nature portfolio

Peer Review File

Seasonal stratification and complex local dynamics control the sub-surface structure of marine heatwaves in Eastern Australia coastal waters

Web links to the author's journal account have been redacted from the decision letters as indicated to maintain confidentiality

12th May 23

Dear Dr Schaeffer,

Your manuscript titled "Seasonal stratification and complex local dynamics control the sub-surface structure of marine heatwaves in coastal waters." has now been seen by 3 reviewers, and I 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.

Specifically, for publication in Communications Earth & Environment to be appropriate, we will need you to include a more detailed description of the methodology, particularly with respect to using different periods to construct the climatology for different depths. In addition, we encourage you to take into account the reviewer's comments.

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.

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

Please use the following link to submit your revised manuscript, point-by-point response to the referees' comments (which should be in a separate document to any cover letter) and the completed checklist:
[link redacted]

** This url links to your confidential home page and associated information about manuscripts you may have submitted or be reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage first **

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.

We understand that due to the current global situation, the time required for revision may be longer than usual. We would appreciate it if you could keep us informed about an estimated timescale for resubmission, to facilitate our planning. Of course, if you are unable to estimate, we are happy to accommodate necessary extensions nevertheless.

Please do not hesitate to contact me 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,

Regina Rodrigues, PhD
Editorial Board Member
Communications Earth & Environment

orcid.org/0000-0001-8010-4018

Heike Langenberg, PhD
Chief Editor
Communications Earth & Environment

EDITORIAL POLICIES AND FORMATTING

We ask that you ensure your manuscript complies with our editorial policies. Please ensure that the following formatting requirements are met, and any checklist relevant to your research is completed and uploaded as a Related Manuscript file type with the revised article.

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Furthermore, please align your manuscript with our format requirements, which are summarized on the following checklist:
Communications Earth & Environment formatting checklist

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.

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All Communications Earth & Environment manuscripts must include a section titled "Data Availability" at the end of the Methods section or main text (if no Methods). More information on this policy, is available at http://www.nature.com/authors/policies/data/data-availability-statements-data-citations.pdf.

In particular, the Data availability statement should include:
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If a community resource is unavailable, data can be submitted to generalist repositories such as <a

href="https://figshare.com/">figshare or Dryad Digital Repository. Please provide a unique identifier for the data (for example a DOI or a permanent URL) in the data availability statement, if possible. If the repository does not provide identifiers, we encourage authors to supply the search terms that will return the data. For data that have been obtained from publically available sources, please provide a URL and the specific data product name in the data availability statement. Data with a DOI should be further cited in the methods reference section.

Please refer to our data policies at http://www.nature.com/authors/policies/availability.html.

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

This study examines extreme and persistent warm condition, known as marine heatwaves (MHWs) in the ocean subsurface at a location along the eastern Australia shelf, where daily subsurface observations are available over a sufficiently long period to allow relatively robust statistical analyses. The analysis shows the existence of three classes of events, including some that are surface intensified, some that extend through the water column, and others that occur in the subsurface with no surface signature. Furthermore, ocean conditions and processes responsible for these different types of events are identified. Based on the events occurred in this region during the period covered by the observations, results indicate that the presence of subsurface signals may be anticipated from the surface anomalies and the ocean conditions. Given that the MHW literature has primarily focused, to date, on sea surface temperature (SST) extremes, this study, which builds on previous investigations by some of the same authors, presents useful and novel methodological approaches for identifying and diagnosing extreme subsurface events. However, the classification of subsurface MHWs presented in this paper, as well as the processes responsible for the different types of events, seem to be specific to this area, and the findings may not be easily generalizable to other locations. For this reason, I would suggest changing the title to something like: ”Seasonal stratification and complex local dynamics control the subsurface structure of marine heatwaves in Eastern Australia coastal waters”.

Also, since the paper identifies different mechanisms for the various MHW classes, some discussion on the expected degree of predictability of these classes based on those mechanisms would be useful.

Overall, the paper is interesting, well-written and novel in its scope. I think that it could make a nice contribution to NCEE.

Specific comments (some of which just small typos):

Lines 22-23 “There has been a large increase in their frequency, duration and intensity across recent decades, which are projected to accelerate in the future”. The authors should briefly acknowledge that this increase could be due to the climate change signal, which may be present in the data used if no form of de-trending was applied.

Lines 37-47. A few other studies have looked at subsurface MHWs in other parts of the world. For example, Scannell et al. (2020; https://doi.org/10.1029/2020GL090548) have examined the subsurface characteristics of MHWs in the Northeast Pacific, and Amaya et al. (2023;

https://doi.org/10.1038/s41467-023-36567-0) have examined bottom heatwaves along the US shelves and their relationship with surface anomalies.

Lines 51-52. What does it mean "reaching the bottom in 30 m"? That these areas are 30-m deep?

Lines 64. I think that the authors mean "complemented" rather than "complimented".

Lines 80-82. Reference [9] presented a different classification of subsurface MHWs in approximately the same region, based on their vertical profiles. That classification should be briefly mentioned here in relation to the current classification.

Caption of Figure 1. The date of the subsurface MHW in the caption (2018-01-10) is different than the one in the figure label (2018-01-17).

Line 109. "anomalies" should be "anomaly".

Caption of Figure 2. I was surprised to see that the definition of Mixed Layer Depth is based exclusively on temperature rather than density. Is salinity not important in this region, so that the two definitions are equivalent? The chosen definition should be justified.

Line 149. Which period is used for the calculation of the anomalies? The total duration of each dataset?

Lines 157-167. After reading this paragraph on subsurface MHWs it is not clear to me what the drivers exactly are. The northward flowing current can be expected to advect colder waters to the region. In addition, there is a heat loss at the surface. Where is the subsurface warming coming from? From the deepened thermocline associated with the downwelling-favorable winds? Although this point is somewhat clarified later in the paper, a sentence summarizing the different processes and stating the likely cause of the subsurface warming should be included here too.

Line 183. "to" is missing before "factors".

Caption of Figure 4, line 2. A parenthesis is missing after "(c, d"

Line 185. Does "JAS" mean "July-August-September"? Why is this 3 months after JFM? These acronyms should be spelled out.

Line 188-189 "23 °Cat" should be "23°C at". Same for "17°C at"

Line 189. "drops of" should be "drops off"

Line 192. I think that "where" should be "are".

Caption of Figure 5. "co-occur" has one extra "r".

Line 241. "isolation" should be "insolation".

Line 244. "enhances" should be "enhance".

Lines 245-246. "between the surface and depth water column" does not seem correct.

Line 248. “as long” should be “as long lasting”.

Line 352. The explanation of the MHW identification is not clear. Are daily anomalies considered for the definition of the MHWs? If so, are the anomalies relative to the daily climatology computed over the “maximum time coverage of each dataset”?

Lines 365-366. I do not understand the definition of subsurface MHWs: “sub-surface MHWs correspond to MHW conditions which persist at all depths >= 40 m but are not in MHW conditions at 15 m and less.” What does it mean that MHW conditions need to persist at all depth greater than 40m, but do not not occur in the top 15 m? Please clarify.

Reviewer #2 (Remarks to the Author):

The writers are providing an innovative analysis on the vertical structure of marine heatwaves using in-situ data. In particular, they assess the sub-surface coherence and physical drivers behind events that occur close to the coast, using a rare daily record of subsurface temperature data off Sydney.

One of the major assets of the paper is the method they used to classify MHWs depending on their structure.In addition, they provide evidence that seasonal stratification, downwelling-favourable winds and ocean currents play a substantial role on the development of events close to the coast.

However, a lot of points in the methodology they used to identify the MHWs in deeper layers are not clear, as well as the correlations they provide between different variables are not properly explained and therefore confuse the reader.
I believe that this is a study with a great potential, which will advance greatly the understanding of MHWs, as well as the physical drivers that modulate their vertical extent, elsewhere in the world.

I recommend publication of this study, after the points that are raised in the attached document are addressed.

Major:

1. Regarding the methodology for detecting MHWs: If the MHWs are detected by applying the algorithm separately at each depth, how can temperature anomalies be connected from one depth to the other? How are the writers sure that the temperature anomalies relative to the 90th percentile at one depth are related to those in the next or previous layers, given that the algorithm requires at least 5 consecutive days for the anomalies (with gaps) in each depth separately?.What is happening when a layer, for example, presents more than 2 gap days but the layer below continues to be in a "mhw state"? Where does the MHW stop then in terms of vertical extent? Defining subsurface MHWs is a similar problem to defining the spatial coverage of MHWs at surface. Connectivity of temperature anomalies can be more easily assumed when examined in one day, but how can the writers be sure that temperature anomalies that are identified separately at each depth are related throughout multiple days? Wouldn't be more appropriate to define the 90th percentile from surface or another layer and then follow the consecutive positive temperature anomalies (without the constraint of the 90th percentile) in the vertical direction in order to decide the vertical extend of the MHW?

Line 108: Could this subsurface MHW be a current which is local to the region or another local oceanographic feature of the area?In other words, is this a "typical" feature of the area rather than an extreme temperature anomaly?

Line 130: When the writers write "*the longest MHWs tend to be the extended events*" what do they mean? How is the duration calculated? As an average throughout all the layers that are part of the MHW or as the maximum duration of an event in one of the layers that are part of the MHW? This is not clear. And the same applies for mean intensity.

Line 177: I am not sure I completely understand the idea behind the '*daily coherence with SST*'. It is not clear to me whether the percentage of surface MHW has to be followed by a number of MHWs in different but consecutive depths (starting from the surface) or if the writers refer to concurrent MHWs at surface and any other deeper layer in the ocean, without a necessary connectivity between depths.

Line 187: It is not clear to me what the writers mean when they say " *43%-57% of surface MHW co-occur with sub-surface MHWs*". This percentage is calculated relative to the sum of MHW days of all the layers that are in a MHW state or not?

Lines 217-218: How can the number of MHW days be correlated with the meridional geostrophic currents? The correlation is in terms of what?Speed?Not clear. I am not sure I understand what does the pearson correlation coefficient correlates in panels of Fig.6d),f),h). Is it a correlation between the timeseries of MHW day counts and timeseries of the equivalent anomalous variables between 1993-2018?Not clear.

Lines 220-221: From what I can understand from plot 6h) there seems to be an anti-correlation between shortwave radiation and...which variable? The MHW day counts, (according to line 221)? Does that mean that more MHW days occur when there is reduced shortwave radiation?These plots need perhaps better explanation to the reader.

Lines 222-223: Once again, I am not sure what kind of correlation is done between latent heat fluxes and geostrophic currents. Given that a simple correlation coefficient does not necessarily imply a causation between two variables, perhaps there is not necessarily a physical meaning behind that correlation.

Line 230: Why do the writers believe that this discrepancy arises between the shortwave radiation and the MHW day counts? Could this be related to the fact that the correlation coefficient does not necessary imply causation between the 2 variables?Or perhaps because, according to the writers, stratified months are related to downwelling-favourable winds, which would not allow the heat due to shortwave radiation , to "accumulate" at surface, rather they would transfer the signal in deeper layers? In addition, the writers refer to enhanced surface mixing and cooling due to turbulent heat fluxes during

these events, which could somehow offset perhaps the heat due to shortwave radiation?It would be useful to give an estimate of the possible mechanisms behind these results.

Lines 224-231: In general, one would think that due to the stratification in strongly stratified months the "signal" of the atmosphere would be stronger at the ocean surface than in deeper layers. The "*decoupling of temperature anomalies between surface and deep water column*" as the writers state in line 245. How do the writers then explain the positive correlations in deeper layers between the variables that are correlated in panels 7d)f)h)?

Lines 265-268: The sentence "*Subsurface MHWs also tend to be most prevalent when the water is more stratified* .." seems to contradict the next sentence "*sub-surface events that warm below and cool above would tend to make the water column less stratified*". Do subsurface events then occur with stratified water column or not? It is a bit confusing these two sentences.

<u>Minor:</u>

Line 9: What does an "*extreme ocean event* " mean? It does not make sense as an expression. Maybe another one with "extreme ocean temperature events" would be a better fit?

Line 56: "MHWs along the coast can often occur **independently** of large-scale off-shelf events.."

Line 57: Instead of "The oceanography.." better to write, "The local oceanographic conditions..".

Figure 1. Caption.: Better to write "Area of interest (black box), " and "The blue dot in each panel indicates the location….". Also instead of "full-line" use "solid-line".

Figure 4:

- Panels d) and f) the box plots are covering the numbers at the left of the plot and in plot d) x axis numbers are overlayed.
- A parenthesis is missing in: "*..extended (c,d)*".
- Which individual events are shown in circles? What is the added value of these circles since the boxplots are provided?
- **Plot 4f** :why does the boxplot depicting the ERA5 winds show 2.3m/s, whereas the x axis shows the standardised anomaly, which inside the text is referred as "*..normalized value of 0.5 standard deviations above the mean..*"?Why isn't the standardised anomaly showed in this boxplot as well, just like the other boxplots?

Line 183: "*..issues related **to** factors*"

Lines 188-189: "… 23 ◦ C at the surface to 17 ◦C at 53 m"

Line 189: " *and the coherence drops of rapidly too below the surface* " What does that mean?

Figures 6 and 7:

- What are the units of the y axis of plots c), e), g)?
- The caption/title "*Correlation with MHW day counts*" is relevant only for the left panels of plots 6 (and 7) d),f)h) or also for the right panels of these plots?

Reviewer #3 (Remarks to the Author):

Main summary
The authors have performed an analysis of what the drivers of marine heatwaves (MHWs) are from the surface to depth in the shallow (~50 m depth) coastal zone. The analysis is primarily based on one in situ sampling station, and supported by physical data (e.g. winds, currents, surface pressure, heat flux) from a nearby Met station. This is all then supported by the spatially closest available pixels for remotely sensed / reanalysis data.

The analysis is certainly sound and important for the literature, but it is by now very familiar, and limited in scope to a single study site. While this research would have been ground-breaking a few years ago (and was when the author(s) first published something similar in 2017+2019), this work now finds itself amongst a rapidly growing body of similar analyses with much broader spatial scopes. Though admittedly the analysis here is still uniquely important in that it is based on in situ data, and not model/reanalysis work. The authors do acknowledge the limited spatial scope of the work, and I think that the way in which they have made all of their concluding arguments keeps in line with that limitation. Meaning that the conclusions presented in this manuscript are supported by the methodology/results.

The methodology itself is sound, with one exception noted below, and contributes to a growing body of literature that analyses the difference in physical drivers of MHWs at the surface vs depth. This paper stands out from those in that it focuses on shallower coastal depths, which I find to be more relevant anyway (at least from an ecological point of view). I also found the figures with which the authors convey the complex results of different drivers at depths, and differences over time/seasons, to be much more clear than what I have seen so far in the literature.

My one issue with the methodology that I would like to see addressed before recommending this work for publication is the use of different climatological time periods for the creation of the 90th percentile thresholds used to detect the MHWs at different depths that are then compared in the results. Most of the other specific comments below are small editorial/typesetting issues.

I think that the time in which these single point analyses are still valid manuscripts for publication is drawing to a close. To keep up with the pace of development in the MHW community, I think that future in situ analyses will need to start to integrate spatial data from ARGOS etc.

Detailed comments

TITLE:
I don't think the authors meant to include a full stop "." at the end of the title. Recommend deleting as this is not standard writing convention.

ABSTRACT:
Ln 9: "ocean events" Consider changing to "seawater events" as a marine heatwave is an event that can occur in any marine environment, not just the oceans (e.g. Mediterranean Sea).
Ln 10: The use of the word "surface" twice in this sentence isn't ideal. But I don't have a helpful recommendation for how to change it...
Ln 11: I'm not sure this sentence is completely accurate any more. In 2017 yes. But in the last couple of years there have been a number of studies using model/reanalysis data to analyse the depth structure of MHWs. I do however agree with the authors' intended purpose of this sentence, which is to say that the in situ understanding of depth structures is still lacking (which I would argue is more important than the model analyses). I recommend that the authors clarify here that this study is

providing direct in situ insight into the depth structure of MHWs, rather than being derived from model outputs, as is primarily the case in the literature.

INTRODUCTION:
Ln 23: "[1] which" -> "[1], which"
Ln 30: "(e.g. 3 decades)" Perhaps cite the Hobday et al. 2016 paper here. Or the WMO paper this point is based on.
Ln 32: "few in situ" Few in situ what? Stations? Time series? A noun is missing here.
Ln 34: "surface observations" I would argue that more and more information is also being derived from model outputs.
Ln 67: I think this paragraph should go before the paragraph starting on Ln 59. This will require some minor re-writing. If the paragraphs are switched, Ln 78 "in this area" will need to explicitly say which area is being referred to.
Lns 87-89: Recommend deleting as I think this is too much of a concluding remark for an intro section. Or rather move it to the discussion.

RESULTS:
Ln 92: I'm not sure this is still a valid argument. There is a lot of modelling work now that shows the depth structure of MHWs.
Ln 97: Here and throughout, it looks like the compiler (LaTeX?) has not applied the matching "" characters correctly. Something to address during the type-setting of the article.
Ln 97: "peters out" I'm happy to see this idiom used here, but consider that a non-native speaker that learned only text-book English may not understand it.
Ln 109: "a cold surface anomalies" Either "a cold surface anomaly" or "cold surface anomalies"
Ln 113: "Overall, over" -> "Overall, during" Or something like that.
Ln 157: Interesting.
Ln 186: "23 ° Cat" Here and elsewhere the spacing of the "°" symbol appears to be off.
Ln 190: That's very interesting.

SUMMARY, DISCUSSION and RECOMMENDATIONS:
Ln 290: Indeed, that is the primary concern of this research. It comes from a single point of sampling. Given the methodology, it is slightly tenuous to say what the role of spatial drivers (i.e. wind and currents) is.

METHODS:
Ln 318: "1min situ" -> "1m in situ"
Lns 354-355: "The maximum time coverage of each dataset is considered..." This methodological choice is technically incorrect. When comparing events derived from a statistical analysis utilising percentiles from a given time period, all time periods must match. Otherwise one is comparing "apples and oranges". The use of different climatology periods when detecting MHWs can have a significant impact on the comparability of the results (see Figure 1 in Schlegel et al. 2019: https://www.frontiersin.org/articles/10.3389/fmars.2019.00737/full). I appreciate that there are issues of time series length between the different data product used in this analysis, but the choice of using different time periods to derive 90th percentile thresholds needs to be argued for much more strongly than it is now. At the very least, the authors should ensure that full years of data are being used for the climatology calculations. I.e. An even greater disturbance to the results may come from including the few extra months from a partial year, rather than just calculating the climatology from Jan-Dec of the final complete year of data. Ideally, the authors should utilise the climatology time period over which all time series from all data products share the same complete years of sampling. Or at least all of the temperature time series. That being said, one does not want to use less than 20 years of data when calculating the 90th percentile threshold.

Lns 362-363: “If 70% of more” -> “If 70% or more”

ACKNOWLEDGEMENTS:
Ln 376: “ASG” -> “A.S.G.”

DATA AVAILABILITY:
Yes, the in situ data are available via the AODN website: https://portal.aodn.org.au/
All of the other websites for the various remote/reanalysis products are also provided.

FIGURE 1:
This figure manages to convey a lot of information very clearly. I like the authors choice to show the period of MHW+category as a solid coloured horizontal bar, rather than as a flame or rug plot. I think it is more clear this way and agree that it isn’t helpful in this context to show the °C values on the y-axis. Note however that the titles for the panels b, e, and f do not appear to have the same justification, font size, or font type. These should be standardised.

FIGURE 2:
“De-seasonalised” How was the seasonal signal removed? Did the missing sub-surface data from 2006 affect the calculation of sub-surface MHW? Looking closely at this figure there appears to be an artefact in the colour of the temperature anomalies at specific isobaths. So the MLD is defined as the shallowest depth at which sub-surface waters are >= 0.5°C of the surface SST on that same day? I don’t think I’ve seen that definition used before. Do the authors have a reference for this methodological choice?

FIGURE 3:
This is an interesting way of presenting this information. I like the use of the purple and green boxes to help indicate to the reader the periods of strong and weak stratification respectively.

FIGURE 4:
Another good figure. I like the continuous colour palette for the boxes and the white text to show the mean values.

FIGURE 5:
These panels would benefit from having black borders, similar to the other figures. Would be better if all text was slightly larger, too. It took me a moment to understand what this figure was showing. But I think it does a good job of conveying this statistical analysis of co-occurrence along the depth gradient.

FIGURE 6+7:
Consider adding black boxes around panels. It may also help to use different colour palettes for the wind, current, and heat flux panels to make more immediately clear that these are different things. Though this could make the figure too colourful. At the authors’ discretion.

FIGURE 8:
Great text-book figure.

FIGURE 9 -> Figure S1:
Which direction is the bias? Is it the mooring that is cooler than the satellite?

FIGURE 10 -> FIGURE S2:
Same question.

References for this review

Schlegel, R. W., Oliver, E. C., Hobday, A. J., & Smit, A. J. (2019). Detecting marine heatwaves with sub-optimal data. Frontiers in Marine Science, 6, 737.

RESPONSE to referees

Reviewer #1 (Remarks to the Author):

This study examines extreme and persistent warm condition, known as marine heatwaves (MHWs) in the ocean subsurface at a location along the eastern Australia shelf, where daily subsurface observations are available over a sufficiently long period to allow relatively robust statistical analyses. The analysis shows the existence of three classes of events, including some that are surface intensified, some that extend through the water column, and others that occur in the subsurface with no surface signature. Furthermore, ocean conditions and processes responsible for these different types of events are identified. Based on the events occurred in this region during the period covered by the observations, results indicate that the presence of subsurface signals may be anticipated from the surface anomalies and the ocean conditions. Given that the MHW literature has primarily focused, to date, on sea surface temperature (SST) extremes, this study, which builds on previous investigations by some of the same authors, presents useful and novel methodological approaches for identifying and diagnosing extreme subsurface events. However, the classification of subsurface MHWs presented in this paper, as well as the processes responsible for the different types of events, seem to be specific to this area, and the findings may not be easily generalizable to other locations. For this reason, I would suggest changing the title to something like: ”Seasonal stratification and complex local dynamics control the subsurface structure of marine heatwaves in Eastern Australia coastal waters”.

Also, since the paper identifies different mechanisms for the various MHW classes, some discussion on the expected degree of predictability of these classes based on those mechanisms would be useful.

Overall, the paper is interesting, well-written and novel in its scope. I think that it could make a nice contribution to NCEE.

We thank the reviewer for their positive and constructive comments. We have modified the title to reflect the local nature of the study. We already had some discussion around predictability, however we have expanded on this.

Our reply to the comments is written in blue below with the line numbers referring to the track-change manuscript.

Specific comments (some of which just small typos):

Lines 22-23 “There has been a large increase in their frequency, duration and intensity across recent decades, which are projected to accelerate in the future”. The authors should briefly acknowledge that this increase could be due to the climate change signal, which may be present in the data used if no form of de-trending was applied.
The text was updated with “primarily due to long-term ocean warming (Oliver, 2019)” lines 24-25.

Oliver, Eric C. J. 'Mean Warming Not Variability Drives Marine Heatwave Trends'. *Climate Dynamics*, 6 March 2019. https://doi.org/10.1007/s00382-019-04707-2.

Lines 37-47. A few other studies have looked at subsurface MHWs in other parts of the world. For example, Scannell et al. (2020; https://doi.org/10.1029/2020GL090548) have examined the subsurface characteristics of MHWs in the Northeast Pacific, and Amaya et al. (2023; https://doi.org/10.1038/s41467-023-36567-0) have examined bottom heatwaves along the US shelves and their relationship with surface anomalies.
We have included these references in the introduction and the discussion. (lines 51, 50, 292-295,302).

Lines 51-52. What does it mean "reaching the bottom in 30 m"? That these areas are 30-m deep?

Yes, it means the areas are 30m deep. The manuscript now reads "reaching the bottom in 30m water depth" lines 58.

Lines 64. I think that the authors mean "complemented" rather than "complimented".
Done

Lines 80-82. Reference [9] presented a different classification of subsurface MHWs in approximately the same region, based on their vertical profiles. That classification should be briefly mentioned here in relation to the current classification.

In [9], it was never called sub-surface MHW, but sub-surface temperature anomalies during MHW, which correspond to our "extended" category. To avoid confusion, the manuscript now reads Elzahaby et al. (2019) used ARGO floats to show that temperature anomalies can extend down to 2000m during some surface-identified MHWs" (lines 42-44).

Caption of Figure 1. The date of the subsurface MHW in the caption (2018-01-10) is different than the one in the figure label (2018-01-17).
The caption was rectified, thanks.

Line 109. "anomalies" should be "anomaly".
Done

Caption of Figure 2. I was surprised to see that the definition of Mixed Layer Depth is based exclusively on temperature rather than density. Is salinity not important in this region, so that the two definitions are equivalent? The chosen definition should be justified.

The mooring design does not include salinity sensors through the water column, so no data is available to compute the density profile. However, in the region, salinity has a negligible influence on density, except for rare extreme river outflow events. Schaeffer et al. (2013) and Schaeffer et al. (2014) compared density estimates assuming constant salinity, to estimates of density computed from

pressure, temperature and salinity from close-by CTDs, and CARS climatology. They found a strong overall agreement.

Added in the text: "(defined as the shallowest depth where the temperature exceeds the SST temperature by more than 0.5°C as salinity is not measured in the water column and has a negligeable effect in the region overall (Schaeffer et al., 2013, 2014)" (caption of figure 2).

Line 149. Which period is used for the calculation of the anomalies? The total duration of each dataset?
Yes, in order to obtain the best climatological estimates using limited record lengths we used the whole period, i.e. not all datasets use the same climatological period. This is now specified in the Methods section "All daily anomalies were computed from day-of-year moving averages (+/- 5 days) over the full duration of each dataset..." (lines 394-395).

More precisely, the *in-situ* temperature >=15 m depth, SST, and ERA reanalysis are all considered over the same time period between January 1992 and December 2019. The satellite altimetry dataset is only available one year later, hence covers 1993-2019. Atmospheric variables from the BoM are taken from 1995-2019 (due to a dubious change of variance in wind speed in 1994). We don't expect a significant difference when comparing a climatology between 26 and 28 years.

The two shorter datasets which are the ADCP measurements covering 2007-2019, and the *in-situ* temperature estimates for depths shallower than 15m which cover January 1992 – May 2006. For these reasons, shallow *in situ* temperature is only used for illustration in figures 2, and in figure 5 for a time-average. None of the MHW analysis uses this short time-series. Similarly, the ADCP data is only used in figure 2 ("current-local") and the relatively small number of events in that time period is specified.

We have added this information in the manuscript: "All daily anomalies were computed from day-of-year moving averages (+/- 5 days) over the whole duration of the datasets (January to December), then smoothed over 31 days (as in Hobday et al., 2016): 1992 to 2019 for satellite and in situ temperature (except for shallow depths, which are only shown for illustration) and ERA5 reanalysis; 1993-2019 for satellite geostrophic currents; 1995-2019 for the BoM meteorological station; 2007-2019 for ADCP current velocities." (lines 394-398).

Lines 157-167. After reading this paragraph on subsurface MHWs it is not clear to me what the drivers exactly are. The northward flowing current can be expected to advect colder waters to the region. In addition, there is a heat loss at the surface. Where is the subsurface warming coming from? From the deepened thermocline associated with the downwelling-favorable winds? Although this point is somewhat clarified later in the paper, a sentence summarizing the different processes and stating the likely cause of the subsurface warming should be included here too.
Yes. We have added: "Hence, the sub-surface warming appears to result from the deepened thermocline associated with the downwelling-favorable winds." (lines 178-179)

Line 183. "to" is missing before "factors".
Done

Caption of Figure 4, line 2. A parenthesis is missing after “(c, d”
Done

Line 185. Does “JAS” mean “July-August-September”? Why is this 3 months after JFM? These acronyms should be spelled out.
Done. Note that we considered that there were 3 months in between, but the reviewer is right in saying that the second period is 6 months after the first one. The manuscript now reads “During weakly stratified months, defined as JAS (July-August-September), 6 months after the strong JFM (January-February-March) stratification.” (lines 206-207)

Line 188-189 ”23 °Cat” should be ”23°C at”. Same for “17°C at”
Done
Line 189. “drops of” should be “drops off”
Done
Line 192. I think that “where” should be “are”.

Done
Caption of Figure 5. “co-occur” has one extra “r”.
Done
Line 241. “isolation” should be “insolation”.
Done
Line 244. “enhances” should be “enhance”.
Done
Lines 245-246. “between the surface and depth water column” does not seem correct.
Corrected. It now reads “between the surface and deeper in the water column”.

Line 248. “as long” should be “as long lasting”.
Done

Line 352. The explanation of the MHW identification is not clear. Are daily anomalies considered for the definition of the MHWs? If so, are the anomalies relative to the daily climatology computed over the “maximum time coverage of each dataset”?
The manuscript has been clarified. “A day-of-the-year climatology of the 90th percentile is computed using the maximum time coverage of the mooring dataset (1992 - 2019) using a moving 10 day window around each day-of-the-year.” (lines 404-407).

“For each depth, the time-series of temperature is used to identify MHWs events independently from other depths.”
Lines 365-366. I do not understand the definition of subsurface MHWs: “sub-surface MHWs correspond to MHW conditions which persist at all depths >= 40 m but are not in MHW conditions at 15 m and less.” What does it mean that MHW conditions need to persist at all depth greater than 40m, but do not not occur in the top 15 m? Please clarify.

The manuscript has been clarified: “Events are classified as a shallow MHW if MHW conditions exist at 0 m from the satellite SST, and 15m from the in situ observations but MHW conditions are not met at all depths ≥40 m. Conversely, a sub-surface MHW occurs when MHW conditions exist at all depths ≥40 m but not at 15 m nor at the surface.” (lines 418-422)

Reviewer #2 (Remarks to the Author):

The writers are providing an innovative analysis on the vertical structure of marine heatwaves using in-situ data. In particular, they assess the sub-surface coherence and physical drivers behind events that occur close to the coast, using a rare daily record of subsurface temperature data off Sydney.

One of the major assets of the paper is the method they used to classify MHWs depending on their structure. In addition, they provide evidence that seasonal stratification, downwelling-favourable winds and ocean currents play a substantial role on the development of events close to the coast.

However, a lot of points in the methodology they used to identify the MHWs in deeper layers are not clear, as well as the correlations they provide between different variables are not properly explained and therefore confuse the reader.
I believe that this is a study with a great potential, which will advance greatly the understanding of MHWs, as well as the physical drivers that modulate their vertical extent, elsewhere in the world.

I recommend publication of this study, after the points that are raised in the attached document are addressed.

We thank the reviewer for these encouraging comments. We have gone through the text in detail and clarified the sections of concern.

Our reply to the comments is written in blue below with the line numbers referring to the track-change manuscript.

Copied from the PDF file.

Major:

1. Regarding the methodology for detecting MHWs: If the MHWs are detected by applying the algorithm separately at each depth, how can temperature anomalies be connected from one depth to the other? How are the writers sure that the temperature anomalies relative to the 90th percentile at one depth are related to those in the next or previous layers, given that the algorithm requires at least 5 consecutive days for the anomalies (with gaps) in each depth separately?. What is happening when a layer, for example, presents more than 2 gap days but the layer below continues to be in a “mhw state”?

The reviewer has understood correctly, we treat each depth level independently. Our analysis then examines if there is coherence across different depth levels. The connection between temperature anomalies from one depth to another is simply a consequence of the connection of temperature over depths, so it depends on the vertical correlation spatial scale. If the anomaly from the 90th percentile lasts 5 days or more at one depth (let’s says depth1), but only 3 days at depth2, then we consider that the MHW is only present at depth1.

Where does the MHW stop then in terms of vertical extent? Defining subsurface MHWs is a similar problem to defining the spatial coverage of MHWs at surface. Connectivity of temperature anomalies can be more easily assumed when examined in one day, but how can the writers be sure that temperature anomalies that are identified separately at each depth are related throughout multiple days? Wouldn't be more appropriate to define the 90th percentile from surface or another layer and then follow the consecutive positive temperature anomalies (without the constraint of the 90th percentile) in the vertical direction in order to decide the vertical extend of the MHW?

Given that we are focused on extreme conditions, just following connected temperature anomalies contiguous to a subsurface MHW wouldn't really work. In addition, we would also need to decide which layer to focus on. However, we acknowledge that identifying MHW events that extend horizontally and vertically is a challenge and new methods are being proposed in the literature (e.g. Sun, D., Jing, Z., Li, F. & Wu, L. Characterizing global marine heatwaves under a spatio-temporal framework. *Prog. Oceanogr.* **211**, 102947 (2023) or https://ocetrac.readthedocs.io/en/latest/index.html). We feel that the method used here is a good starting point and has the advantage of not depending on parametrised length-scales like in the cited methods.

Line 108: Could this subsurface MHW be a current which is local to the region or another local oceanographic feature of the area? In other words, is this a "typical" feature of the area rather than an extreme temperature anomaly?

Yes, this type of subsurface event occurs many times in the record. There are a number of possible mechanisms including currents that might cause such events. However, our analysis later in the manuscript suggests that the dominant process for subsurface events in this region relates to downward movement of isotherms associated with local winds.

Line 130: When the writers write "the longest MHWs tend to be the extended events" what do they mean? How is the duration calculated? As an average throughout all the layers that are part of the MHW or as the maximum duration of an event in one of the layers that are part of the MHW? This is not clear. And the same applies for mean intensity.

This is explained in the methods section: "The mean intensity, duration and category of events are then extracted from the MHW events at three layers (15m, 30m or 53m), which represent the majority of shallow, extended, or sub-surface conditions, respectively." (lines 423-425).

Line 177: I am not sure I completely understand the idea behind the 'daily coherence with SST'. It is not clear to me whether the percentage of surface MHW has to be followed by a number of MHWs in different but consecutive depths (starting from the surface) or if the writers refer to concurrent MHWs at surface and any other deeper layer in the ocean, without a necessary connectivity between depths.

The connectivity between depths is not considered. This is now clarified in the manuscript: "Note that each depth is compared to the surface, independently to other depths (hence there is not necessarily a vertical continuity)." (lines 198-199)

Line 187: It is not clear to me what the writers mean when they say " 43%-57% of surface MHW cooccur with sub-surface MHWs". This percentage is calculated relative to the sum of MHW days of all the layers that are in a MHW state or not?

Yes, the percentage is calculated relative to the number of MHWs at the surface. The manuscript is clarified: “Between 43-57% of surface MHWs days co-occur with MHW days which were identified in the different depth layers between 15m and 53m.” (lines 208-210)

Lines 217-218: How can the number of MHW days be correlated with the meridional geostrophic currents? The correlation is in terms of what?Speed?Not clear. I am not sure I understand what does the pearson correlation coefficient correlates in panels of Fig.6d),f),h). Is it a correlation between the timeseries of MHW day counts and timeseries of the equivalent anomalous variables between 19932018? Not clear.

By “meridional geostrophic current”, we mean the projected component of geostrophic current (usually referred to as “v”). We have changed it to “meridional geostrophic velocity”.

Yes, the reviewer’s understanding is accurate we are correlating annual data. We have clarified the caption of the figure. “For instance, in panel h), the bar at depth “40m” for panel QSW, shows a Pearson correlation between the number of MHW days per year at 40m (shown in brown in panel a) and the annually averaged short-wave heat-flux anomalies (shown in dark blue in panel g) over the 1995-2019 period (25 data points).” (caption of figure 6)

Lines 220-221: From what I can understand from plot 6h) there seems to be an anti-correlation between shortwave radiation and...which variable? The MHW day counts, (according to line 221)? Does that mean that more MHW days occur when there is reduced shortwave radiation? These plots need perhaps better explanation to the reader.

The plot in figure 6h indeed shows a significant anti-correlation between annually averaged shortwave radiation and annual number of MHW days based on SST (see the caption) during weakly stratified months. This would mean a link between increased MHW day counts and negative anomalies of shortwave radiation, which is indeed counter intuitive. It means that some other mechanism is driving the MHWs. For example, it could be that the anomalous southward geostrophic current (the EAC) which modulates MHWs, is linked to increased cloud coverage for some reason. It should also be kept in mind that QSW is derived from a reanalysis, while other variables are observed.

For overall clarity, we hope that the addition in the caption (see previous comment) is suitable.

Lines 222-223: Once again, I am not sure what kind of correlation is done between latent heat fluxes and geostrophic currents. Given that a simple correlation coefficient does not necessarily imply a causation between two variables, perhaps there is not necessarily a physical meaning behind that correlation.

Please see next reply.

Line 230: Why do the writers believe that this discrepancy arises between the shortwave radiation and the MHW day counts? Could this be related to the fact that the correlation coefficient does not necessary imply causation between the 2 variables?Or perhaps because, according to the writers, stratified months are related to downwelling-favourable winds, which would not allow the heat due to shortwave radiation , to “accumulate” at surface, rather they would transfer the signal in deeper layers? In addition, the writers refer to enhanced surface mixing and cooling due to turbulent heat fluxes duringthese events, which could somehow offset perhaps the heat due to shortwave radiation?It would be useful to give an estimate of the possible mechanisms behind these results.

These are all interesting and plausible explanations. Also, the temporal scale between "event-based" MHWs (figure 5b) and the MHW counts over 25 years (figure 7h) is very different. It could be that positive QSW anomalies act to drive surface MHWs at the beginning of the events, but do not stand out when averaged over 3 months every year. Given that all these explanations are highly speculative and are based on a link between an observational and a modelling product, we choose not to add a long discussion in the manuscript. In the future, it would be interesting to investigate this using model output only, focusing on the dynamics between the fluxes and MHW days in the model without necessarily trying to match the timing of the observed events. This is, however, outside of the scope of this paper.

The manuscript now reads "We found some counter-intuitive relationships that were statistically significant in Fig.6g-h (e.g. MHW days counts and reduced short-wave radiation at the surface, or MHW day counts and increased latent heat fluxes in ERA5 reanalysis).These may be spurious relationships related to data issues, or might result from some additional process affecting both of the variables." (lines 246-250).

Lines 224-231: In general, one would think that due to the stratification in strongly stratified months the "signal" of the atmosphere would be stronger at the ocean surface than in deeper layers. The "decoupling of temperature anomalies between surface and deep water column" as the writers state in line 245. How do the writers then explain the positive correlations in deeper layers between the variables that are correlated in panels 7d)f)h)?

The correlations in deeper layers in figure 7h are weak, so we suspect that these may be spurious and not an indication of a causal mechanism. In figure 7d, the correlations between along-shelf winds and MHW days counts in deeper layers are expected since dowelling processes mostly act to warm deeper layers where the vertical temperature gradient is largest, as shown in the schematics.

Lines 265-268: The sentence "Subsurface MHWs also tend to be most prevalent when the water is more stratified .." seems to contradict the next sentence "sub-surface events that warm below and cool above would tend to make the water column less stratified". Do subsurface events then occur with stratified water column or not? It is a bit confusing these two sentences.

The fact that it gets slightly warmer below and cooler above would decrease the stratification, but that doesn't mean that it would break down the strong seasonal stratification, it would just tend to weaken. We have now specified "Unlike surface events, sub-surface events that warm below and cool above would tend to make the water column less stratified **which could explain their shorter duration."** (lines 305-306).

Minor:

Line 9: What does an "extreme ocean event " mean? It does not make sense as an expression. Maybe another one with "extreme ocean temperature events" would be a better fit?

The reviewer is right. The manuscript was modified to "extreme seawater temperature events" since reviewer 1 pointed out that MHWs can be in seas, not only oceans.

Line 56: "MHWs along the coast can often occur independently of large-scale off-shelf events.."

Done

Line 57: Instead of “The oceanography..” better to write, “The local oceanographic conditions..”.

Done

Figure 1. Caption.: Better to write “Area of interest (black box), ” and “The blue dot in each panel indicates the location….”. Also instead of “full-line” use “solid-line”.

Done

Figure 4: - Panels d) and f) the box plots are covering the numbers at the left of the plot and in plot d) x axis numbers are overlayed. - A parenthesis is missing in: “..extended (c,d)”. - Which individual events are shown in circles? What is the added value of these circles since the boxplots are provided?-

Thanks, we moved the covered numbers. The circles allow a more precise visulalisation and we feel like it does not deter from the main message of the figure.

The added values of the circles allow to visually estimate the number of events the boxplots are based on.

Plot 4f :why does the boxplot depicting the ERA5 winds show 2.3m/s, whereas the x axis shows the standardised anomaly, which inside the text is referred as “..normalized value of 0.5 standard deviations above the mean..”?Why isn’t the standardised anomaly showed in this boxplot as well, just like the other boxplots?

We have gone back and forth between showing the standardized or raw anomalies for ERA5 variables. In the end, we chose to show non standardized anomalies in the panel because it allows to compare the relative influence of the fluxes in W/m2. The text includes both for clarity.

Line 183: “..issues related to factors”

Done

Lines 188-189: “… 23 ◦ C at the surface to 17 ◦C at 53 m” Line 189: “ and the coherence drops of rapidly too below the surface “ What does that mean?

The manuscript was clarified “and the coherence also drops rapidly with depth (purple in Fig. 5b). Indeed, the co-occurrence between surface SST MHW days and deep MHW days is only 6% at 53m depth.” (lines 211-213)

Figures 6 and 7: - What are the units of the y axis of plots c), e), g)?- The caption/title “Correlation with MHW day counts” is relevant only for the left panels of plots 6 (and 7) d),f)h) or also for the right panels of these plots?

Thanks for pointing it out. The units are now present, and we have moved the label to the center for clarity since it is relevant for the left and right axes.

Reviewer #3 (Remarks to the Author):

Main summary
The authors have performed an analysis of what the drivers of marine heatwaves (MHWs) are from the surface to depth in the shallow (~50 m depth) coastal zone. The analysis is primarily based on one in situ sampling station, and supported by physical data (e.g. winds, currents, surface pressure, heat flux) from a nearby Met station. This is all then supported by the spatially closest available pixels for remotely sensed / reanalysis data.

The analysis is certainly sound and important for the literature, but it is by now very familiar, and limited in scope to a single study site. While this research would have been ground-breaking a few years ago (and was when the author(s) first published something similar in 2017+2019), this work now finds itself amongst a rapidly growing body of similar analyses with much broader spatial scopes. Though admittedly the analysis here is still uniquely important in that it is based on in situ data, and not model/reanalysis work. The authors do acknowledge the limited spatial scope of the work, and I think that the way in which they have made all of their concluding arguments keeps in line with that limitation. Meaning that the conclusions presented in this manuscript are supported by the methodology/results.

The methodology itself is sound, with one exception noted below, and contributes to a growing body of literature that analyses the difference in physical drivers of MHWs at the surface vs depth. This paper stands out from those in that it focuses on shallower coastal depths, which I find to be more relevant anyway (at least from an ecological point of view). I also found the figures with which the authors convey the complex results of different drivers at depths, and differences over time/seasons, to be much more clear than what I have seen so far in the literature.

My one issue with the methodology that I would like to see addressed before recommending this work for publication is the use of different climatological time periods for the creation of the 90th percentile thresholds used to detect the MHWs at different depths that are then compared in the results. Most of the other specific comments below are small editorial/typesetting issues.
I think that the time in which these single point analyses are still valid manuscripts for publication is drawing to a close. To keep up with the pace of development in the MHW community, I think that future in situ analyses will need to start to integrate spatial data from ARGOS etc.

Thank you for the enthusiastic and detailed comments. Our reply to the comments is written in blue below with the line numbers referring to the track-change manuscript.

We apologize for the confusion about the methodology. The climatological time-period to detect MHWs at different depths is the same: 1992-2019 for in situ temperature >=15m and satellite SST. The only different period is for in situ temperature shallower than 15m. Unfortunately, these depth levels were only measured until 2006. For this reason, in situ depth less than 15m were excluded from the MHW

analysis and are only used for illustration in figures 1 and 2, and in figure 5 for a time-average. We hope that this addresses the reviewer's concerns.

We agree that knowledge on subsurface MHWs is increasing quickly. However, even if models and computation techniques are more and more powerful and accurate, the lack of **long-term** in-situ sub-surface observations is going to be a limitation for model evaluation and process understanding for some time to come. In this study we are exploiting an almost unique subsurface dataset, we feel it is vital to extract as much information as possible from such unprecedented observational records.

Unfortunately, ARGO floats do not move from the open ocean onto the continental shelves, and have much lower temporal resolution, thus making long term on-shelf mooring observations of particular value. While we used Argo floats to investigate the propagation of temperature anomalies at depth during surface MHWs in the deep ocean (Elzahaby et al., 2018), there is no daily climatology from observations to use to identify sub-surface MHWs from ARGO floats.

Detailed comments

TITLE:
I don't think the authors meant to include a full stop "." at the end of the title. Recommend deleting as this is not standard writing convention.
Done

ABSTRACT:
Ln 9: "ocean events" Consider changing to "seawater events" as a marine heatwave is an event that can occur in any marine environment, not just the oceans (e.g. Mediterranean Sea).
Done
Ln 10: The use of the word "surface" twice in this sentence isn't ideal. But I don't have a helpful recommendation for how to change it...
The sentence now reads "The extent of the ecological damage depends not only on the easily observed surface signature but on the MHW structure at depth." (lines 11-13)

Ln 11: I'm not sure this sentence is completely accurate any more. In 2017 yes. But in the last couple of years there have been a number of studies using model/reanalysis data to analyse the depth structure of MHWs. I do however agree with the authors' intended purpose of this sentence, which is to say that the in situ understanding of depth structures is still lacking (which I would argue is more important than the model analyses). I recommend that the authors clarify here that this study is providing direct in situ insight into the depth structure of MHWs, rather than being derived from model outputs, as is primarily the case in the literature.
The sentence was rewritten "Few studies have systematically looked at sub-surface MHWs, and even fewer have provided direct in situ insight into the depth structure of MHWs rather than being derived from model outputs." (lines 40-42). The references to Amaya et al (2023) and Scannel et al. (2020) were also added to the paragraph. (lines 51, 50, 292-295,302)

INTRODUCTION:
Ln 23: "[1] which" -> "[1], which"

Done

Ln 30: “(e.g. 3 decades)” Perhaps cite the Hobday et al. 2016 paper here. Or the WMO paper this point is based on.
We cited Hobday et al. (2016) and Schlegel et al. (2019) lines 33.

Ln 32: “few in situ” Few in situ what? Stations? Time series? A noun is missing here.
Sorry. It should read “few in situ platforms”.

Ln 34: “surface observations” I would argue that more and more information is also being derived from model outputs.
Good point. The sentence now reads “Therefore, most knowledge of MHWs are derived from surface observations and modelling outputs, and little is known about the occurrence or co-occurrence of events at depth.” (lines 36-38)

Ln 67: I think this paragraph should go before the paragraph starting on Ln 59. This will require some minor re-writing. If the paragraphs are switched, Ln 78 “in this area” will need to explicitly say which area is being referred to.
Done, thanks for the suggestion.

Lns 87-89: Recommend deleting as I think this is too much of a concluding remark for an intro section. Or rather move it to the discussion.
Done. The sentence has been moved to the discussion section.

RESULTS:
Ln 92: I’m not sure this is still a valid argument. There is a lot of modelling work now that shows the depth structure of MHWs.
Changed to “Observational evidence”. We agree that there is a growing number of modelling studies, but we argue that modelling results depend on the resolution and simulation of relevant processes. Unfortunately, sub-surface stratification, air-sea exchanges and bottom dynamics are often poorly represented. We argue that there is still value in observing processes (keeping in mind uncertainties and limitations of each dataset).

Ln 97: Here and throughout, it looks like the compiler (LaTeX?) has not applied the matching “” characters correctly. Something to address during the type-setting of the article.
The reviewer is right, this has been fixed now.

Ln 97: “peters out” I’m happy to see this idiom used here, but consider that a non-native speaker that learned only text-book English may not understand it.
Changed to “fades away”.

Ln 109: “a cold surface anomalies” Either “a cold surface anomaly” or “cold surface anomalies”
Done

Ln 113: “Overall, over” -> “Overall, during” Or something like that.
Done

Ln 157: Interesting.
Thanks, we found it interesting too.

Ln 186: “23 ° Cat” Here and elsewhere the spacing of the “°” symbol appears to be off.
Done

Ln 190: That’s very interesting.
Thanks, glad the results are of interest. We also expected more coherence over this relatively shallow depth.

SUMMARY, DISCUSSION and RECOMMENDATIONS:
Ln 290: Indeed, that is the primary concern of this research. It comes from a single point of sampling. Given the methodology, it is slightly tenuous to say what the role of spatial drivers (i.e. wind and currents) is.
This location is impacted by typical coastal dynamics, moderate freshwater inflow, moderate wind forcing, a medium shelf width, moderate tidal flow, and an offshore faster flowing WBC that can impact the mid- outer shelf. The dominant dynamics could be observed on many continental shelf regions globally. This case study shows the impact of different dynamics through the water column, and highlights the need to look below the surface. For this reason, we are confident that these results are useful and applicable in other regions, even if not directly analogous.
We added the following to the manuscript. “Given the drivers and prevailing conditions described at this mooring occur in other locations, it seems quite plausible that similar processes may operate in other locations. In particular, they may be more generally applicable to coastal regions which experience regular stratification (from seasonal heating or freshwater outputs), and prevailing downwelling-favourable winds. Unfortunately, long-term sub-surface observations are rare in other locations.” (lines 335)

METHODS:
Ln 318: “1min situ” -> “1m in situ”
Done.

Lns 354-355: “The maximum time coverage of each dataset is considered...” This methodological choice is technically incorrect. When comparing events derived from a statistical analysis utilising percentiles from a given time period, all time periods must match. Otherwise one is comparing “apples and oranges”. The use of different climatology periods when detecting MHWs can have a significant impact on the comparability of the results (see Figure 1 in Schlegel et al. 2019: https://www.frontiersin.org/articles/10.3389/fmars.2019.00737/full). I appreciate that there are issues of time series length between the different data product used in this analysis, but the choice of using different time periods to derive 90th percentile thresholds needs to be argued for much more strongly than it is now. At the very least, the authors should ensure that full years of data are being used for the

climatology calculations. I.e. An even greater disturbance to the results may come from including the few extra months from a partial year, rather than just calculating the climatology from Jan-Dec of the final complete year of data. Ideally, the authors should utilise the climatology time period over which all time series from all data products share the same complete years of sampling. Or at least all of the temperature time series. That being said, one does not want to use less than 20 years of data when calculating the 90th percentile threshold.
Apologies for the confusion, we have now clarified the text. “All daily anomalies were computed from day-of-year moving averages (+/- 5 days) over the whole duration of the datasets (January to December), then smoothed over 31 days (as in \cite{Hobday2016}): 1992 to 2019 for satellite and in situ temperature (except for shallow depths, which are only shown for illustration) and ERA5 reanalysis; 1993-2019 for satellite geostrophic currents; 1995-2019 for the BoM meteorological station; 2007-2019 for ADCP current velocities.” (lines 384-398)

The analysis based on surface and subsurface temperature use the same climatological period (1992-2019). As the reviewer correctly points out this consistency is very important. Where the in situ data does not extend from 1992-2019 (in the upper ocean), we only use it for illustrative purposes.

Lns 362-363: “If 70% of more” -> “If 70% or more”
Done

ACKNOWLEDGEMENTS:
Ln 376: “ASG” -> “A.S.G.”
Done

DATA AVAILABILITY:
Yes, the in situ data are available via the AODN website: https://portal.aodn.org.au/
All of the other websites for the various remote/reanalysis products are also provided.

FIGURE 1:
This figure manages to convey a lot of information very clearly. I like the authors choice to show the period of MHW+category as a solid coloured horizontal bar, rather than as a flame or rug plot. I think it is more clear this way and agree that it isn’t helpful in this context to show the °C values on the y-axis. Note however that the titles for the panels b, e, and f do not appear to have the same justification, font size, or font type. These should be standardised.
Done.

FIGURE 2:
“De-seasonalised” How was the seasonal signal removed? Did the missing sub-surface data from 2006 affect the calculation of sub-surface MHW? Looking closely at this figure there appears to be an artefact in the colour of the temperature anomalies at specific isobaths. So the MLD is defined as the shallowest depth at which sub-surface waters are >= 0.5°C of the surface SST on that same day? I don’t think I’ve seen that definition used before. Do the authors have a reference for this methodological choice?

The seasonal signal was removed using day-of-year climatological means (with a +/-5 days window, then smoothed over 31 days). This is specified in the Methods section.

The missing sub-surface data from 2006 affects the calculation of the climatology and anomalies, but we feel that it is fine as it is only visual.
The MLD criteria is only based on temperature (since there are no profiles of salinity). The threshold 0.5°C is somewhat subjective but was used in Levitus climatology (*Monterey and Levitus* [1997] together with a density criterium), or alone in many other studies cited in de Boyer Montégut et al., 2004 (table 1). This is now cited in the caption (lines 31).

de Boyer Montégut, C., Madec, G., Fischer, A. S., Lazar, A., and Iudicone, D. (2004), Mixed layer depth over the global ocean: An examination of profile data and a profile-based climatology, *J. Geophys. Res.*, 109, C12003, doi:10.1029/2004JC002378

FIGURE 3:
This is an interesting way of presenting this information. I like the use of the purple and green boxes to help indicate to the reader the periods of strong and weak stratification respectively.
Thank you. It is good to know that the details are useful.

FIGURE 4:
Another good figure. I like the continuous colour palette for the boxes and the white text to show the mean values.
Thank you.

FIGURE 5:
These panels would benefit from having black borders, similar to the other figures. Would be better if all text was slightly larger, too. It took me a moment to understand what this figure was showing. But I think it does a good job of conveying this statistical analysis of co-occurrence along the depth gradient.
Done

FIGURE 6+7:
Consider adding black boxes around panels. It may also help to use different colour palettes for the wind, current, and heat flux panels to make more immediately clear that these are different things. Though this could make the figure too colourful. At the authors' discretion.
We have added black boxes around the panels but found that using different colours did not help.

FIGURE 8:
Great text-book figure.
Thank you!

FIGURE 9 -> Figure S1:
Which direction is the bias? Is it the mooring that is cooler than the satellite?
Good point. The caption now reads: "The negative bias shows a slight overestimation of the in-situ temperature compared to the SST."

FIGURE 10 -> FIGURE S2:
Same question.

Good point. The manuscript now reads: “The bias considers the variable on the y-axis minus the variable on the x-axis.“ (caption of figure 10)

References for this review

Schlegel, R. W., Oliver, E. C., Hobday, A. J., & Smit, A. J. (2019). Detecting marine heatwaves with sub-optimal data. Frontiers in Marine Science, 6, 737.
The reference was added.

3rd Jul 23

Dear Dr Schaeffer,

Thank you for submitting your revised manuscript titled "Seasonal stratification and complex local dynamics control the sub-surface structure of marine heatwaves in Eastern Australia coastal waters." In light of your responses to the reviewer comments, we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment under the open access CC BY license (Creative Commons Attribution v4.0 International License).

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Editorial Board Member
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