Response to reviewer #1

General Comments

This manuscript provides a relatively systematic analysis of MHWs over the Patagonian Shelf region based on satellite-derived daily SST data. The study quantifies MHW frequency, intensity, days, and long-term trends, and further explores their relationship with the ENSO as well as the sensitivity of results to different baselines.

While the manuscript is generally well structured and the figures are informative, several important issues need to be addressed to strengthen the scientific rigor and clarity of the study. These include better articulation of the motivation, clearer connection between physical mechanisms and MHW patterns, justification of methodological choices, and more accurate interpretation of results. I hereafter give major comments, followed by line-by-line suggestions to ease revision.

We thank the reviewer for the thorough and constructive revision. We have modified the original manuscript and figures according to the suggestions. We believe that the revised version is clearer now, with an improved explanation of the study's motivation, which also strengthens the relevance of our findings.

In addition, the clarifications introduced throughout the methodology and the interpretation of the results have improved the overall scientific rigor of the study. We have made a greater effort to explain the physical mechanisms modulating the spatial distribution and interannual variability of Marine Heatwaves (MHWs) over the Patagonian Shelf (PS). Furthermore, several figures have been revised to improve clarity in the presentation and interpretation of MHW characteristics in the PS.

All changes are highlighted in red.

Major Comments:

1.Abstract: The abstract clearly summarizes what was done and what was found. However, it does not explicitly explain why this study is important. The authors should better articulate the scientific motivation and highlight the novelty of their work, so that readers can immediately understand what is new or unique about this study.

Introduction: The author's research motivation is not clearly articulated. It is recommended that the introduction be strengthened by establishing clearer connections with previous studies and by explaining the necessity and relevance of the present research.

Thank you very much for this valuable comment. We agree that the scientific motivation of the study was not sufficiently articulated in the original version. In

response, we have revised the Abstract and substantially expanded the Introduction to more clearly explain the motivation, relevance, and novelty of this work. The revised Introduction now provides a stronger justification for conducting this study in the Southwestern Atlantic Ocean (SWA), particularly along the PS.

Specifically, we now emphasize the importance of comparing and selecting methodological approaches commonly used to define MHWs on a regional scale, a choice that is not trivial. We clarify that the influence of these methodological decisions is region-dependent and tends to be particularly pronounced in areas with strong long-term sea surface temperature (SST) variability or rapid warming, where MHWs statistics can be substantially affected.

In addition, we highlight the global ecological relevance of the PS as a highly productive region and a major carbon sink that supports extensive phytoplankton blooms and key fishery resources. Given that phytoplankton biomass in this area is already exhibiting changes associated with increasing SST and mixed-layer shoaling, understanding how MHWs manifest in this region is essential. Despite its importance, a comprehensive characterization of MHWs along the PS has been lacking, representing a clear knowledge gap that our study directly addresses.

We believe these additions clarify the scientific motivation behind the study and strengthen the reason for conducting the analysis in this region. Please see the updated Introduction in the revised manuscript.

Section 2.1 provides a detailed description of the complex oceanic background of the PS, highlighting features such as the confluence of the BC and the MC. However, the subsequent analysis of MHW results (e.g., trends and distribution) fails to explicitly link the spatial distribution and temporal variability of MHWs to these specific regional physical mechanisms. The authors should consider analyzing the influence of local oceanographic features on MHW occurrence. Otherwise, the detailed context provided in Section 2.1 becomes scientifically superfluous.

We acknowledge the reviewer's point regarding the importance of identifying the physical mechanisms that influence MHWs occurrence. However, a comprehensive analysis of the drivers triggering MHWs falls outside the primary objectives of the present study. Our focus is on evaluating different methodological approaches and datasets, and on providing the first broad characterization of MHWs frequency, spatial patterns, and long-term evolution in this still poorly studied sector of the ocean.

A thorough assessment of the drivers operating on the PS would require a separate, in-depth investigation. The region is highly heterogeneous, influenced by estuarine dynamics, substantial freshwater inputs, the convergence of subtropical and

subpolar water masses, major boundary currents, strong westerly winds, and a wide gradient in tidal regimes. Because these processes interact in complex ways, attributing MHWs variability to specific climate modes would demand a dedicated, process-oriented analysis including atmospheric and oceanic forcing at different geographical scales (because of the diversity of the study area) that goes well beyond the scope of the current manuscript.

Nevertheless, in direct response to the reviewer's concern, we have strengthened the linkage between the regional physical setting and the observed MHWs patterns throughout the manuscript. Specifically, we revised Section 2.1 (Regional setting) to better support the interpretation of the MHWs results by emphasizing physical features that are directly relevant to the spatial distribution of MHWs intensity and to the interannual variability of MHWs occurrence. The revised section now highlights the role of shelf bathymetry and the importance of tidal currents in controlling water-column mixing, as well as the presence of air temperature gradients over the PS.

In addition, and importantly, we have modified Sections 3.2 and 3.4 to explicitly deepen the discussion of the physical mechanisms underlying the observed patterns. These sections now provide a more physically grounded interpretation of (i) the spatial distribution of MHWs intensity and (ii) the interannual variability in both the number of MHWs days and event intensity. Together, we believe these revisions ensure that the regional oceanographic context is directly linked to the MHWs results and interpretations, rather than serving as a purely descriptive background.

The PS is the main focus of this study. It is unclear why Section 3.1.1 discusses the influence of different MHW detection methods on the ZA region, as this seems inconsistent with the stated research objectives. If the authors wish to illustrate that the sensitivity of MHW statistics to baseline selection differs between open ocean and continental shelf areas, it is recommended to provide a direct comparison between the ZA and PS regions and explain why such differences occur. Clarifying these points would help align the analysis with the study's primary focus.

We agree with the reviewer that the PS is the primary focus of this study and that the inclusion of the Zapiola Anticyclone (ZA) region in Section 3.1.1 could distract from the main objectives and potentially cause confusion.

The original intention of including the ZA analysis was to illustrate that the choice of MHWs detection methodology is not trivial. In regions such as the ZA or the Mediterranean Sea, where long-term temperature trends are stronger, the use of a fixed versus a moving baseline climatology can lead to markedly different MHWs statistics. By contrast, our results show that, over the PS, both approaches currently

yield very similar outcomes, implying that either method can be applied depending on the specific research objective.

To better align the manuscript with its stated focus, we have moved the ZA analysis to Appendix B, where it is presented as a direct comparison. The main text now exclusively presents the methodological analysis for the PS, while interested readers can consult the appendix for a complementary discussion of how methodological sensitivity differs in a nearby open-ocean region.

Figure 7d is not addressed in the text. Please include a clear description and interpretation of its content.

We have revised the figures, and Figure 7b has been removed as it was not sufficiently relevant to be included in the manuscript.

The correlation between MHW EOF1 and SOI is very weak (r = 0.14), the conclusion that they exhibit a"consistent pattern"appears unsubstantiated based on this statistic alone. Is it possible to interpret this relationship from the perspective of underlying physical or dynamical mechanism?

We thank the reviewer for this insightful comment. In response, we have revised the manuscript to temper the original statement and to provide a more physically grounded interpretation of the relationship between MHWs and ENSO.

Specifically, we have clarified that the coherence between the Southern Oscillation Index (SOI) and the leading EOF of the annual number of MHW days is weak across all timescales, indicating that ENSO exerts only a limited influence on MHWs frequency. We now explicitly discuss that this weak coupling is likely masked by regional and local atmospheric–oceanic processes, as well as mesoscale variability, which dominate the occurrence of MHW days in the study region.

In contrast, we have added a new interpretation highlighting that MHWs intensity shows a substantially stronger coherence with SOI at interannual timescales (4–5 years), suggesting a more robust ENSO imprint on thermal anomalies. We further discuss that this contrast implies distinct controlling mechanisms, whereby large-scale climate variability more effectively modulates the magnitude of thermal anomalies than the frequency of MHWs events.

Minor comments:

1. Line 55-56. Delete "the incidence of climate forcing"

We have deleted that part of the sentence.

2. Line 69. Please list the full name of SWA

We have listed the full name.

3. Figure 1. The PS and SWA region is not clearly indicated. Please improve the figure to make the PS and SWA more distinguishable.

We specified both regions in Figure 1.

4. Section 2.2. Please include the spatial resolution of OSTIA-SST and ERA5-SST to clarify differences between the datasets.

We have added both resolutions.

5. Line 120. Please explain why the last 20 years were chosen as the moving baseline.

Our aim was to construct a definition of MHWs that consistently represents rare extreme warm anomalies while accounting for the ongoing shift in the underlying climatology. To do so, it is necessary that the reference period used to define MHWs thresholds adapt over time.

We selected the most recent 20-year period prior to the year under analysis for two main reasons. First, while the World Meteorological Organization recommends a 30-year baseline, applying a 30-year moving window in the context of a rapidly warming sea would inevitably incorporate older and cooler years, thus lowering the threshold and artificially inflating the number of detected MHWs events. Using 20 years offers a more balanced compromise: it is long enough to provide statistical robustness, yet sufficiently recent to avoid diluting present-day climatological conditions.

Second, the satellite SST record begins in 1982. Using a 30-year moving baseline would restrict the set of years for which a fully retrospective analysis can be performed. By using a 20-year baseline, we maximize temporal coverage and while still ensuring methodological consistency. This choice allows us to compare MHWs statistics over a longer period without relying on reference windows extending beyond the year under study.

For these reasons, the 20-year annually updated baseline provides the most coherent, and temporally inclusive framework for defining MHWs in our analysis. This information has been also added in the text, in section 2.3.

6. Line 150. "different baseline lengths" refers to the length of moving baseline?

It refers to the fixed baseline. However, it was no clear, so we rephrased it: "*Although somewhat subjective, there is general agreement that the baseline period should be at least 30 years in fixed baseline climatology.*"

7. Section 3.1.2. compares MHW detection results from multiple datasets. Please explain why ESACCI was chosen for the main analysis and what advantages it offers relative to the other datasets.

The ESA Climate Change Initiative (ESACCI) dataset was selected because it is specifically designed to provide long-term, stable, and high-quality climate data records for SST. One of the key goals of the ESACCI program is to ensure temporal consistency and accuracy, which are essential for studies focused on long-term variability and trends, such as the present analysis. Additionally, in our comparative assessment, ESACCI SST values tended to fall within the intermediate range among the datasets considered, suggesting a balanced representation that avoids the higher or lower biases observed in some other products. This information has been added to the manuscript now.

8. Line 215. "Wang and Zhou (2004)" typo

Thank you for noting the mistake, it was corrected.

9. Line 231&234. "duration" is the MHW day? MHW day is a cumulative annual metric. Duration is a single-event metric. They are not interchangeable.

It was a mistake, we have now revised the whole document and replace "duration" by "# of days".

10. "prolonged" Since the author's EOF analysis relies on the number of MHW days (a metric determined by the frequency and duration), simply stating that MHWs are "prolonged" is insufficient.

We have now modified the "prolonged" term to number of days.

Response to reviewer #2 (in blue)

General Comment

The manuscript provides a systematic analysis of marine heatwaves (MHWs) and their statistics over the Patagonian Shelf (PS) region, based on satellite-derived daily SST data. This is a very interesting and relevant region to study MHWs in terms of their impacts, given that it is a large and highly productive continental shelf. I encourage the authors to further discuss this aspect. Defining MHWs not only in terms of shifting climatology but also in relation to their ecological and physical impacts is a current and important topic. The impacts are likely to be stronger in shallow areas, particularly when using only surface data.

The manuscript is well structured and well written. The figures are informative and provide a clear descriptive analysis. I also highlight the authors' effort in comparing

different datasets and climatologies. However, I believe that some major issues require further attention.

Thank you for your constructive revision. We believe that the insights and suggestions of literature have greatly improved the scientific quality of our manuscript. All modifications are highlighted in red.

Major Comments

Forcing mechanisms:

There is little effort to investigate or explain the forcing mechanisms driving MHWs in the PS. How does ENSO influence MHWs in this region? Why was the 1998 MHW so intense, and why was it associated with La Niña? When did it start and end? When was its peak, and what was the cumulative intensity? Was it mainly driven by surface heat fluxes or oceanic advection? Understanding these mechanisms is key when studying MHWs—not only describing their spatio-temporal variability but also identifying the processes behind them.

We fully agree that understanding the forcing mechanisms behind MHWs is essential. However, a detailed investigation of the physical drivers was beyond the main scope of this study. Our primary objective was to compare methodologies and datasets, and to provide the first comprehensive description of the occurrence, spatial distribution, and trends of MHWs over this largely unexplored region.

Conducting a full analysis of the drivers in the Patagonian Shelf would require an extensive, dedicated study, given the complexity and variability of the area. This region includes estuarine zones, strong riverine influences, contrasting water masses ranging from subtropical to subpolar, major boundary currents, westerly wind regimes, and pronounced tidal forcing (ranging from microtidal to macrotidal regions). These multiple interacting factors make it challenging to isolate the contribution of any single climate mode without a specific process-oriented approach. In addition, the analysis of each La Niña on different regions require an atmospheric/oceanographic analysis which is not performed in this paper.

However, considering the reviewer's comment, we have revised the manuscript to more clearly integrate the regional physical framework with the analysis of MHWs over the Patagonian Shelf. In particular, Section 2.1 (Regional setting) was reworked to better frame the interpretation of the results, with greater emphasis on physical characteristics that are directly relevant to the interannual variability of MHW occurrence, as well as to the spatial patterns of MHW intensity. The revised section now places increased focus on the influence of shelf bathymetry and tidal dynamics on water-column mixing, together with the role of large-scale air temperature gradients over the PS.

Furthermore, Sections 3.2 and 3.4 were updated to strengthen the physical interpretation of the results. These revisions emphasize the mechanisms driving year-to-year variability in both the number of MHW days and event intensity, while also refining the discussion of spatial intensity patterns. Overall, these changes ensure that the regional oceanographic setting is explicitly embedded in the interpretation of the MHW results, rather than remaining as a standalone descriptive component.

Specifically, we have clarified that the coherence between the Southern Oscillation Index (SOI) and the leading EOF of the annual number of MHW days is weak across all timescales, indicating that ENSO exerts only a limited influence on MHW frequency. We now explicitly discuss that this weak coupling is likely masked by regional and local atmospheric–oceanic processes, as well as mesoscale variability, which dominate the occurrence of MHW days in the study region. In contrast, MHW intensity shows substantially stronger coherence with SOI at interannual timescales (4–5 years), suggesting a more robust ENSO imprint on thermal anomalies. This contrast indicates that distinct mechanisms control MHW frequency and intensity, with large-scale climate variability more effectively modulating the magnitude of thermal anomalies than the frequency of events. We hope these additions clarify the context while keeping the focus on the main objectives of the present work.

Relevance of the Zapiola Anticyclone (ZA):

Why is the ZA included in this manuscript? Isn't the focus supposed to be on the shelf? Its inclusion is confusing, especially for readers who do not know the region in detail, since the dynamics of the ZA are very different from those of the shelf region and are mostly mesoscale eddy-driven. I recommend removing this section from the paper. Otherwise, please restructure the manuscript accordingly (starting with the title).

We acknowledge the reviewer's point that the PS is the central focus of this study, and that including the Zapiola Anticyclone (ZA) region in Section 3.1.1 could potentially divert attention from the main objectives.

The ZA analysis was originally included to illustrate the importance of the MHW detection methodology. In regions such as the ZA or the Mediterranean Sea, where long-term temperature trends are stronger, the use of a fixed versus a moving baseline climatology can produce substantially different MHW statistics. By contrast, over the PS, both approaches yield very similar results, indicating that either method is appropriate depending on the research question.

To maintain a clear focus on the PS, we have relocated the ZA analysis to Appendix B, presenting it as a direct methodological comparison. The main text now

exclusively details the methodological considerations for the PS, while readers interested in exploring methodological sensitivity in a neighboring open-ocean region can refer to the appendix for additional context.

Minor Comments

Some citations about the region are missing. For example:

- Physical Changes in the Patagonian Shelf and references therein: https://link.springer.com/chapter/10.1007/978-3-030-86676-1_3

Thank you for the suggestion and bringing up this study, we have now fully revised it and included in the regional setting section (2.1), which is important to explain the negative SST trend observed in the southern tip of the Patagonian Shelf.

-Line 60: "These features enhance biological productivity, making the Patagonian Shelf (PS) not only one of the most biologically productive regions globally but also a major carbon sink that supports one of the world's most important fisheries."

Please include citations here, for example: https://doi.org/10.1016/j.jmarsys.2017.10.007

We have added the correspondent references to justify the background. "... *Within the SWA, the Patagonian shelf (PS), extending from the southern tip of South America (~55°S) to the Brazil/Malvinas Confluence (~38°S), covers less than 2% of the Southern Ocean's surface but ranks among its most biologically productive regions and largest carbon sinks (e.g. García et al., 2008; Lutz et al., 2010; Bianchi et al., 2005, Bianchi et al., 2009, Kahl et al., 2017; Fig. 1).*"

- Bianchi, A. A., Bianucci, L., Piola, A. R., Pino, D. R., Schloss, I., Poisson, A., & Balestrini, C. F. (2005). Vertical stratification and air-sea CO_2 fluxes in the Patagonian shelf. Journal of Geophysical Research: Oceans, 110(C7).

- Bianchi, A. A., Pino, D. R., Perlender, H. G. I., Osiroff, A. P., Segura, V., Lutz, V., ... & Piola, A. R. (2009). Annual balance and seasonal variability of sea-air CO_2 fluxes in the Patagonia Sea: Their relationship with fronts and chlorophyll distribution. Journal of Geophysical Research: Oceans, 114(C3).

- García, V. M., Garcia, C. A., Mata, M. M., Pollery, R. C., Piola, A. R., Signorini, S. R., ... & Iglesias-Rodriguez, M. D. (2008). Environmental factors controlling the phytoplankton blooms at the Patagonia shelf-break in spring. Deep Sea Research Part I: Oceanographic Research Papers, 55(9), 1150-1166. https://doi.org/10.1016/j.dsr.2008.04.011

- Kahl, L. C., Bianchi, A. A., Osiroff, A. P., Pino, D. R., & Piola, A. R. (2017). Distribution of sea-air CO_2 fluxes in the Patagonian Sea: Seasonal, biological and thermal effects. Continental Shelf Research, 143, 18-28.

- Lutz, V. A., Segura, V., Dogliotti, A. I., Gagliardini, D. A., Bianchi, A. A., & Balestrini, C. F. (2010). Primary production in the Argentine Sea during spring estimated by field and satellite models. Journal of Plankton Research, 32(2), 181-195. https://doi.org/10.1093/plankt/fbp117

-Line 215: Have you considered this paper? It focuses precisely on MHWs and air–sea fluxes in the surroundings of the Río de la Plata:

https://doi.org/10.1029/2018GL081070

We have now incorporated this reference into the introduction and discussion of the interannual variability of MHW events (Section 3.4). This study reinforces the diversity of atmospheric and oceanic processes that can modulate both the intensity and duration of MHWs in our complex study region.

It is important to note, however, that this work focuses primarily on the Río de la Plata region, which represents the northern limit of our study area. A similar situation applies to Rodríguez et al. (2015) and Artana et al. (2024), whose analyses are geographically close but still outside the Patagonian Shelf domain. Nevertheless, the mechanisms highlighted in these studies provide relevant context and support our interpretation of the drivers influencing MHWs variability in the southwestern Atlantic.