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Peer Review File

Mass flowering of the seagrass Posidonia oceanica after 2022 record-breaking marine heatwaves: a Pan-Mediterranean study

Corresponding Author: Dr Patrick Astruch

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

Version 0:

Decision Letter:

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Dear Mr Astruch,

Please accept our apologies for the delay in sending a decision on your manuscript. Your manuscript titled "Mass flowering of the seagrass Posidonia oceanica after 2022 record-breaking marine heatwaves: a Pan-Mediterranean study" has now been seen by 3 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.

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

Please submit your point-by-point responses as a separate file, distinct from your cover letter where you can add responses to the Editors' comments that you do not want to be made available to the reviewers. Word files are preferred. We recommend that any figures, tables or graphs that are included in the response to reviewers are also included in the main article or Supplementary Information.

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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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Editorial Board Member

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REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

This multi-authored manuscript presents data on flowering in the seagrass Posidonia oceanica in the year following the summer 2022 marine heatwave affecting water temperatures of the Mediterranean Sea. Summer sea surface temperatures were more than 2°C above the 1987-2021 average, while the cumulative heat gain for the summer exceeded 200 degree-days above average. Flowering in Posidonia is apparently a rare event, so the observation of flowering across hundreds of Mediterranean sites was stunning, including both high prevalence (fraction of sites where flowering was observed) and intensity (fraction of shoots with flowers at a site). Areas with more anomalously warm water exhibited more flowering, and the relationship was similar for flowering after the 2022 heat wave and for data from fewer sites after the 2003 heat wave. The scope of the data collection is impressive, the event was essentially unprecedented, and the analyses are compelling in linking thermal conditions to flowering by capitalizing on regional variation.

Line 166: Add a sentence or two addressing the timing of this data collection. For instance, all sites were visited between Sep 2022 and Jan 2023 [or whatever], which spans typical flowering initiation in P. oceanica and the x-month developmental period from flowers to fruits.
Overall I really like the figures.
Figure 1B. It is not immediately clear why average daily summer anomaly was selected for this graph, while all other visualizations emphasize heat accumulation. From the methods, only MHW-icum was used as a predictor. In lines 176-179, you could be more explicit. MHW-icum was positively related to summer SST and positively related to SST-anomaly, while SST anomaly was negatively related to SST, highlighting that the highest anomalies occurred in cooler regions. Statistically, flowering was examined in relation to MHW-icum [and duration? That information on line 225 came as a surprise] because many floral life-history transitions depend on degree-days.
Figure 4A. Consider swapping the x and y axes. I think it would be easier to interpret cool-warm (normally) sites on the x-axis, with the heat wave index on the y-axis, especially since it's the heat wave index that is related to flowering (higher on y-axis generates higher flowering).
Line 261 – why does the positive relationship to SST-JJA reinforce the role of thermal anomalies? Is this because the region factor has accounted for the existence of cooler and warmer regions within the Mediterranean?
Line 741: I was unfamiliar with Giraud's classification, and the phrasing of these sentences didn't help explain it. After looking at the supplemental material, I see that Giraud classified density of flowers. Is the point of this part of the methods to explain how flowering intensity was determined at all the sites, even those where FI was not collected directly but was inferred from Giraud's index of flower density? If so, this could be written more directly.
Line 790 following – Can you add a phrase indicating why a Bayesian approach was selected here, and also what the parameters are in the equations for beta0 (i.e. 3, 0, 10) and beta1 (i.e. 3, 0, 2.5)?
Line 818 – I really appreciated the structure of this final model that incorporated a factor for region, in addition to the heat index (MHW-icum). But in Fig. 4D it would be helpful to have language that addresses whether the regional coefficients should be interpreted as the strength of the relationship (ie flowering intensity relationship to MHW-icum) within the region, or interpreted for the regional average – that is, it is a within-region or across-region coefficient? I should probably be able to figure it out from model structure, but why not be specific.

Reviewer #2 (Remarks to the Author):

The authors present a very interesting collaborative study in the Mediterranean area that shows a possible relationship between temperature and flowering frequency in the emblematic species Posidonia oceanica following an anomalous warming event that occurred in 2022. The study deserves to be published; however, there are several aspects that should be clarified. One of the main issues the authors need to address concerns certain assumptions or threshold values used throughout the article, whose justification is not clearly explained in the manuscript. For example, the choice of the 0.07 threshold to differentiate between low and high flowering intensity is not explained, nor is the selection of 180 degree-days. Similarly, the manuscript repeatedly refers to the 2022 event as an "unprecedented flowering," but it is not clear what the authors mean by this term or on what basis they classify it as such. Do the authors have collaborative data from previous years that could demonstrate that the 2022 flowering was exceptional or massive? In this collaborative work, many areas are shown where flowering did not occur or where intensity was very low, which suggests that any comparison intended to demonstrate that this flowering was unprecedented should be conducted using data with an equivalent level of spatial detail. Moreover, authors unnecessarily over-complicate the analysis and its presentation. Most of the results could likely be shown using Figures 2, 3A, and 5. Everything else, including the Bayesian analysis, appears unnecessary and largely redundant. Furthermore, the manuscript attempts to establish an association between these flowering events and a potential negative effect on different populations; however, no quantitative evidence is provided to support this link. It may be advisable to temper this assumption throughout the manuscript until, hopefully, in the coming years, the collaborative network established here can provide evidence of such negative effects.

Below are some some speficics questions

Line 136 "… a foundational species …"
Line 137-138: Two consecutive sentences starting by "Yet, … . However, …", is grammatically far from ideal.
Lines 138 to 140. "However, flowering is energetically costly, with P. oceanica 139 needing to store starch and nutrients in its rhizomes over two to three years before undergoing 140 flowering [13, 14]." I read carefully both articles and I was unable to find how they support such statement.
Lines 142 to 144. "A greater investment in sexual reproduction may shift energy from other vital processes such as shoot growth, clonal propagation, or even seed maturation and development;" Can authors provide any reference to support such statement?
Line 150: "Given its crucial ecological roles, P. oceanica is protected under the EU Habitats Directive (92/43/EEC) and the

Barcelona Convention."
Usually, the last sentence in a paragraph states its conclusion. In this case, I do not see how the sentence above concludes this paragraph, centred around the dynamics of flowering in seagrasses. I also do not see how this sentence adds to the Introduction, or to the article.
Line 169: Please describe how the east-west and north-south gradients behave. Does the SST annual amplitude increase or decrease from east to west and from north to south?
Line 176-179: The figure showing this should be embedded in the body of the text, and not as Supplementary Material.
Line 182: A "flowering occurrence" is any site for which at least one flowering was observed? This is likely explained in the Methods. However, given that in Nature journals the Methods section comes in the end, in the former sections is preferable to use language as clear as possible. In this case, this can be exemplified by
"Following the summer of 2022, flowering occurred in 333 out of the 442 sites monitored, corresponding to a Flowering Prevalence (FP) of 75%"

Figure 1: Panel A is little informative given the very unbalanced number of records among ecoregions. Furthermore, the fact that the highest FI were (coincidently?) mostly observed in the ecoregions subject to the highest sampling effort, may lead the readers to question about the existence and magnitude of a sampling bias.

Panel B: How is it possible that for the Tunisian Plateau and Levantine Sea the average FI is known but the FP is unknown? "no data for Tunisian Plateau and Levantine Sea". No data or FP=0%? FP being 0% is different from being unknown.

Overall, I do not find this figure informative. I wonder whether it wouldn't be more informative a graph of SSTA-JJA vs FI (with the 442 sites), and/or a graph of SSTA-JJA vs FP (with the 9 ecoregions). It can also be tried with MHW-icum instead of SSTA-JJA.

Figure 2: What relevant information do the photographs show? If they are really necessary, shouldn't they be in a separate Figure?

Line 218. I found no sense for the categorical categories for meadow structure (lines 694-697). Which are the ecological meaning of such categories? In addition, probably Depth can be used as a continuos variable instead of a categorical ones.

Lines 225-226: "MHW-icum and marine heatwave duration (MHW-dur) were significantly correlated with FI ($R = 0.523$ and $R = 0.449$, respectively; $p < 0.001$) (Fig. S3)."
This is, in my perspective, the most interesting and relevant information in this submission. I find very inadequate that the respective figure is only supplied as supplementary material, instead of being the most fundamental figure embedded in the body of the text.

Lines 233-241 and Fig.3B: On what grounds was FI=0.07 chosen as a threshold? Furthermore, it is not true that "All records exhibiting FI > 0.07 … were associated with elevated MHW-icum". There was at least one high FI associated with one of the lowest observed MHW-icum.
This text and Fig.3B seem poorly grounded, ineffective and unnecessary. In fact, a graph showing the FI vs MHW-icum with the 442 sites would be much clearer and should suffice.

Lines 248-262 and Figure 4: I do not see that this part of the text adds relevant information, whereas it may add some noise. The statistical significances of the results are not shown.
In panel B, the alternating positive and negative slopes (4 of each) suggests that SST-JJA was not a significant factor for FI, and does not add to the effect of MHW-icum.
Furthermore, all this seems quite antagonistic with the former section 2.1 "Thermal regimes and warming trends in the Mediterranean" \ lines 177-179, saying that "SSTA-JJA and SST-JJA were negatively correlated (Spearman test: $R = -0.856$; $p < 0.001$), highlighting that the highest anomalies occurred in colder regions (e.g., Liguro-Provençal).".
How is it possible that the average anomaly (SSTA-JJA) is negatively correlated with the average temperature (SST-JJA), whereas the cumulative anomaly (MHW-icum) is positively correlated with the average temperature (SST-JJA), and "the most extreme flowering events may arise from the combined influence of high SST-JJA and elevated MHW-icum." (lines 257-258)?

Fig.5 and related text: In my opinion, this is the better achieved analysis, text and figure, and should work as the fundamental basis for this article.
Nevertheless, I still do not find the grounds for the 180 degree.days and 0.07 FI as thresholds.

Furthermore, in lines 285-287 the authors mention "Consistent with our earlier analysis, flowering occurrences in 2003 were more likely when MHW-icum exceeded approximately 100 °Cdays [101 ± 34 286 °Cdays] (Fig. 5B)."

First, are the authors sure they mean 100 °Cdays? I really do not see this in Fig.5B.

Second, a 100 °Cdays threshold for the 2003 data is very different from a 180 °Cdays threshold for the 2022 data. I do not see any consistency here.

I suggest that the authors try log-transform the data, and fit a regression line. In case there are zeros in the FI, do not log(x+1) transform as this introduces a severe bias, particularly in linear regressions (O'Hara and Kotze 2010, Xiao Xiao et al 2011, St Pierre et al 2018, Chen and Roth 2023,). Any log(x+C) transform introduces bias, which is greater the largest the C relative to x. In the present case 1 is always larger than FI, rendering the bias huge. An alternative is applying the logit transform y=exp(x)/(1+exp(x)), which works perfect for 0<x<1, leading y to range between 0.5 and 0.73. Reverse calculation is given by x=log(y/(1-y)) (See Vieira et al., 2018 – Subsection: composite survival rates).

Reviewer #3 (Remarks to the Author):

This MS reports a striking—and worrisome—large-scale ecosystem response to climate with potentially major consequences for the marine ecosystems of the Mediterranean Sea. The mass flowering of the seagrass Posidonia following a marine heat wave is an unmistakable signal of stress from climate warming that has reached levels likely to transform the entire regional ecosystem in coming years. The coordination of researchers sampling 442 sites across the region and the resulting dataset are impressive, and the data are generally convincing. This is a story that needs widespread attention.

I have a few suggestions for improving the MS:

First, the analysis and presentation. The general and important result of mass flowering across the region following a heat wave is clear in the data. But the paper discusses several thresholds that are not justified in the text. For example:

Line 230: "flowering (n = 333) occurred preferentially under the warmest conditions (Fig. 3A), with a probability of flowering higher than 50% when MHW-icum exceeds 120°C days." First, there are no labels on the vertical axis of the figure so it is impossible to know whether or when flowering occurrence exceeded 50%. Second, these numbers are presented without context or justification. What is the significance of the 50% threshold for flowering, rather than, for example, 10% or 80%? Is this a standard convention? And why 120 degree days? To my eye the transition in figure 3A appears closer to 160 days, when the occurrence of flowering abruptly increases. Did these threshold numbers come from a quantitative analysis? if so, this should be explained. If not, how were they chosen?

Line 805: "The threshold of 0.07 of FI was defined according to the distribution of our dataset and considering historical value range from Diaz-Almela et al. [12]." This number is not intuitive and this sentence is not very informative. Did the cited paper find the threshold of 0.07? Apparently not, since I searched the Diaz-Almela reference for "0.07" and did not find it. What sort of analysis of these data produced the result of 0.07? What is the confidence in that number? Since space is less restricted for the online methods (yes?) there should be plenty of room to explain here. These thresholds are clearly central to the narrative of this MS so it is critical to explain and justify them.

My second concern involves the writing. The text is very dense, filled with abbreviations that are cumbersome, often unnecessary, and taxing on the reader. Flowering Intensity is abbreviated as FI throughout, which is not necessary. To avoid confusion I recommend defining flowering intensity and prevalence in the abstract when they are first used and simply using the words. Also I feel confident that this group of authors can find a better label than MHW-icum, an opaque and previously unknown term (I googled it) with 8 characters, almost as long as a full, more descriptive word that would be much more digestible by readers. I recommend finding such a word and simply defining it clearly at the beginning. Similarly, rather than using SST-JJA can you simply define this and then call it "summer temperature"? Section headers especially should avoid cumbersome abbreviations (for example, 2.3: "Relationships between MHW descriptors and MFE"). Readers use section headers to navigate around a MS, so their value is primarily for those who have not yet read the paper carefully. Do them (and yourselves) a favor and make the headers clear. Similarly, there are paragraphs full of parenthetical numbers (e.g., lines 190-199) that would be better presented in tables or figures. The text should provide a concise but rigorous narrative about what happened in as plain language as possible, referencing the tables and figures for the details.

Other comments:

Line 127: mass mortality of what?

Line 153: "We assessed its spatial distribution, characterized summer thermal conditions using high-resolution satellite-derived SST data, and examined potential links with both biotic (e.g., meadow structure) and abiotic (e.g., depth, protection status, temperature) factors." No results are presented regarding effects of protection status so why is it described here (and in online methods)?

Line 157: Flowering intensity has now been referred to at least twice without a definition.

Figure 1B: Why is there no symbol for flowering prevalence for Tunisia?

Figure 3A: Is flowering occurrence the same as prevalence? Terms should be harmonized between figure and text.

Figure 4a is puzzling and difficult to interpret. I would leave it out.

Figure S3: it would be helpful to show the curves and confidence limits in the figures with the points since it is often not at all clear where the correlations come from. For example, FI versus latitude shows a correlation of .235 with three asterisks, yet the scatterplot does not appear to show any relationship. Probably this is because the data are highly abnormally distributed. Whatever the case it would be good to see the fitted curve and confidence intervals.

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Version 1:

Decision Letter:

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Dear Mr Astruch,

Your revised manuscript titled "Mass flowering of the seagrass Posidonia oceanica after 2022 record-breaking marine heatwaves: a Pan-Mediterranean study" has now been seen by 2 reviewers, whose comments are appended below. You will see that they find your work of ongoing potential interest. However, they have raised substantial concerns that must be addressed. In light of these comments, extensive revisions will be required before we can further consider the manuscript for publication. We would, however, be interested in considering a revised version that fully addresses these serious concerns.

In particular, we require that you:
-rigorously demonstrate mass flowering of the seagrass Posidonia oceanica after 2022 record-breaking marine heatwaves,
-fully justify the choice of 0.07 PI and 180ºCdays thresholds in your analysis.

We hope you will find the reviewers' comments useful as you decide how to proceed. If additional work allows you to either incorporate or refute these criticisms, we will be happy to look at a substantially revised manuscript. If you choose to take up this option, please either highlight all changes in the manuscript text file, or provide a list of the changes to the manuscript with your responses to the reviewers.

When resubmitting, please provide a point-by-point response to the reviewers' comments. Please submit your responses as a separate file, distinct from your cover letter where you can add responses to the Editors' comments that you do not want to be made available to the reviewers. Word files are preferred. We recommend that any figures, tables or graphs that are included in the response to reviewers are also included in the main article or Supplementary Information.

Please bear in mind that we will be reluctant to approach the reviewers again in the absence of substantial revisions.

If the revision process takes significantly longer than three months, we will 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.

We are committed to providing a fair and constructive peer-review process. Please do not hesitate to contact us if you wish to discuss the revision in more detail.

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Please do not hesitate to contact us if you have any questions or would like to discuss the required revisions further. Thank you for the opportunity to review your work.

Best regards,

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REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

I read the response to review and the revised manuscript and feel that the authors have addressed my review comments successfully.

Reviewer #2 (Remarks to the Author):

I would like to thank the authors for submitting the revised manuscript and for the improvements made to the text. However, I still have several questions and concerns that, in my opinion, were not adequately addressed.
All reviewers consistently requested clarification of the thresholds used in the manuscript, namely FI = 0.07 and 180 degree-days. This clearly indicates that this crucial aspect of the study was not sufficiently explained in the original version. In the revised manuscript, the authors attempt to justify the selected thresholds and include new figures. Nevertheless, the choice of a 7% FI threshold still appears largely arbitrary. The K-means and threshold analysis shown in Figure S7 suggests that a 6% threshold might be more appropriate, which would substantially lower the proposed MHWicum threshold from the currently suggested 180 °C·days. Conversely, if the authors had selected the 10% threshold mentioned in their response, the resulting degree-day threshold—derived using their own method (Supplementary Figure S3 and their reply)—would be considerably higher than 180 °C·days. Further highlighting the inconsistency of proposing thresholds for Marine Heat Wave (MHW) impacts on seagrass flowering, there is a striking discrepancy between the 180 °C·days threshold obtained when MHWicum is compared with FI and the 120 °C·days threshold obtained when it is compared with FP.
Several of the co-authors previously published a study on the effects of MHWs on the same species (Posidonia oceanica) and within part of the same geographical domain (Eastern Atlantic; García-Escudero et al., 2024). In that study, no thresholds were proposed. Instead, the authors analyzed the effect of the same MHWicum predictor on the same FI response using a GAMM, which yielded a linear relationship. This is consistent with the suggestion already raised during the first version of the manuscript. Their results clearly indicate a progressive, rather than abrupt, response to MHWs, in contrast to the threshold-based response now proposed. In that study, MHWs led to above-average FI values from approximately 30 °C·days onward, which strongly contrasts with the 180 °C·days threshold proposed here. Moreover, despite the observed increase in FI, values were almost always below the 7% threshold now proposed, with a mean FI of

2.52%. Only two observations exceeded 7%, reaching 8.47% and 8.86%, as reported in their supplementary Excel file. Based on these considerations, I find that the thresholds currently proposed are not sufficiently supported and may be misleading. Instead, the authors should focus on a progressive response to MHWs, as demonstrated in previous work by some of the same co-authors (García-Escudero et al., 2024).
I also believe that the term “unprecedented flowering” should be removed or replaced, as the authors do not demonstrate that the 2022 event was truly unprecedented. Notably, the authors overlook their own previous study (André et al., 2023), which examined the effects of the same 2022 MHW on the same species and within part of the same geographical domain (French Mediterranean coast). That study explicitly addressed whether the observed mass flowering was unprecedented and concluded that it was not, directly contradicting the current claim. Furthermore, in that earlier work (page 91), the authors report multiple historical mass flowering events of Posidonia oceanica in 1961, 1963, 1970, 1971, 1974, 1975, 1982, 1983, 1985, 1994, and 2003. Given this historical context, it is unclear how the 2022 event can be described as unprecedented when similar events have occurred on more than ten previous occasions.
The manuscript would be substantially strengthened if these historical data were incorporated into the analysis, allowing the 2022 event to be placed within a broader temporal and ecological context.
Finally, the authors rejected the suggestion to transform the data, arguing that it would complicate interpretation and provide no added value. I disagree with both points. Regarding interpretation, the authors could easily present dual axes, with the original values displayed alongside (and parallel to) the transformed scale. Concerning added value, the current approach discards all observations with zero flowering intensity (lines 879–882): “Finally, we developed a model to track flowering intensity continuously, using a log-normal distribution to account for the non-negative and skewed nature of the flowering intensity data (Eq. 7). The dataset was subset to include only the observations where flowering occurred following summer 2022, and any values with no flowering observations were removed.” This procedure clearly introduces a systematic bias. By applying the suggested logit transformation, all observations—including zeros—could be retained, thereby avoiding this bias.

André S, Astruch P, Boudouresque CF, et al (2023). The 2022 mass flowering of Posidonia oceanica in the French Mediterranean Sea: is it unprecedented? Scientific Reports of Port-Cros National Park, 37: 65-100.

García-Escudero CA, Litsi-Mizan V, Efthymiadis PT, Gerakaris V, Serrano O, Apostolaki ET (2024). Strong marine heatwaves trigger flowering in seagrass. Limnology and Oceanography, 69(7): 1494-1507.

Version 2:

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Your manuscript titled "Mass flowering of the seagrass Posidonia oceanica after 2022 record-breaking marine heatwaves: a Pan-Mediterranean study" has now been seen by the editorial team. We are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment.

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December, 20th 2025

REPLY TO REVIEWERS

Communications Earth & Environment

Revision version manuscript: COMMSENV-25-4036-T

REVIEWER COMMENTS: in bold and italics, line numbers refer to original submission

OUR RESPONSES line numbers refer to revised word doc or pdf without visible track changes.

Reviewers' Comments:

Reviewer #1 (Remarks to the Author):

This multi-authored manuscript presents data on flowering in the seagrass Posidonia oceanica in the year following the summer 2022 marine heatwave affecting water temperatures of the Mediterranean Sea. Summer sea surface temperatures were more than 2°C above the 1987-2021 average, while the cumulative heat gain for the summer exceeded 200 degree-days above average. Flowering in Posidonia is apparently a rare event, so the observation of flowering across hundreds of Mediterranean sites was stunning, including both high prevalence (fraction of sites where flowering was observed) and intensity (fraction of shoots with flowers at a site). Areas with more anomalously warm water exhibited more flowering, and the relationship was similar for flowering after the 2022 heat wave and for data from fewer sites after the 2003 heat wave. The scope of the data collection is impressive, the event was essentially unprecedented, and the analyses are compelling in linking thermal conditions to flowering by capitalizing on regional variation.

Response: We thank Reviewer 1 for his/her positive comments

-Line 166: Add a sentence or two addressing the timing of this data collection. For instance, all sites were visited between Sep 2022 and Jan 2023 [or whatever], which spans typical flowering initiation in **P. oceanica** ***and the x-month developmental period from flowers to fruits).***

Response: The timing was specified in section 4.1.1 of the online methods. We have now included a sentence regarding the timing of data collection and the stages of plant development.

Lines 723-724: "Compiled data included qualitative, semi-quantitative and quantitative estimates of flowering obtained between September 2022 and July 2023, spanning the entire sequence from inflorescence formation to fruit dispersal."

- Overall I really like the figures.

<u>Response:</u> We thank the reviewer for this comment. Much appreciated.

- Figure 1B. It is not immediately clear why average daily summer anomaly was selected for this graph, while all other visualizations emphasize heat accumulation. From the methods, only MHW-icum was used as a predictor.

<u>Response:</u> We appreciate the reviewer's comment. In the original manuscript, we chose to display the summer sea surface temperature anomaly (SSTA-JJA) in Figure 1B (now Fig. 2B) because it is strongly correlated with MHW-icum and provides a broader view of seasonal thermal conditions. However, we agree that using MHW-icum would be more consistent with the predictors used in the analyses and with other heat-accumulation visualizations.

Therefore, in the new version of the manuscript, we have prepared a revised version of Figure 2B in which SSTA-JJA has been replaced with MHW-icum. To further improve clarity, we have also adjusted the orientation of the x- and y-axes in both panels A and B. We believe these changes enhance the coherence and readability of the figure.

- In lines 176-179, you could be more explicit. MHW-icum was positively related to summer SST and positively related to SST-anomaly, while SST anomaly was negatively related to SST, highlighting that the highest anomalies occurred in cooler regions.

<u>Response:</u> We have modified the sentence according to the reviewer's suggestion. The sentence is now in lines 177-181.

Lines 177-181: "MHW cumulative intensity (MHW-icum) was positively related to summer SST in June-July-August (JJA) and positively related to SSTA-JJA (Spearman test: $p < 0.001$; Fig. 1), while SSTA-JJA was negatively related to SST-JJA (Spearman test: $R = -0.856$; $p < 0.001$), highlighting that the highest anomalies occurred in colder regions (e.g., Liguro-Provençal)."

- Statistically, flowering was examined in relation to MHW-icum [and duration? That information on line 225 came as a surprise] because many floral life-history transitions depend on degree-days.

<u>Response:</u> We acknowledge the reviewer's point. We referred to MHW duration because it is strongly correlated with MHW-icum, as was originally shown in Figure S3 (now moved to Fig 1), and both metrics are presented in the Online Methods. We understand that its appearance in the Results section may have seemed unexpected, likely because the Methods appear after the Results in the online format. To address this, we have moved the previous supplementary figure Fig S3 to become Fig. 1 in the body text, which illustrates all thermal descriptors.

- Figure 4A. Consider swapping the x and y axes. I think it would be easier to interpret cool-warm (normally) sites on the x-axis, with the heat wave index on the y-axis, especially since it's the heat wave index that is related to flowering (higher on y-axis generates higher flowering).

Response: We have modified Figure 4A (now Fig. 5A) following the reviewer's comments.

- Line 261 – why does the positive relationship to SST-JJA reinforce the role of thermal anomalies? Is this because the region factor has accounted for the existence of cooler and warmer regions within the Mediterranean?

Response: Indeed, as the reviewer indicates, Mediterranean ecoregions exhibit differences in their long-term thermal regimes. By accounting for region as a factor, the model controls for these baseline differences. The positive relationship with SST-JJA therefore reinforces the role of anomalously warm summer conditions, as opposed to simply warmer regions, on flowering intensity. The Fig. S1 may provide some additional insights. We have clarified this point in the revised ms.

Lines 177-181: "MHW cumulative intensity (MHW-icum) was positively related to summer SST in June-July-August (JJA) and positively related to SSTA-JJA (Spearman test: $p < 0.001$; Fig. 1), while SSTA-JJA was negatively related to SST-JJA (Spearman test: $R = -0.856$; $p < 0.001$), highlighting that the highest anomalies occurred in colder regions (e.g., Liguro-Provençal)."

- Line 741: I was unfamiliar with Giraud's classification, and the phrasing of these sentences didn't help explain it. After looking at the supplemental material, I see that Giraud classified density of flowers. Is the point of this part of the methods to explain how flowering intensity was determined at all the sites, even those where FI was not collected directly but was inferred from Giraud's index of flower density? If so, this could be written more directly.

Response: Thank you for this comment. Giraud's classification originates from an earlier study that quantified flowering based solely on inflorescence density, at a time when flowering events were very sparse. In our study, we aimed to account for both inflorescence density and shoot density, making the Flowering Intensity (FI) metric more representative of actual flowering at each site. The strong correlation between Giraud's index and FI metric confirms the consistency of the two approaches, while justifying our choice to use FI as the primary descriptor. We have revised the Methods section to explain this rationale more directly.

Lines 763-765: "As Giraud's classification [61] and FI level are strongly correlated (Spearman test: $R = 0.965$, p-value < 0.001), FI level, more representative of actual flowering at each site, was used to display the results,".

- Line 790 following – Can you add a phrase indicating why a Bayesian approach was selected here, and also what the parameters are in the equations for beta0 (i.e. 3, 0, 10) and beta1 (i.e. 3, 0, 2.5)?

Response: We have clarified this in the revised text.

Lines 818-826: Here, β_0 is the intercept and β_1 is the coefficient for MHW-icum. Both parameters were assigned Student-t priors, specified by three values: degrees of freedom (3), location (mean, 0), and scale (spread; 10 for β_0, 2.5 for β_1). These weakly informative priors allow the data to primarily drive the inference while limiting extreme values. We chose a Bayesian approach to fully propagate uncertainty in parameter estimates and to accommodate the hierarchical structure of the data. The model was implemented using the brms package [69] with four Markov chains of 4,000 iterations each (1,000 warm-up steps). Convergence was assessed via R-hat values (<1.01) [70] and trace plots. We set adapt_delta = 0.99 and max_treedepth = 15 to ensure efficient and reliable sampling.

- Line 818 – I really appreciated the structure of this final model that incorporated a factor for region, in addition to the heat index (MHW-icum). But in Fig. 4D it would be helpful to have language that addresses whether the regional coefficients should be interpreted as the strength of the relationship (ie flowering intensity relationship to MHW-icum) within the region, or interpreted for the regional average – that is, it is a within-region or across-region coefficient? I should probably be able to figure it out from model structure, but why not be specific.

Response: We thank the reviewer for the suggestion to clarify the interpretation of the regional coefficients in Fig. 4B-D (now Fig. 5B-D). As described in the Methods, the model for continuous flowering intensity includes both fixed effects (β_1 for MHW-icum and β_2 for SST-JJA) and hierarchical effects for ecoregions (ζ). The fixed effect for MHW-icum represents the overall average relationship between flowering intensity and cumulative heat exposure across all ecoregions. The ecoregion-level hierarchical coefficients capture deviations from this overall effect, indicating how strongly flowering intensity responds to MHW-icum within each ecoregion relative to the overall average. Therefore, the regional coefficients in Fig. 5B-D should be interpreted as within-region slopes rather than regional averages. For example, a positive ecoregion coefficient indicates that flowering intensity in that region, is more responsive to MHW-icum than the overall average (Fig 5D).

Lines 267-279: These findings suggest that the most extreme flowering events may arise from the combined influence of high SST-JJA and elevated MHW-icum. Region-specific modelling further showed that almost all intercepts were negative (Fig. 5B), indicating that baseline flowering was generally unlikely in the absence of strong thermal forcing, particularly in western regions. In contrast, the slope estimates for both SST-JJA and MHW-icum were predominantly positive (Figs. 5C and 5D), reinforcing the role of thermal anomalies in driving flowering. The regional slopes in Fig. 5B–D reflect variation in the strength of the flowering response to MHW-icum across ecoregions. For MHW-icum and the intercept, 95% credible intervals did not cross zero, making these coefficients interpretable. Credible intervals for SST-JJA were wider and often including zero, reflecting greater uncertainty and contrasting trends among regions. Positive regional slope values indicate that flowering in that region responds more strongly to thermal anomalies than the entire Mediterranean.

Reviewer #2 (Remarks to the Author):

The authors present a very interesting collaborative study in the Mediterranean area that shows a possible relationship between temperature and flowering frequency in the emblematic species Posidonia oceanica following an anomalous warming event that occurred in 2022. The study deserves to be published; however, there are several aspects that should be clarified. One of the main issues the authors need to address concerns certain assumptions or threshold values used throughout the article, whose justification is not clearly explained in the manuscript. For example, the choice of the 0.07 threshold to differentiate between low and high flowering intensity is not explained, nor is the selection of 180 degree-days.

<u>Response:</u> See the reply to the specific comment about the 0.07 threshold hereafter.

The selection of the MHW-icum threshold of 180°Cday was defined according to the results represented in Figures 3 and 5 (now Figures 4 and 6), showing an increasing in FI when MHW-icum reaches or surpasses this value, which we observe for both 2022 data and 2003. To clarify, we have also added in the supplementary material a new figure (now Fig S3), which clearly illustrates a 50% probability of high flowering (above 0.07) when surpassing 180°Cdays.

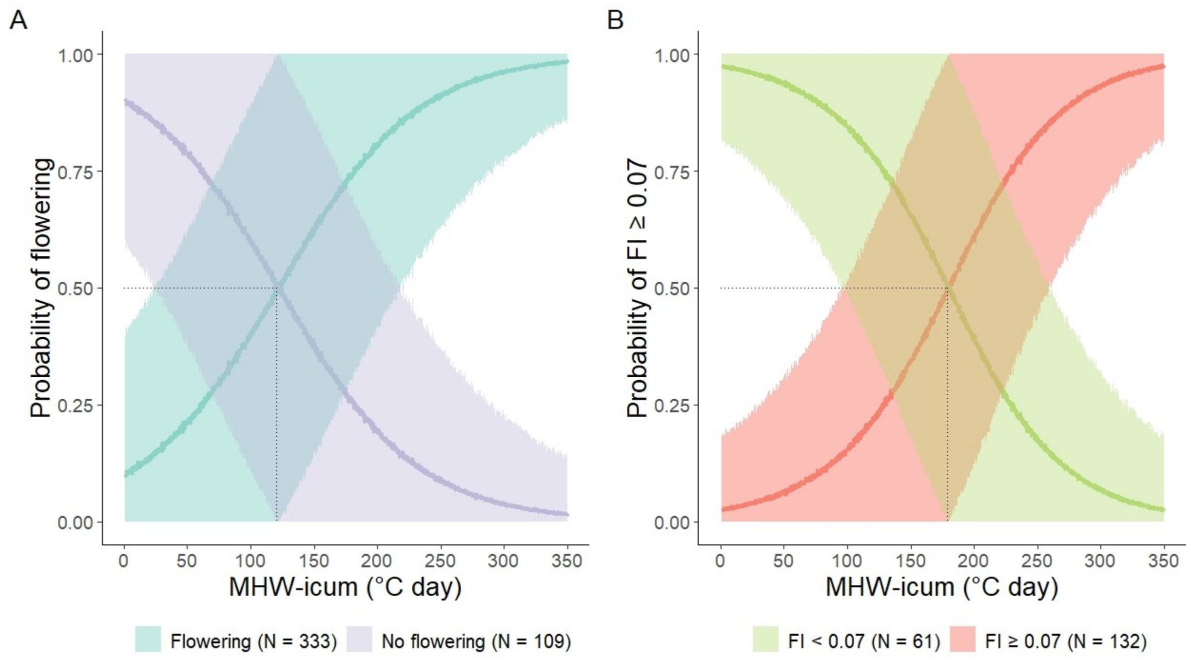


Fig. S3. Panel A: Probability of flowering according to marine heatwave cumulative intensity (MHW-icum in °Cday). Panel B: Probability of observing a flowering intensity over or equal to 0.07 according to MHW-icum.

- Similarly, the manuscript repeatedly refers to the 2022 event as an "unprecedented flowering," but it is not clear what the authors mean by this term or on what basis they classify it as such. Do the authors have collaborative data from previous years that could demonstrate that the 2022 flowering was exceptional or massive? In this collaborative work, many areas are shown where flowering did not occur or where intensity was very low, which

suggests that any comparison intended to demonstrate that this flowering was unprecedented should be conducted using data with an equivalent level of spatial detail.

Response: Unfortunately, such collaborative data collection like the one we did in the frame of this work did not happen in the past, and no previous works have encompassed the level of spatial detail that this work has.

Because of the sparsity and unevenness of *P. oceanica* flowering, seminal research on *P. oceanica* sexual reproduction (Molinier & Picard, 1952) had suggested that the species was in an evolutive dead-end. Diaz-Almela et al. (2007) did a remarkable literature review to collect flowering (and lack thereof) evidences through time, and they were able to detect exceptionally high flowering in 2003. The sparsity of the records and their intensity (when available) do not allow us to characterise without any doubt that the 2022 event was unprecedented, but given the data obtained in this work, and the lack of a similar phenomenon having been observed or reported since then, at least for the Western Mediterranean, we think it is reasonable to name this flowering event as "unprecedented" within the temporal scale of existing studies so far. It is also worth noting that the 2022 marine heatwaves were unprecedented in terms of cumulative intensity in the western Mediterranean. This work aims to pave the way toward standardised and long-term monitoring of *P. oceanica* flowering at Mediterranean scale in the future so we can set baselines that allow to further characterize the flowering phenomenon and the factors driving it.

- Moreover, authors unnecessarily over-complicate the analysis and its presentation. Most of the results could likely be shown using Figures 2, 3A, and 5. Everything else, including the Bayesian analysis, appears unnecessary and largely redundant.

Response: We respectfully note that the Bayesian analyses are an integral part of our study because they allow us to account for both the hierarchical structure of the data (variation among ecoregions) and the uncertainty in the estimated relationships between flowering intensity and thermal anomalies. While Figures 2, 3A, and 5 summarize general patterns, the Bayesian approach provides additional insight by quantifying the strength of these relationships within and across ecoregions, allowing us to capture regional deviations from the overall trends. This is particularly important for understanding how extreme flowering events emerge in specific regions under varying thermal conditions. We have therefore retained these analyses to ensure the results are both statistically robust and fully interpretable.

- Furthermore, the manuscript attempts to establish an association between these flowering events and a potential negative effect on different populations; however, no quantitative evidence is provided to support this link. It may be advisable to temper this assumption throughout the manuscript until, hopefully, in the coming years, the collaborative network established here can provide evidence of such negative effects.

Response: We collectively proposed a discussion, based on solid references, to display the main hypothesis in terms of sexual reproduction perspectives with regards to *P. oceanica* fate in a global change context. We have considered carefully both positive and negative potential consequences and parameters involved.

Please refer to lines 329-414 for the detailed discussion.

Below are some some questions

- Line 136 "… a foundational species …"

<u>Response:</u> Line 136: 'a' has been added

- Line 137-138: Two consecutive sentences starting by "Yet, … . However, …", is grammatically far from ideal.

<u>Response:</u> Lines 138-139: 'Yet' has been replaced by 'Nevertheless' and 'However' by 'Still'.

Lines 138 to 140. "However, flowering is energetically costly, with P. oceanica 139 needing to store starch and nutrients in its rhizomes over two to three years before undergoing 140 flowering [13, 14]." I read carefully both articles and I was unable to find how they support such statement.

<u>Response:</u> We list the references cited below

[13] Gobert et al. (2005) stated that flowering requires extra resources from the plant.

[14] Calvo et al. (2006) demonstrated that the rhizome growth is lower 2 years prior to flowering compare to shoots that did not flower (see page 365 of the article: https://onlinelibrary.wiley.com/doi/epdf/10.1111/j.1439-0485.2006.00132.x).

For more clarity, we propose to cite Gobert et al. (2005) after 'energetically costly [13] and Calvo et al. (2006) at the end of the sentence.

Line 139-141: "Still, flowering is energetically costly [13], with *P. oceanica* needing to store starch and nutrients in its rhizomes over two to three years before undergoing flowering [14]."

- Lines 142 to 144. "A greater investment in sexual reproduction may shift energy from other vital processes such as shoot growth, clonal propagation, or even seed maturation and development;" Can authors provide any reference to support such statement?

<u>Response:</u> We propose to cite again Gobert et al. (2005) and remove 'or even seed maturation and development'

Lines 143-145: "A greater investment in sexual reproduction may shift energy from other vital processes such as shoot growth, clonal propagation [13]"

- Line 150: "Given its crucial ecological roles, P. oceanica is protected under the EU Habitats Directive (92/43/EEC) and the Barcelona Convention." Usually, the last sentence in a paragraph states its conclusion. In this case, I do not see how the sentence above concludes

this paragraph, centred around the dynamics of flowering in seagrasses. I also do not see how this sentence adds to the Introduction, or to the article.

Response: The sentence has been removed.

- Line 169: Please describe how the east-west and north-south gradients behave. Does the SST annual amplitude increase or decrease from east to west and from north to south?

Response: Please refer to Fig S1 which displays SST pattern among ecoregions

- Line 176-179: The figure showing this should be embedded in the body of the text, and not as Supplementary Material.

Response: As we are limited in number of figures, we chose to move this figure in supplementary material. We also consider that this figure (Fig. S1) presents general features about thermal trends and patterns of Mediterranean ecoregions which are not specific results from our work.

- Line 182: A "flowering occurrence" is any site for which at least one flowering was observed? This is likely explained in the Methods. However, given that in Nature journals the Methods section comes in the end, in the former sections is preferable to use language as clear as possible. In this case, this can be exemplified by "Following the summer of 2022, flowering occurred in 333 out of the 442 sites monitored, corresponding to a Flowering Prevalence (FP) of 75%"

Response: Thank you for this suggestion. We agree and have inserted the suggested sentence instead of the previous one for more clarity.

Lines 194-196: "Following the summer of 2022, flowering occurred in 333 out of the 442 sites monitored, corresponding to a Flowering Prevalence (FP: number of records reporting flowering in relation to the total number of records) of 76%."

- Figure 1: Panel A is little informative given the very unbalanced number of records among ecoregions. Furthermore, the fact that the highest FI were (coincidently?) mostly observed in the ecoregions subject to the highest sampling effort, may lead the readers to question about the existence and magnitude of a sampling bias.

Response: Panel A of Fig 1 (now Fig. 2) presents FI level in each ecoregion. As the reviewer noticed, the ecoregion with the most records (i.e. 156 for Liguro-Provençal) exhibits high FI levels, but several other ecoregions with quite numerous records (respectively 25 and 59 for Adriatic Sea and Aegean Sea) exhibit lower FI level, tempering the unbalance of the records among ecoregions. Indeed, as this work is an opportunistic collaborative data collection, there is an unbalance.

- Panel B: How is it possible that for the Tunisian Plateau and Levantine Sea the average FI is known but the FP is unknown? "no data for Tunisian Plateau and Levantine Sea". No data or FP=0%? FP being 0% is different from being unknown.

<u>Response:</u> We did not present the flowering prevalence for ecoregions with less than 10 records (only the case for Tunisan Plateau and Levantine Sea).

See section 4.1.3. lines 758-759: "FP was calculated for each ecoregion that contained at least 10 records"

- Overall, I do not find this figure informative. I wonder whether it wouldn't be more informative a graph of SSTA-JJA vs FI (with the 442 sites), and/or a graph of SSTA-JJA vs FP (with the 9 ecoregions). It can also be tried with MHW-icum instead of SSTA-JJA.

<u>Response:</u> We proposed a revised version of the Fig1B (now Fig. 2B) with MHW-icum instead of SSTA-JJA.

- Figure 2: What relevant information does the photographs show? If they are really necessary, shouldn't they be in a separate Figure?

<u>Response:</u> We wanted to show some images of inflorescences in different regions within the manuscript without adding a figure (because of the limit in number of figures). There is already a supplementary figure (see Fig S2) with a set of inflorescence pictures. If the editor agrees, we would like to keep the Fig 2 (now Fig. 3) as it is.

- Line 218. I found no sense for the categorical categories for meadow structure (lines 694-697). Which are the ecological meaning of such categories?

<u>Response:</u> Data about the meadow structure (fragmentation and meadow cover) was not available for most of the sites. To analyse a potential effect of the meadow structure on the flowering intensity, we qualitatively defined these three categories based on the expert judgement of the authors. The threshold of 80% of meadow cover is considered as the lower limit for healthy *P. oceanica* meadow (see e.g., Personnic et al., 2014: doi:10.1371/journal.pone.0098994). Under 80% cover, we aimed to distinguish meadows fragmented naturally (e.g., hydrodynamism) or under human stressors.

- In addition, probably Depth can be used as a continuous variable instead of a categorical one.

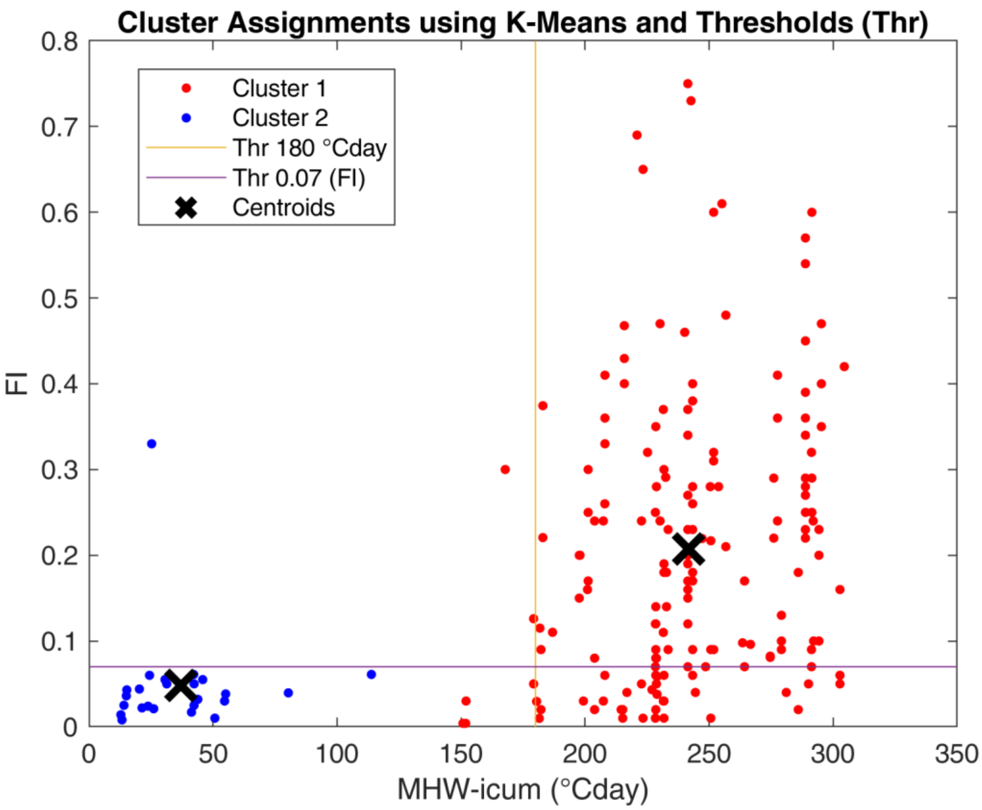
<u>Response:</u> With regards to the depth, we proposed to use depth categories because we did not have a good resolution for some sites. Considering this, we chose to constrain it to depth categories to ensure accuracy.

- Lines 225-226: "MHW-icum and marine heatwave duration (MHW-dur) were significantly correlated with FI (R = 0.523 and R = 0.449, respectively; p < 0.001) (Fig. S3)." This is, in my perspective, the most interesting and relevant information in this submission. I find very inadequate that the respective figure is only supplied as supplementary material, instead of being the most fundamental figure embedded in the body of the text.

Response: Thank you for this suggestion, indeed, Fig S3 is very informative but we had to select among our set of figures and we decided to move it to supplementary material file. Following your comment and the Editor agrees, we propose to add it in the manuscript as Fig 1.

- Lines 233-241 and Fig.3B: On what grounds was FI=0.07 chosen as a threshold?

Response: As also responded to Reviewer #3, the threshold of FI = 0.07 was chosen based on both historical records and the distribution of observed flowering intensity in our dataset. Historical data from Diaz-Almela et al. (2007) indicate that prior to 2003, flowering events with FI above 0.07 were very rare and typically occurred during large, synchronised events (e.g., 1975, 1982, 1985, 1993–1994). In Diaz-Almela et al. (2007) dataset, the distribution of FI values (see Fig. 5c in Diaz-Almela et al., 2007) shows that the most frequent values fall around 0.05, while higher values (≥0.1) correspond to relatively high flowering events. This threshold is also supported by clustering of the original quantitative data from our data set using K-Means as shown in the Figure below now added as Figure S7. By selecting a threshold of 0.07, we capture events that are clearly above baseline levels but not so restrictive as to exclude moderately elevated flowering, allowing a meaningful assessment of ecologically significant increases in flowering intensity. This threshold therefore reflects a balance between historical rarity and contemporary relevance.



Supplementary Figure 7: Cluster assignments of flowering intensity (FI) according to marine heatwaves cumulative intensity (MHW-icum) used to define the FI threshold (Thr) of 0.07 and corresponding MHW-icum threshold of 180°Cday.

Lines 828-831: The threshold of 0.07 of FI was defined according to the distribution of our dataset and considering historical value range from Diaz-Almela et al. [12]. This threshold is also supported by clustering of the original quantitative data from our data set using K-Means (Fig. S7).

- Furthermore, it is not true that "All records exhibiting FI > 0.07 … were associated with elevated MHW-icum". There was at least one high FI associated with one of the lowest observed MHW-icum.

<u>Response:</u> We replaced 'All' by 'Most of the'

Line 299-300: "Most of the records exhibiting FI > 0.07 (n = 132, mean = 0.38 ± 0.12) were associated with elevated MHW-icum…"

- This text and Fig.3B seem poorly grounded, ineffective and unnecessary. In fact, a graph showing the FI vs MHW-icum with the 442 sites would be much clearer and should suffice.

<u>Response:</u> We think that the panel B of Figure 3 (now Fig. 4) is complementary to panel A and informative to address a proper description of the flowering intensity according MHW-icum. The graph showing FI vs MHW-icum is displayed in the correlation matrix of Fig 1 (previously Fig. S3).

- Lines 248-262 and Figure 4: I do not see that this part of the text adds relevant information, whereas it may add some noise. The statistical significances of the results are not shown. In panel B, the alternating positive and negative slopes (4 of each) suggests that SST-JJA was not a significant factor for FI, and does not add to the effect of MHW-icum. Furthermore, all this seems quite antagonistic with the former section 2.1 "Thermal regimes and warming trends in the Mediterranean" \ lines 177-179, saying that "SSTA-JJA and SST-JJA were negatively correlated (Spearman test: R = -0.856; p < 0.001), highlighting that the highest anomalies occurred in colder regions (e.g., Liguro-Provençal)."

<u>Response:</u> We note that the analyses in Fig. 4 (now Fig. 5) are based on a Bayesian hierarchical model, where classical notions of statistical significance (p-values) are not applicable. Instead, we report posterior distributions and credible intervals, which allow us to quantify the strength and uncertainty of the effects of SST-JJA and MHW-icum on flowering intensity within and across regions. Figure 5 illustrates regional variation in these effects, showing how the response to thermal drivers differs among ecoregions. While some slopes are negative, this reflects regional deviations from the overall positive trend rather than an absence of effect. Including this figure provides a clearer understanding of spatial heterogeneity in the flowering response, which cannot be captured by simple summary statistics or pooled analyses. We have added a statement regarding the interpretations of our results in the text.

Lines 267-279: "These findings suggest that the most extreme flowering events may arise from the combined influence of high SST-JJA and elevated MHW-icum. Region-specific modelling further showed that almost all intercepts were negative (Fig. 5B), indicating that baseline flowering was generally unlikely in the absence of strong thermal forcing, particularly in western regions. In contrast, the slope estimates for both SST-JJA and MHW-icum were predominantly positive (Figs. 5C and 5D), reinforcing the role of thermal anomalies in driving flowering. The regional slopes in Fig. 5B–D reflect variation in the strength of the flowering response to MHW-icum across ecoregions. For MHW-icum and the intercept, 95% credible intervals did not cross zero, making these coefficients interpretable. Credible intervals for SST-JJA were wider and often including zero, reflecting greater uncertainty and contrasting trends among regions. Positive regional slope values indicate that flowering in that region responds more strongly to thermal anomalies than the entire Mediterranean."

- How is it possible that the average anomaly (SSTA-JJA) is negatively correlated with the average temperature (SST-JJA), whereas the cumulative anomaly (MHW-icum) is positively correlated with the average temperature (SST-JJA), and "the most extreme flowering events may arise from the combined influence of high SST-JJA and elevated MHW-icum." (lines 257-258)?

Response: The apparent discrepancy arises because SSTA-JJA, SST-JJA, and MHW-icum capture different aspects of thermal conditions. SSTA-JJA measures deviations from the local long-term mean, so the largest anomalies often occur in historically cooler regions, leading to a negative correlation with SST-JJA. In contrast, MHW-icum integrates the intensity and duration of marine heatwaves, which tend to be higher in warmer regions, producing a positive correlation with SST-JJA. Consequently, the most extreme flowering events occur where high absolute temperatures (SST-JJA) coincide with substantial cumulative heatwave exposure (MHW-icum), even if SSTA-JJA is moderate, explaining the patterns reported in the manuscript.

- ***Fig.5 and related text: In my opinion, this is the better achieved analysis, text and figure, and should work as the fundamental basis for this article.***

Nevertheless, I still do not find the grounds for the 180 degree.days and 0.07 FI as thresholds.

Response: This 180°Cdays threshold is clearly visible on the panel B and C of the Fig. 5 (now Fig. 6). This is also consistent with Fig. 3 (now Fig. 4, on both panel A and B, the occurrence of high FI increases at about 180°Cdays and more MHWicum) and Fig 4 (now Fig. 5; the large majority of high FI are related to MHWicum>180°Cdays). This threshold also corresponds to the 50% probability to obtain a FI higher than 0.07 (see the added supplementary figure Fig S3).

Please refer to the previous comment concerning the defining of the 0.07 FI threshold.

- Furthermore, in lines 285-287 the authors mention "Consistent with our earlier analysis, flowering occurrences in 2003 were more likely when MHW-icum exceeded approximately 100 °Cdays [101 ± 34°Cdays] (Fig. 5B)."

First, are the authors sure they mean 100 °Cdays? I really do not see this in Fig.5B. Second, a 100 °Cdays threshold for the 2003 data is very different from a 180 °Cdays threshold for the 2022 data. I do not see any consistency here.

Response: We acknowledge that this sentence is confusing. The 100°Cdays value corresponds to the mean of the cluster under 0.07 FI. We understand it could seem inconsistent. The threshold of 180°Cdays corresponds to high FI triggering, which means that low FI can occur when MHW-icum is lower. The two last sentences of this paragraph have been rephrased for more clarity and consistency.

Lines 305-308: "Consistent with our earlier analysis, high-intensity flowering in 2003 was associated with MHW-icum values centred around 180 °Cdays [182 ± 9 °Cdays] (Fig. 6B), closely aligning with the probability threshold identified in our 2022 dataset (Fig. 6C)."

- I suggest that the authors try log-transform the data, and fit a regression line. In case there are zeros in the FI, do not log(x+1) transform as this introduces a severe bias, particularly in linear regressions (O'Hara and Kotze 2010, Xiao Xiao et al 2011, St Pierre et al 2018, Chen and Roth 2023,). Any log(x+C) transform introduces bias, which is greater the largest the C relative to x. In the present case 1 is always larger than FI, rendering the bias huge. An alternative is applying the logit transform y=exp(x)/(1+exp(x)), which works perfect for 0<x<1, leading y to range between 0.5 and 0.73. Reverse calculation is given by x=log(y/(1-y)) (See Vieira et al., 2018 – Subsection: composite survival rates).

Response: Log transformations were avoided in order to not introduced any bias as the reviewer suggests. We also did not apply the logit transformation to FI, we opted to retain FI on its original scale. This choice facilitates interpretation and avoids introducing additional transformations that do not substantially improve model performance.

Reviewer #3 (Remarks to the Author):

This MS reports a striking—and worrisome—large-scale ecosystem response to climate with potentially major consequences for the marine ecosystems of the Mediterranean Sea. The mass flowering of the seagrass Posidonia following a marine heat wave is an unmistakable signal of stress from climate warming that has reached levels likely to transform the entire regional ecosystem in coming years. The coordination of researchers sampling 442 sites across the region and the resulting dataset are impressive, and the data are generally convincing. This is a story that needs widespread attention.

I have a few suggestions for improving the MS:

First, the analysis and presentation. The general and important result of mass flowering across the region following a heat wave is clear in the data. But the paper discusses several thresholds that are not justified in the text. For example:

Line 230: "flowering (n = 333) occurred preferentially under the warmest conditions (Fig. 3A), with a probability of flowering higher than 50% when MHW-icum exceeds 120°C days." First, there are no labels on the vertical axis of the figure so it is impossible to know whether or when flowering occurrence exceeded 50%. Second, these numbers are presented without context or justification. What is the significance of the 50% threshold for flowering, rather than, for example, 10% or 80%? Is this a standard convention? And why 120 degree days? To my eye the transition in figure 3A appears closer to 160 days, when the occurrence of flowering abruptly increases. Did these threshold numbers come from a quantitative analysis? if so, this should be explained. If not, how were they chosen?

Response: We thank the reviewer for this relevant remark. Figure 3 (now Fig4) A and B panels present the probability of flowering according to MHW-icum. The both panels have been updated by adding a Y axis and a scale.

To clarify the probability of flowering, we added a supplementary figure, Figure S3, which presents the relative probability of flowering under different thermal conditions. Because the model is binomial (0 = no flowering, 1 = flowering), the 50% probability threshold corresponds to the point at which flowering becomes more likely than not. In Figure S3A, this occurs at approximately 120 °C·days of cumulative MHW exposure (MHW-icum), while in terms of flowering intensity (FI > 0.07, Fig. S3B), the 50% threshold occurs around 180 °C·days. These thresholds are derived directly from the model predictions rather than being arbitrarily chosen.

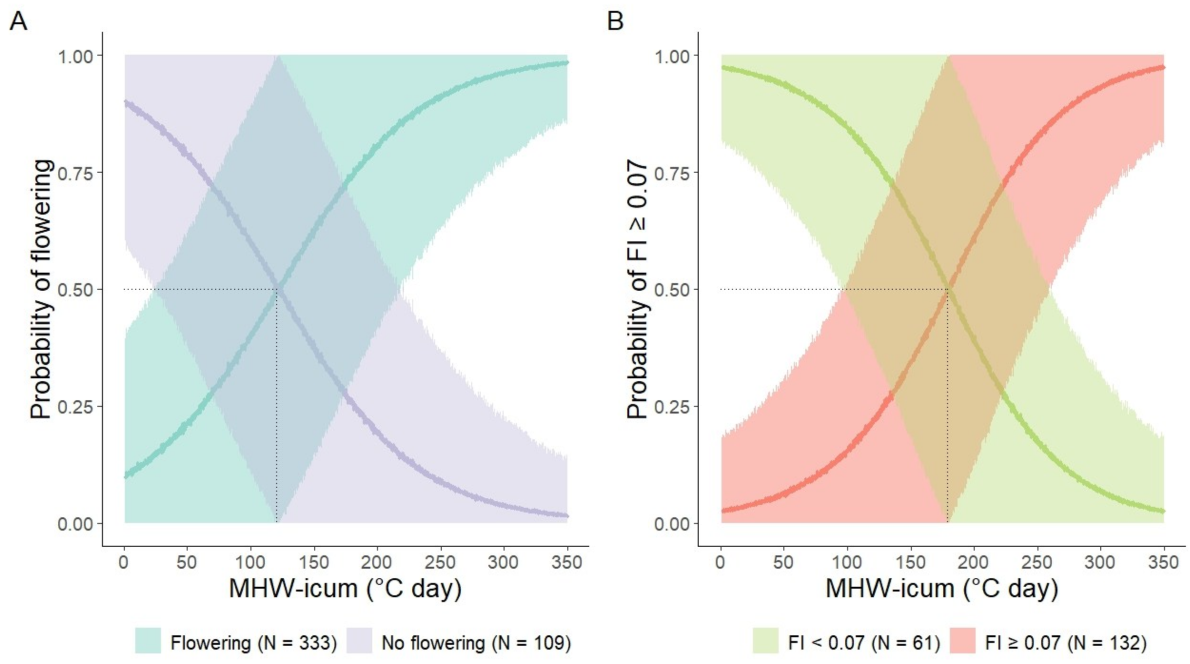
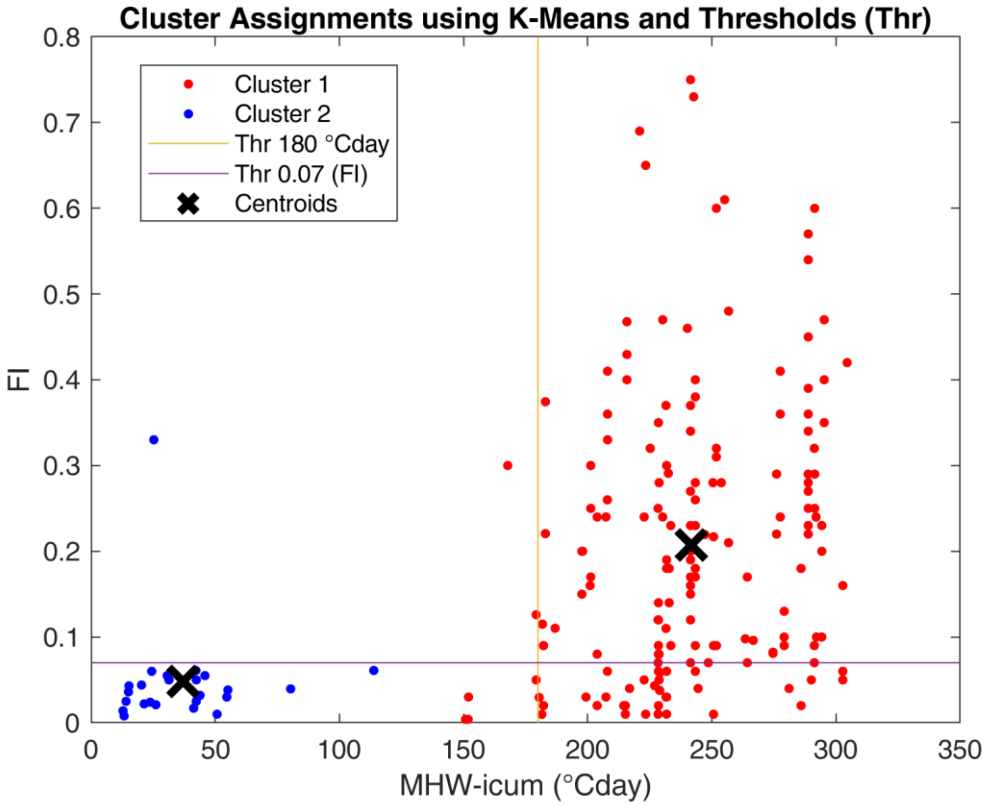


Fig. S3. Panel A: Probability of flowering according to marine heatwave cumulative intensity (MHW-icum in °Cday). Panel B: Probability of observing a flowering intensity over or equal to 0.07 according to MHW-icum.

- Line 805: "The threshold of 0.07 of FI was defined according to the distribution of our dataset and considering historical value range from Diaz-Almela et al. [12]." This number is not intuitive and this sentence is not very informative. Did the cited paper find the threshold of 0.07? Apparently not, since I searched the Diaz-Almela reference for "0.07" and did not find it. What sort of analysis of these data produced the result of 0.07? What is the confidence in that number? Since space is less restricted for the online methods (yes?) there should be plenty of room to explain here. These thresholds are clearly central to the narrative of this MS so it is critical to explain and justify them.

<u>Response:</u> As also responded to Reviewer #2, the threshold of FI = 0.07 was chosen based on both historical records and the distribution of observed flowering intensity in our dataset. Historical data from Diaz-Almela et al. (2007) indicate that prior to 2003, flowering events with FI above 0.07 were very rare and typically occurred during large, synchronised events (e.g., 1975, 1982, 1985, 1993–1994). In Diaz-Almela et al. (2007) dataset, the distribution of FI values (see Fig. 5c in Diaz-Almela et al., 2007) shows that the most frequent values fall around 0.05, while higher values (≥0.1) correspond to relatively high flowering events. This threshold is also supported by clustering of the original quantitative data from our data set using K-Means as shown in the Figure below now added as Figure S7. By selecting a threshold of 0.07, we capture events that are clearly above baseline levels but not so restrictive as to exclude moderately elevated flowering, allowing a meaningful assessment of ecologically significant increases in flowering intensity. This threshold therefore reflects a balance between historical rarity and contemporary relevance. A sentence has been added in the online methods to clarify this point.



Supplementary Figure 7: Cluster assignments of flowering intensity (FI) according to marine heatwaves cumulative intensity (MHW-icum) used to define the FI threshold (Thr) of 0.07 and corresponding MHW-icum threshold of 180°Cday.

Lines 828-831: The threshold of 0.07 of FI was defined according to the distribution of our dataset and considering historical value range from Diaz-Almela et al. [12]. This threshold is also supported by clustering of the original quantitative data from our data set using K-Means (Fig. S7).

- My second concern involves the writing. The text is very dense, filled with abbreviations that are cumbersome, often unnecessary, and taxing on the reader. Flowering Intensity is abbreviated as FI throughout, which is not necessary. To avoid confusion, I recommend defining flowering intensity and prevalence in the abstract when they are first used and simply using the words.

Response: Abbreviations were used to save space with regards to the limited size of the body text. FI for Flowering Intensity or FP for Flowering Prevalence have been already used in several previous papers, including Diaz-Almela et al. (2007), and those acronyms are defined the first time they are introduced in the text.

- Also I feel confident that this group of authors can find a better label than MHW-icum, an opaque and previously unknown term (I googled it) with 8 characters, almost as long as a full, more descriptive word that would be much more digestible by readers. I recommend

finding such a word and simply defining it clearly at the beginning. Similarly, rather than using SST-JJA can you simply define this and then call it "summer temperature"?

Response: We prefer to keep the labels / acronyms as they are right now._MHW-icum is a common abbreviation for MHW cumulative intensity, already used in many scientific papers worldwide (e.g., Ibrahim et al., 2021: https://doi.org/10.3390/jmse9060643; Qi et al., 2022: https://doi.org/10.1029/2022JC018732; Hamdeno et al., 2023: https://doi.org/10.3389/fmars.2023.1093760) . We consider that writing systematically 'MHW cumulative intensity' instead of MHW-icum would complicate the readability of the text. Similarly, SST is a widely used abbreviation in Climate Change research in the marine realm, we could use summer-SST, instead of SST-JJA, but JJA reflects more accurately the months we are working with (June, July, August). We believe that using the term summer temperature would bring inaccuracy. The first time that an abbreviation is presented in the text it is defined in order to avoid confusions.

- Section headers especially should avoid cumbersome abbreviations (for example, 2.3: "Relationships between MHW descriptors and MFE"). Readers use section headers to navigate around a MS, so their value is primarily for those who have not yet read the paper carefully. Do them (and yourselves) a favor and make the headers clear.

Response: We agree with the reviewer, and we have changed the title and removed the abbreviation

Line 234: "2.3 Relationships between marine heatwave descriptors and the mass flowering event"

- Similarly, there are paragraphs full of parenthetical numbers (e.g., lines 190-199) that would be better presented in tables or figures. The text should provide a concise but rigorous narrative about what happened in as plain language as possible, referencing the tables and figures for the details.

Response: The numbers are also presented in table 1. We agree with the reviewer's suggestion. We have removed the number between brackets for a better readability.

Lines 196-199: The highest FP values were observed in the Liguro-Provençal region and the Balearic Sea. Intermediate FPs were observed in the Alboran and Tyrrhenian Seas. The Aegean and Ionian Seas were characterised by low FPs, while no flowering was recorded within the Adriatic Sea (Table 1, Fig. 2).

Lines 201-208: The overall mean Flowering Intensity (FI; proportion of shoots bearing inflorescence in relation to the total number of shoots) was 0.15 ± 0.16 across the whole Mediterranean basin, ranging between 0 (e.g., Adriatic Sea) and 0.75 (Giglio Island, Tuscany, Liguro-Provençal ecoregion). Strong differences in FI were observed between ecoregions ($p < 0.001$; Fig. 2, Table S2), particularly between western and eastern ecoregions (pairwise test, $p < 0.05$). A higher mean FI was observed in the Tunisian Plateau with 0.60 ± 0.19 (n = 4 records), followed by the Balearic Sea, the Liguro-Provençal ecoregion, and the Tyrrhenian Sea. The mean FI was low in the Alboran Sea and very low in the Aegean and Ionian Seas (Figs. 2 and 3; Table 1).

Other comments:

- Line 127: mass mortality of what?

Response: Here, we mean mass mortality of different organisms (mostly including sessile benthic invertebrates; see the corresponding references such as Garrabou et al., 2022) at large. We have specified this in the sentence.

Line 126-128: Summer 2022 saw record-high MHWs in the Mediterranean Sea [4, 5], resulting in mass mortality events of many sessile benthic organisms [6, 7].

- Line 153: "We assessed its spatial distribution, characterized summer thermal conditions using high-resolution satellite- derived SST data, and examined potential links with both biotic (e.g., meadow structure) and abiotic (e.g., depth, protection status, temperature) factors." No results are presented regarding effects of protection status so why is it described here (and in online methods)?

Response: Management level is shortly analysed in the last paragraph of 2.2. We changed "protection status" by "management level" for more clarity.

Line 155: "(e.g., depth, management level, temperature)"

- Line 157: Flowering intensity has now been referred to at least twice without a definition.

Response: We have added a short definition between brackets

Line 156: "Additionally, we compared flowering intensity (proportion of shoots bearing an inflorescence) between…"

- Figure 1B: Why is there no symbol for flowering prevalence for Tunisia?

Response: We did not present the flowering prevalence for ecoregions with less than 10 records (only the case for Tunisan Plateau and Levantine Sea).

See section 4.1.3. lines 758-759: "FP was calculated for each ecoregion that contained at least 10 records"

-Figure 3A: Is flowering occurrence the same as prevalence? Terms should be harmonized between figure and text.

Response: No, occurrence and prevalence have different meaning here. In Fig 3A (now Fig. 4A), the flowering occurrence corresponds to the observation of at least one inflorescence on a site while the flowering prevalence corresponds to the proportion of sites where flowering happened in a given area (e.g., ecoregion). This has been further specified/ clarified in the text (see caption of Figure 4: lines 254-257 and section 4.1.3. lines 757-758).

Lines 254-257: Figure 4: Relative distribution of the marine heatwave cumulative intensities (MHW-icum; ºCday) associated with *Posidonia oceanica* flowering: A) probability of flowering occurrence (i.e., observation of a flowering event) and B) probability of flowering intensities equal to or lower than 0.07, or greater than 0.07. The two FI clusters were defined using k-means.

Lines 757-758: Flowering Prevalence (FP) was estimated as the number of records reporting flowering in relation to the total number of records obtained throughout the study [12].

- Figure 4a is puzzling and difficult to interpret. I would leave it out.

Response: While we understand that Figure 4A (now Fig. 5A) may appear complex, we found it useful to visualize the structure of the hierarchical model and the distribution of the data across Mediterranean ecoregions, which helps contextualize the subsequent regional slopes in Figures 5B–D. Indeed, as noted by Reviewer 1, this panel provides a clear overview of how flowering intensity relates to thermal conditions within and across ecoregions. Therefore, we believe retaining Figure 5A is important for readers to interpret the model outputs accurately.

- Figure S3: it would be helpful to show the curves and confidence limits in the figures with the points since it is often not at all clear where the correlations come from. For example, FI versus latitude shows a correlation of .235 with three asterisks, yet the scatterplot does not appear to show any relationship. Probably this is because the data are highly abnormally distributed. Whatever the case it would be good to see the fitted curve and confidence intervals.

Response: We proposed a revised version of the figure adding the curve and the confidence intervals (95%). As suggested by the reviewer 2, the Fig S3 has been added in the manuscript as Fig. 1.

April 19th 2026

REPLY TO REVIEWERS

Communications Earth & Environment

Revision version manuscript: COMMSENV-25-4036-T

REVIEWER COMMENTS: in bold and italics, line numbers refer to original submission
OUR RESPONSES line numbers refer to revised word doc or pdf without visible track changes.

Editor's comments

Our reply below sounds a bit vague and, extraordinarily, I am contacting you with a personalized and detailed accounting of the situation.

I entirely agree with the concerns raised by reviewers 2 and 3. On basically everything, but particularly about the proposed 0.07 PI and 180ºCdays thresholds lacking support. Even more, now that reviewer 2 brought to our attention that it is also conflictive with the results from the FE metric as well as previous recent works by yourselves, with the same species in the same places and the same heatwave.

Our usual policy is to make a final decision at this stage. In such case, my recommendation was for Rejection based on the above-mentioned lack of support of the thresholds you propose and the big role that they play in the message you are trying to pass. However, on a general basis, I find your work extremely relevant. Therefore, I recommended and appealed that you were given an extraordinary chance to change your work following these recommendations by reviewers 2 and 3.

I find very unlikely that me, as well as reviewers 2 and 3, change our stance and recommend approval if you insist on those thresholds. I highlight that the next decision will be final.

With due changes, I would be very glad to see this very interesting work published in COMMSENV.

Reviewers' Comments:

Reviewer #1 (Remarks to the Author):

I read the response to review and the revised manuscript and feel that the authors have addressed my review comments successfully.

Response: We thank Reviewer #1 for his/her positive comments.

Reviewer #2 (Remarks to the Author):

I would like to thank the authors for submitting the revised manuscript and for the improvements made to the text. However, I still have several questions and concerns that, in my opinion, were not adequately addressed.

Response: We thank you for your time and your second review of our manuscript.

All reviewers consistently requested clarification of the thresholds used in the manuscript, namely FI = 0.07 and 180 degree-days. This clearly indicates that this crucial aspect of the study was not sufficiently explained in the original version. In the revised manuscript, the authors attempt to justify the selected thresholds and include new figures. Nevertheless, the choice of a 7% FI threshold still appears largely arbitrary. The K-means and threshold analysis shown in Figure S7 suggests that a 6% threshold might be more appropriate, which would substantially lower the proposed MHWicum threshold from the currently suggested 180 °C·days. Conversely, if the authors had selected the 10% threshold mentioned in their response, the resulting degree-day threshold—derived using their own method (Supplementary Figure S3 and their reply)—would be considerably higher than 180 °C·days. Further highlighting the inconsistency of proposing thresholds for Marine Heat Wave (MHW) impacts on seagrass flowering, there is a striking discrepancy between the 180 °C·days threshold obtained when MHWicum is compared with FI and the 120 °C·days threshold obtained when it is compared with FP.

Response:

Thank you for this comment. We agree with the arbitrary character of the threshold defining method, even if data-driven. Consequently, we propose to abandon the initially proposed thresholds for FI (0.07) and MHW-icum (180°Cdays) throughout the manuscript as suggested by the reviewer. Consequently, we also propose the removal of Panel B of figure 4 and Panel B of Fig S3.

We modified the text as follows:

Line 244-255 (Results): "The analysis of FI quantitative data above zero (n = 193) highlighted contrasted patterns depending on thermal conditions (Fig. 3). Most of the records exhibiting moderate to large FI level (3, 4 or 5 FI level; n = 126, mean = 0.29 ± 0.16) were associated with elevated MHW-icum (mean = 246 ± 72 °Cdays). In contrast, large scatter of thermal exposure (mean = 180 ± 72 °Cdays) was found for small and isolated flowering (1 or 2 FI levels; n = 82, mean = 0.05 ± 0.16). When flowering was absent (FI = 0; n = 110), thermal exposure was found lower (mean = 128 ± 75 °Cdays).

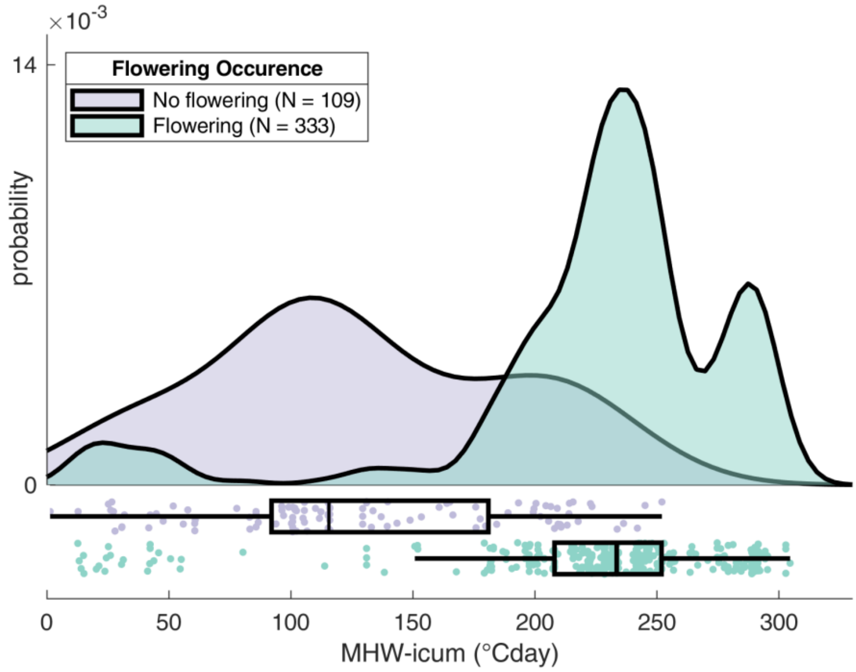


Figure 4: Relative distribution of the marine heatwave cumulative intensities (MHW-icum; ºCday) associated with the probability of *Posidonia oceanica* flowering occurrence (i.e., observation of a flowering event)."

Lines 84-836 (Online methods): "We further applied a second Bayesian approach to track flowering intensity continuously. Because FI is bounded between 0 and 1, values were logit-transformed, and a normal likelihood was applied on the transformed scale (Eq. 4–7)."

Several of the co-authors previously published a study on the effects of MHWs on the same species (Posidonia oceanica) and within part of the same geographical domain (Eastern Atlantic; García-Escudero et al., 2024). In that study, no thresholds were proposed. Instead, the authors analyzed the effect of the same MHWicum predictor on the same FI response using a GAMM, which yielded a linear relationship. This is consistent with the suggestion already raised during the first version of the manuscript. Their results clearly indicate a progressive, rather than abrupt, response to MHWs, in contrast to the threshold-based response now proposed. In that study, MHWs led to above-average FI values from approximately 30 °C·days onward, which strongly contrasts with the 180 °C·days threshold proposed here. Moreover, despite the observed increase in FI, values were almost always below the 7% threshold now proposed, with a mean FI of 2.52%. Only two observations exceeded 7%, reaching 8.47% and 8.86%, as reported in their supplementary Excel file.

Based on these considerations, I find that the thresholds currently proposed are not sufficiently supported and may be misleading. Instead, the authors should focus on a progressive response to MHWs, as demonstrated in previous work by some of the same co-authors (García-Escudero et al., 2024).

<u>Response:</u> We agree to modify our statement by removing the threshold hypothesis for MHWicum and FI and modifying the text accordingly.

The results of García-Escudero et al. (2024), to which several co-authors of the present manuscript also contributed, focus exclusively on the Greek Seas (Ionian and Aegean), i.e. the eastern Mediterranean basin, which is characterised by thermal regimes that differ fundamentally from those of the western Mediterranean. In particular, thermal descriptors over 2010–2021 in the eastern basin remain far below the extreme conditions reached in 2022 in the western basin. For example, MHWicum values associated with higher FP in the eastern basin reach at most ~75 °C·days (e.g. in 2010 and 2018), whereas values up to ~250 °C·days and more were recorded in the western basin in 2022.

García-Escudero et al. (2024) indeed demonstrate a relationship between marine heatwaves and flowering, but in a thermal context where cumulative intensities never exceeded 150 °C·days and are most often below 50 °C·days. These levels are comparatively very low when placed in the basin-scale perspective of major events such as 2003 and, especially, 2022. Consequently, the thermal forcing explored in that study only partly overlap with the range of intensities analysed here.

Our statement is therefore not hypothesis-driven but strictly data-driven, and is directly supported by our observations (Fig. 6B, C). In addition, flowering in *Posidonia oceanica* is not controlled by temperature alone. The process requires sufficient prior storage of carbon reserves (starch) to meet the energetic cost of inflorescence production. As a result, flowering is unlikely to occur in consecutive years at the same site, which explains why some locations do not flower despite experiencing high MHWicum.

Finally, 2022 represents an unprecedented thermal event for the Mediterranean basin. Even the extreme summer of 2003, long considered exceptional, did not reach comparable cumulative intensities (Fig. 6A).

I also believe that the term "unprecedented flowering" should be removed or replaced, as the authors do not demonstrate that the 2022 event was truly unprecedented. Notably, the authors overlook their own previous study (André et al., 2023), which examined the effects of the same 2022 MHW on the same species and within part of the same geographical domain (French Mediterranean coast). That study explicitly addressed whether the observed mass flowering was unprecedented and concluded that it was not, directly contradicting the current claim. Furthermore, in that earlier work (page 91), the authors report multiple historical mass flowering events of Posidonia oceanica in 1961, 1963, 1970, 1971, 1974, 1975, 1982, 1983, 1985, 1994, and 2003. Given this historical context, it is unclear how the 2022 event can be described as unprecedented when similar events have occurred on more than ten previous occasions.

The manuscript would be substantially strengthened if these historical data were incorporated into the analysis, allowing the 2022 event to be placed within a broader temporal and ecological context.

<u>Response:</u> Unfortunately, earlier events do not have sufficient data available to include them in our analysis. Only the 2003 event provided enough exploitable records (n = 24) to allow meaningful integration, as shown in Fig. 6B. This is why we believe our data collection (from 2022 event) as unique for this specific topic (*Posidonia oceanica* flowering). We therefore rely here on the pivotal study by Díaz-Almela et al. (2007), which clearly demonstrates that flowering intensity (FI) during 2003 and before did not approach the levels observed in 2022. This supports our interpretation that the 2022 event represents a fundamentally unprecedented thermal regime.

Following your suggestion, we have considered the other flowering records available from Diaz-Almela et al. (2007) and Garcia-Escudero et al. (2024). Consequently, we have updated Fig6B by adding data from 1982 to 2001 (mostly from Western Mediterranean) and from 2009-2021 (from Greek Aegean Sea), completing the initial 2003 plot. This brings spatial and temporal unbalance to the plot but provides an interesting insight to past high FI value (up to 0.3) despite low MHW-icum, jeopardizing our initial hypothesis about FI and MHW-icum thresholds but it also highlights that historical data about FI and related MHW-icum are far lower compare to 2003 and 2022, confirming the exceptional nature of 2022 event. We have also updated Fig6C to remove the FI 0.07 threshold information.

We have now clarified these points explicitly in the revised text in the Results and Online methods sections as follows:

Lines 294-317: Results

"**Comparison with the 2003 Mass Flowering Event (western Mediterranean) and other historical records**

To further contextualize our findings, we compared our results with the historical MFE of *P. oceanica* reported for the western Mediterranean in 2003 [12] completed by additional historical records available from 1982-2001 [12] and 2009-2021 [20]. Analysis of SSTA-JJA in the western Mediterranean from 1982 to 2022 revealed that both 2003 and 2022 were characterized by anomalously high summer SST, exceeding the historical range of the 1982–2021 time series. Summers 2022 and 2003 ranked as the warmest and 2nd warmest, respectively, since 1982 (26.10°C and 25.84°C, respectively). Median SSTA-JJA values during these two years reached 2.12°C [Q25: 1.72°C, Q75: 2.55°C] and 2.53°C [Q25: 1.96°C, Q75: 2.80°C] for 2003 and 2022 respectively (Fig. 6A). Consistent with our earlier analysis, high-intensity flowering in 2003 was associated with MHW-icum values above 150 °Cdays [182 ± 9 °Cdays] (Fig. 6B), closely aligning with the probability identified in our 2022 dataset (Fig. 6C). However, it is worth noting that historical records from 1982-2021 (excluding 2003) are associated with low MHW-icum (Fig 6B; mean = 26 ± 40 °Cdays), confirming the exceptional nature of 2003 and 2022 MHWs (Fig 6A).

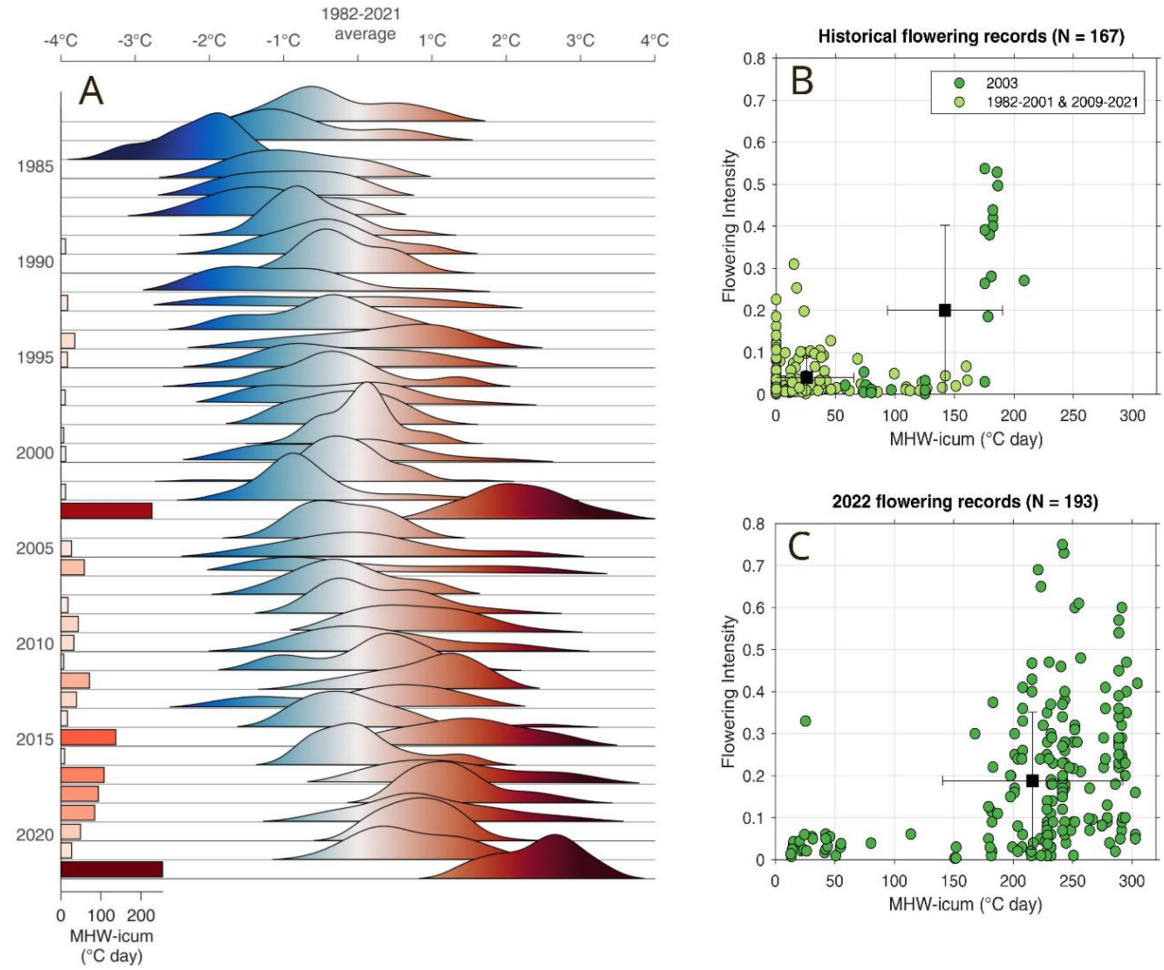


Figure 6: A) Summer daily sea surface temperature anomaly (SSTA in °C, referenced to the period 1982-2021) and marine heatwave cumulative intensities (MHW-icum in °Cday) from 1982 to 2022 in the western Mediterranean; Flowering Intensity (FI) in relation to MHW-icum for: B) 2003 in dark green and 1982-2001 and 2009-2021 in light green (from Diaz-Almela et al. 2007 and Garcia-Escudero et al., 2024) and for C) 2022 (present work). The mean ± standard deviation values are shown in black."

Lines 803-810: Online methods

"Comparison with 2003 flowering event and other available historical records
The dataset from Díaz-Almela et al. [12] on flowering records reported after the 2003 MHWs, was completed with authors' records, and was imported in numerical format and graphically analysed. The validated dataset consists of quantitative FI data for 24 sites for which nearest satellite SST time series were extracted and considered for MHW analysis. The dataset validated with original authors consists of 24 records (sites) for 2003 (available in Supp. Mat. 1). In addition, historical records from 1982-2001 (from [12]; n = 101) and from 2009-2021 (from [27]; n = 42) were also considered for MHW analysis (Supp. Mat. 1)."

In addition, we propose to specify that the 2022 event is one of the most intense 'among documented events', we cannot exclude the possibility that earlier, undocumented events may have been more intense than those reported in the literature (Díaz-Almela et al., 2007; André et al., 2023). It is also important to note that the conclusion of André et al. (2023) does not establish the unprecedented nature of the event at the Mediterranean scale, as their analysis is restricted to the French Mediterranean coast. By contrast, the present manuscript compiles a basin-scale dataset and reports the highest flowering intensity (FI) values ever recorded, exceeding the previous record of 0.54 observed in the Balearic Islands in 2003. Also considering a more extended historical dataset (Fig 6B), as suggested by the reviewer #2, we provide relevant elements to justify our statement.

We have clarified this point in the revised text as follows:

Line 321-323: Based on the currently available historical records [12, 20], the 2022 event appears to be one of the most intense flowering events ever documented for this species.

Finally, the authors rejected the suggestion to transform the data, arguing that it would complicate interpretation and provide no added value. I disagree with both points. Regarding interpretation, the authors could easily present dual axes, with the original values displayed alongside (and parallel to) the transformed scale. Concerning added value, the current approach discards all observations with zero flowering intensity (lines 879–882): "Finally, we developed a model to track flowering intensity continuously, using a log-normal distribution to account for the non-negative and skewed nature of the flowering intensity data (Eq. 7). The dataset was subset to include only the observations where flowering occurred following summer 2022, and any values with no flowering observations were removed." This procedure clearly introduces a systematic bias. By applying the suggested logit transformation, all observations—including zeros—could be retained, thereby avoiding this bias.

<u>Response:</u>

We thank the reviewer for this constructive comment. We agree that transforming the data can improve model behaviour, and we have now revised our analysis accordingly. Following the reviewer's suggestion, flowering-intensity values were logit-transformed prior to modelling while retaining all zero-FI observations. No observations were discarded in the revised analysis. We have updated the Methods to explicitly describe this transformation and now clarify that the continuous FI model uses the full dataset, including zero-FI cases.

Lines 834-850: "We further applied a second Bayesian approach to track flowering intensity continuously. Because FI is bounded between 0 and 1, values were logit-transformed, and a normal likelihood was applied on the transformed scale (Eq. 4–7).

The model includes MHW-icum, SST-JJA and a vector of n = 9 levels of ecoregions observed. These vectors construct a hierarchical matrix ζ with n rows and two columns, representing ecoregion-level additive deviations from β_0. In this model, Ω is the Cholesky factor of the correlation matrix among hierarchical effects, Z is a diagonal matrix with a vector of among-ecoregion standard deviations (σ_ζ), and δ_s is an s-by-two matrix of standardized hierarchical effects. We ran the model with four chains, each with 4 000 draws and a warm-up period of 1 000 steps, retaining 12 000 posterior draws for analysis. Models (1), (4) and (7) displayed R^2 of 0.32, 0.33 and 0.68 respectively. All statistical analyses were conducted using R v. 4.4.3 [71]."

In addition, the figures and the Results section associated with this model have been updated to reflect the logit transformation. This revision removes the potential bias previously highlighted by the reviewer and improves the model's interpretability and statistical robustness.

Lines 257-277: “To explore spatial variation in *P. oceanica* FI across the Mediterranean, we modelled continuous FI values as a function of mean summer SST (SST-JJA) and cumulative marine heatwave intensity (MHW-icum), while accounting for inherent baseline temperature differences among the different Mediterranean ecoregions (Fig. 5A). Our results reveal a positive relationship between thermal anomalies and flowering: both SST-JJA and MHW-icum were positively associated with FI across the dataset, with variations across regions. The highest MHW-icum values were recorded over the Tyrrhenian Sea, corresponding to some of the most intense flowering events observed. Similarly, the Balearic Sea and the Tunisian Plateau experienced both high MHW-icum and SST-JJA values, which also coincided with substantial flowering. These findings indicate that the most extreme flowering events are associated with the joint occurrence of high SST-JJA and elevated MHW-icum. Region-specific modelling further showed that all intercepts were negative (Fig. 5B), indicating that baseline flowering was generally unlikely in the absence of strong thermal forcing, particularly in western regions. In contrast, the slope estimates for both SST-JJA and MHW-icum were predominantly positive (Figs. 5C and 5D), supporting an association between thermal anomalies and flowering intensity. The regional slopes in Fig. 5B–D reflect variation in the strength of the flowering response to MHW-icum across ecoregions. For SST-JJA and the intercept, 75% credible intervals did not cross zero, making these coefficients interpretable. Credible intervals for MHW-icum were wider and often including zero, reflecting greater uncertainty and contrasting trends among regions. Positive regional slope values indicate that flowering in that region responds more strongly to thermal anomalies than the entire Mediterranean.”

Lines 278-292: “

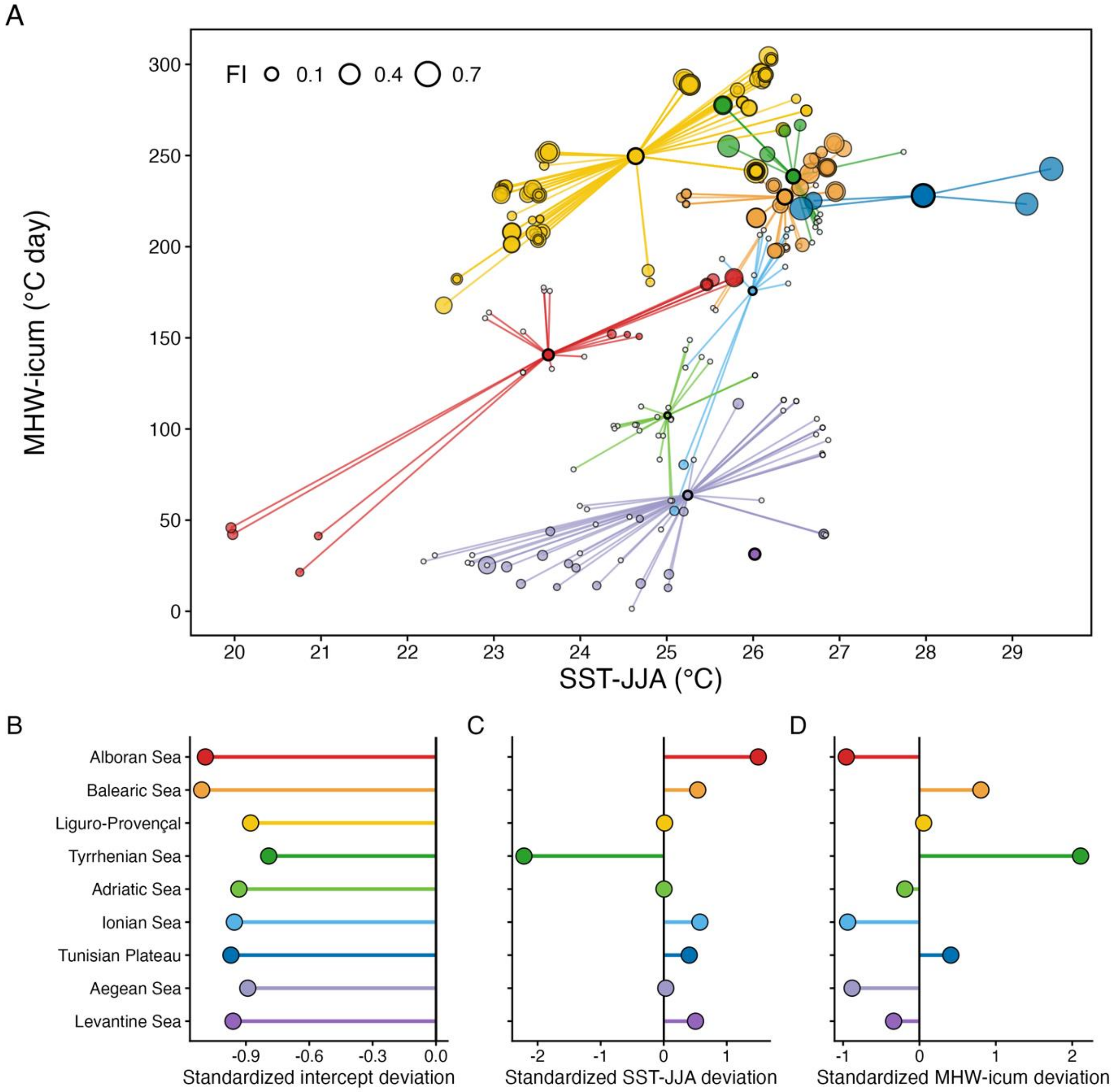


Figure 5: Relationships between flowering intensity (FI), summer sea surface temperature (SST-JJA), and cumulative marine heatwave cumulative intensity (MHW-icum) across nine Mediterranean ecoregions. A) Three-dimensional scatterplot illustrating the relationships among FI, SST-JJA, and MHW-icum. Each point represents a site, scaled by FI magnitude and connected to its corresponding regional centroid. No FI observations are filled in white. (B–D) Standardized random-effect deviations from the hierarchical Bayesian model of logit-transformed FI as a function of SST-JJA and MHW-icum (see Online Methods section 4.3, equation 8). These values represent how much each ecoregion departs from the global intercept or global slopes, expressed in standard deviation units: B) deviation from the global intercept, C) deviation from the global SST-JJA effect, D) deviation from the global MHW-icum effect. Colours denote regions: Alboran Sea (red), Balearic Sea (orange), Liguro-Provençal Basin (yellow), Tyrrhenian Sea (green), Adriatic Sea (light green), Ionian Sea (light blue), Tunisian Plateau (dark blue), Aegean Sea (light purple), and Levantine Sea (dark purple)."