methods section (lines 552-555): “Then we evaluate if there is a relationship between the MHW intensity and the LH change at the interest interval T2. We calculate the Pearson correlation for each MHW using the MHW intensity-LH pairs of its concurrent area, and the corresponding p-value obtained from the student t distribution.”.

4. Discussion and contextualization

a. A more comprehensive contextualization would enhance the paper by situating its findings within the broader global framework of climate change. Drawing comparisons between the Mediterranean region and other areas worldwide could outline shared patterns or distinct challenges.

To better place our work in the global climate change context, three new references and related comments have been included in the manuscript *Introduction* section, please see the new first paragraph on the introduction (lines 72-80).

Merchant, C. J., Allan, R. P. & Embury, O. Quantifying the acceleration of multidecadal global sea surface warming driven by Earth's energy imbalance. *Environmental Research Letters* **20**, (2025).

Cheng, L. *et al.* New Record Ocean Temperatures and Related Climate Indicators in 2023. *Advances in Atmospheric Sciences* **41**, 1068–1082 (2024).

Cheng, L. *et al.* Past and future ocean warming. *Nature Reviews Earth and Environment* vol. 3 776–794 at <https://doi.org/10.1038/s43017-022-00345-1> (2022).

5. Minor comments/corrections:

a. Line 81: the terms “MHW and AHW” should be “MHWs and AHWs”.

Thank you for the comment, we corrected it.

b. Lines 296-297: “equal 0” should probably be “equal to 0”, and “(a)” and “(b)” would be better fitted before, correspondingly, “positive” and “negative” words.

Thank you for the comment, we corrected it.

c. Lines 310-314: Please add to the legend of Figure 6 that the analysis is focused solely on T1.

Thank you for the comment, we added it.

d. Line 322: Replacing the term “influence” with “may influence”.

Thank you for the comment, we replaced it.

e. Lines 401-403: I believe the reference to (C3S-CDS, 2023) is not included in the reference list. If it is not, it should be added, as it is mentioned in the text. Additionally, the inclusion of the HTTP link is unnecessary if it is added to the references section.

Thank you for the comment. After reviewing the data section, we noticed that lines 401-403 “ERA5 data from 1940 to 2022 were accessed in the MARS archive through the C3S Climate Data Store (C3S-CDS, 2023) facility (<https://cds.climate.copernicus.eu/>).” should be included in the Data Availability Section and we move it to the corresponding section.

f. I suggest that the distinct subtopics within the first part of the methodology section could be separated into individual paragraphs for improved clarity and readability—for example: one paragraph for the definition of MHWs, a separate paragraph for the definition of AHWs, and another for the elements common to both MHWs and AHWs (concurrency).

Thank you for the comment, we added the paragraph suggested separations.

g. Line 447: “An MHW” should be “a MHW”.

Thank you for the comment, we corrected it.

h. Line 460: The title of this section could be revised to something more specific, such as “Marine Heatwave Intensity and Heat Flux Analysis During Concurrent Periods” or something like this to better reflect the content discussed.

Thank you for the comment, we updated the title to “MHW intensity and Heat Fluxes analysis during concurrent stages”.

i. Lines 484-486: the sentence “As MHWs have different durations, and on each grid point, concurrency may start and lasts for different times, and thereafter. it is difficult to compare different full concurrent MHW events.” would read more clearly if “and thereafter” was removed. The word “lasts” should be replaced by “last”.

Thank you for the comment, we applied these changes.

j. Paragraph 494-499: please consider adding a reference to Figure 9 within this paragraph to reinforce the connection between the discussion and the visual representation. Alternatively, you could merge this paragraph with the preceding one.

Thank you for the comment, we added a reference to Figure 9.

k. Line 507: “detail” should be “detailed”.

Thanks for the comment, we fixed it.

Reviewer #1 (Remarks to the Author):

The authors have made an effort to address the previous comments, which have resolved many of my concerns. There are still areas that need clarification before the manuscript can be considered for publication in the journal.

We want to thank the reviewer for the positive comment. We hope that the actual manuscript version will fully address all the clarifications needed.

1. The title of the manuscript is "Concurrent atmospheric heatwave drives marine heatwave intensification through air-sea heat flux change". However, this point is barely mentioned in the abstract, except in a phrase, "heat flux disruption". Instead, the focus is on the sharp increase of latent heat "at concurrence end". These should be better clarified.

We appreciate your comment, we have modified the abstract accordingly (lines 35-37):

“Using ERA5 reanalysis data (1940–2022), here we analyse heat flux variations **and the marine heatwave intensification** during concurrent atmospheric and marine heatwaves in the Mediterranean, a climate change hotspot.”

2. Line 146-147: "predictability" is low?

We have changed this sentence to

A complete understanding of heat flux performance on MHW-AHW concurrence is crucial for MHW forecasting, which still faces many challenges, especially in the Mediterranean

In the paper of Jacox et al (2022), they state that forecast skill is dependent on the region, the forecast lead time and study area. Regarding regional dependence they say “*the degree of forecast skill is highly dependent on region*” while for lead time “*As forecast lead time increases, the global pattern of forecast skill is retained, but forecast skill degrades*”. In the case of the Mediterranean sea they claim that “*In regions with MHWs driven by rapid atmospheric or oceanic fluctuations, such as the Mediterranean Sea and western boundary currents like the Gulf Stream, skillful forecast lead times are often limited to two months or less (Fig. 2b) and even intense MHWs are unpredictable at longer lead times*”.

De Boissésou et al (2024) also confirm that “*MHW prediction skill is very much area dependent*” and that their “*low skill in capturing major events in the Mediterranean*” agrees with previous studies of Jacox (2022) and McAdam (2023).

We have also added a new reference (McAdam et al. 2024) that outlines the difficulties of MHW forecasting in the Mediterranean, although promising skills were obtained.

McAdam, R., Bonino, G., Clementi, E. & Masina, S. Forecasting the Mediterranean Sea marine heatwave of summer 2022. *State of the Planet* **4-osr8**, 1–10 (2024).

3. line 153-155: This sentence reads awkwardly.

We appreciate your comment, we have rephrased it: “The main objective is to describe the changes in heat fluxes during concurrence, focusing on the onset and end stages. These stages represent the transitions between different air-sea heat exchange regimes that result from the absence or presence of the AHW.”.

4. line 162-163: Need a brief introduction of the method. The figure implies that MHW is longer than AMW, which needs to be stated. Or you have only looked at those cases.

The methodology is described in detail in the appropriate section. As it is quite complex we understand that briefly summarising could lead to misunderstandings, so we prefer to direct the reader to the detailed description.

We have looked at all the cases, and the duration of MHWs is almost always longer than that of AHWs in our study period and area. The present analysis does not aim to characterise concurrent AHW and MHW –this has been done in the previous article by the authors that is cited in the Methods section.

5. Line 239-240: I think there needs to be some explanation of why this is the case. There should be a delayed effect of air-sea heat flux anomalies on SST. This is the key point highlighted in the title.

We agree with the reviewer on the importance of this analysis but, at this stage, is out of the scope of this work. Our main objective, stated in the introduction section, was to “describe the changes in heat fluxes during concurrence”. A further analysis on the physical processes involved will be part of our future work in the research line on atmospheric and marine extremes concurrence and feedback.

Reviewer #2 (Remarks to the Author):

I have a few minor comments for the authors that could be further addressed before publication:

We want to thank the reviewer for the positive comment. We hope that the actual manuscript version will fully address all the clarifications needed.

1. Regarding my previous comment “Line 424: The reference of the WMO that justifies using the reference period 1961-1990 should be mentioned in the references list.”, I think the citation is not mentioned in the references list, although it is justified in the text. A comma “,” is missing between words “organisation” and ”for analysis” in line 443.

Done, we appreciate your comment.

2. Fig. 8 is missing latitude and longitude identification (eg:N,E).

Thank you, we have included it.

3. Line 548, "selected interval" should be "interest interval".

Done, thank you for noticing.

4. Figs. 1 and 3 of the results section are missing the b) point.

Fixed, thank you.

5. Line 555, "student t distribution" should be "student's t-distribution".

Thank you for your comment, we have fixed it.

6. Lines 317-321: Please add to the legend of Figure 6 that the analysis is focused solely on T1.

We appreciate your comment, we have rewrite the legend of Figure 6 accordingly.

7. Line 150, "unknown" instead of "runknown".

Thank you, we have fixed it.

8. Lines 424-434, "an MHW" should be "a MHW".

Thank you for noticing, we have corrected that.

9. Line 440, "a AHW" should be "an AHW".

Fixed. Thank you.

10. Line 193 , "no-MHW" should probably be "non-MHW days" for consistency.

Thank you, we have fixed it.