

Reviewer 1

Review of Mawren et al. "Gliding through marine heatwaves: Subsurface biogeochemical characteristics on the Australian continental shelf"

Marine heatwaves (MHWs) are known to influence oxygen and biological productivity in the water column, but the majority of studies investigating them focus solely on the surface. This study addresses this important gap by analysing 16 years of glider observations on the Australian continental shelf across four contrasting coastal regions.

The authors show that summer surface marine heatwaves had a shallower mixed layer depth and enhanced stratification, confining warming to the upper ocean, whereas in other seasons there is deeper penetration under weakly stratified conditions. The study discusses the impact this has on biogeochemical variables (chlorophyll fluorescence and dissolved oxygen) across contrasting regions and also seasonally. By incorporating rigorous analysis and investigation of the vertical profiles, this study achieves the aim of showing the impact of MHWs goes beyond the surface. The authors have done a good job of summarising a large, multi-regional dataset and presenting a coherent analysis across regions and variables.

Overall, this study fits well within the scope of *Ocean Science* and is well aligned with the glider special issue theme and I recommend it for publication after the authors address specific comments below:

Response: We thank the reviewer for the positive comments and detailed review with great insights.

General comments:

In situ does not need to be italicised. (See English Guidelines in Submission on the Ocean Science webpage).

Response: These have now been changed throughout the manuscript.

There is some inconsistency in spelling conventions, with a mix of Oxford and British spelling (e.g., categorized vs characterised). Either convention is acceptable, but consistency throughout the manuscript is required. (See English Guidelines in Submission on the Ocean Science webpage).

Response: Thank you for pointing this out. We have carefully proofread the manuscript to ensure consistency.

Please ensure consistency in the use of abbreviations: define them on the first use only, and use the abbreviated form thereafter. Whilst I have highlighted a few in my specific comments, it may be worth checking through them all.

Response: Thank you, we revised the abbreviations.

Within the Results and Discussion, the manuscript refers to different areas within the defined regions such as the 'continental shelf' and 'inner shelf'. For clarity, it would be helpful to explicitly define these terms (e.g. by approximate isobaths) or annotate them in Fig. 1, particularly for readers unfamiliar with the regional shelf structure.

Response: We have reviewed our terminology for defined regions. While Figure 1 includes isobaths and glider profiles, the continental shelf is narrow in areas such that additional isobaths would be masked by the glider profiles. We have rephrased Figure 1 caption to clarify that glider profiles were over the continental shelf and shallower than 200 m.

I have a query regarding the calculated of the MHW severity. It is done using the surface based satellite data, but as the authors have pointed out, MHWs can show stronger signals in the subsurface compared to the surface. Would it be possible to justify the use of the SST data only (whether this is quality or sample size) and discuss the limitations.

Response: We thank the reviewer for this comment. We emphasize that the objective of this study is not to identify subsurface MHWs, but rather to examine the vertical structure associated with surface MHWs. We used satellite SST as it provides sufficient daily temporal resolution and long time series required to compute a climatology and percentile-based thresholds (which is impossible with gliders), as well as a high spatial resolution over the continental shelf. We acknowledge the limitations which are discussed in the manuscript at lines 778-780.

The use of surface MHW detection is important in this context to ensure consistency with existing MHW studies and to allow intercomparison across regions and datasets, particularly given our a focus on the biogeochemical response to surface MHWs on the Australian continental shelf.

Specific comments:

Introduction

Lines 63 – 65: Consider explicitly linking MHW characteristics to stratification here. This relationship recurs throughout the manuscript and may benefit from being introduced earlier.

Response: We thank the reviewer for this suggestion. In the Introduction at lines 65-71, we have included several sentences to introduce the role of stratification in controlling MHW characteristics with supporting references.

Line 71: The emphasis on coastal areas could be clarified. I appreciate the next paragraph addresses this, but as one of the examples of subsurface MHWs is in the North Pacific, the coastal comment here lacks context and the importance of coastal regions could be emphasised here.

Response: We agree with the reviewer and have added the importance of coastal regions by highlighting how strong stratification, complex circulation and shallow bathymetry can amplify subsurface temperature anomalies. Please see lines 75-79 in the manuscript.

Lines 85-87: This is where it is important to understand the link between MHWs and stratification.

Response: We have strengthened this section by linking MHW to stratification through mixed layer dynamics, vertical mixing and thermocline variability in modifying heat storage at lines 69-71.

Line 111: Please indicate the temporal limits of glider coverage and comment on how this sampling frequency relates to the duration of extreme events, particularly MHWs.

Response: Unlike a fixed mooring which provides a continuous time series at a single location, gliders offer flexible deployments: seagliders can operate for several months at a time while slocum gliders typically conduct missions lasting 3-5 weeks (see line 185). Consequently, glider sampling is intermittent and extreme events such as MHWs may not always be fully captured. Due to this limitation, we do not attempt to quantify the full duration of MHWs. Instead the analysis focuses on the localised impact of these events on the vertical physical and biogeochemical structure, which can be robustly assessed using these glider profiles.

Lines 113 – 114: Sensors on gliders do not directly measure stratification or phytoplankton, it may be better to list the variables they do measure (e.g. temperature, salinity, oxygen, chlorophyll fluorescence) and then indicate that from these stratification and phytoplankton dynamics can be inferred.

Response: We have retained the sentence stating the variables measured by gliders at lines 119-121 and lines 123-124 stating that they can be used to infer stratification and phytoplankton dynamics.

Lines 117 – 120: Were any of the cited studies specifically focused on MHWs? If so, please clarify. Additionally, please define what is meant by “short-term” (e.g. hours, days).

Response: To our knowledge, only a limited number of studies have explicitly examined MHWs using glider observations around Australia. We have cited the relevant study by Benthuisen et al. (2018) who captured a specific MHW off northeastern Australia and have removed short-term as the measurements from IMOS are continual monitoring aimed at capturing mean conditions over specific regions. Please see lines 131-134.

Methods

Line 151: Extra space between “is” and “used”.

Response: Corrected in the revised manuscript.

Line 163 – 164: IMOS has already been defined on line 123

Response: This has been modified accordingly.

Line 194 – 197: What was the quenching depth? Is it worth retaining daytime measurements from this depth and below to limit the removal of valuable data? Particularly given several hypotheses rely on chlorophyll interpretation.

Response: We thank the reviewer for this comment. A fixed quenching depth was not defined as quenching varies with different factors such as light conditions, stratification, and mixed layer depth particularly over shallow continental shelf regions. Given our focus being on bathymetry ~ 40-100m depth depending on the region, we used a more conservative approach and retained only night-time chlorophyll observations, following Schaeffer et al., (2016b), rather than applying an uncertain depth based on daytime data.

Line 202: Consider using “few” instead of “rare”.

Response: Corrected accordingly.

Line 217: There appears to be a typesetting issue here and the link doesn't work.

Response: We have included a reference for the Best Practice Manual at line 203.

Line 229 – 230: Is excluding negative seasonal anomalies standard practise when identifying MHWs? Please clarify whether this refers to identifying events or calculating the severity index.

Response: The MHW identification followed the standard methodology by Sen Gupta et al. (2020), based on severity >1, without excluding negative anomalies. Previously, we only retained temperature anomalies above the seasonal mean to highlight the warm anomalies linked to MHW conditions when calculating the averaged profiles in Figures 6-9. Although results were maintained, we recomputed the averaged profiles using all available MHW profiles (Figs. 6-9), irrespective of whether some profiles fell below the seasonal mean to keep methodological consistency.

Line 238: Add “defined” before “as”

Response: Corrected accordingly.

Line 328: Potentially an extra space between “sampled” and “off”.

Response: Corrected in the revised manuscript.

Lines 334 – 341: Where possible, figure captions should appear on the same page as the figure.

Response: Corrected in the revised text.

Results

Lines 369 – 370: How do you define the continental shelf and offshore waters? Also, clarify the area spoken about here is in the QLD region (if I have understood correctly).

Response: We define the continental shelf as waters shallower than 200m, following the United Nations Convention on the Law of the Sea (UNCLOS). Offshore waters are thus deeper than 200m. The 200m continental shelf boundary is now mentioned in the updated Figure 1 caption.

Lines 370 – 372: Not sure what is meant here by “In agreement with the higher frequency”, please clarify the link between frequency and duration.

Response: We have removed “In agreement with the higher frequency”.

Line 372: GBR has not yet been defined.

Response: Thank you for pointing this out. A thorough check has been performed to make sure all abbreviations are defined at first use.

Lines 379 – 381: It could be helpful to briefly clarify what types of quality control criteria led to the exclusion of these profiles (e.g. sensor issues, incomplete dives), and whether this could bias the representation of the most extreme conditions.

Response: We thank the reviewer for this comment. The gliders shown in Figure 4 represent the full duration of each deployment after quality control. Some individual profiles were excluded due to the criteria outlined in section 2.4, including data outside the depth range, or daytime measurements etc. These criteria are methodological and were applied consistently across all deployments, rather than selectively during MHWs. While this may result in the exclusion of some profiles coinciding with the most extreme conditions, the number of removed profiles is relatively small to the total dataset.

Lines 389 – 391: Could more frequent summer and autumn events reflect increased glider sampling during these seasons?

Response: The IMOS Ocean Glider program includes routine deployments which are spread over the seasons, as well as targeted MHW deployments, which tends to be biased towards summer/autumn when the ocean temperatures are high. We have rephrased this sentence at lines 402-408 to avoid confusion.

Figure 4: Can the axis labels and colourbar labels be larger for readability?

Response: Corrected accordingly.

Line 410: DOX has already been defined on line 270.

Response: Corrected accordingly.

Line 415: For clarity, refer to the MLD rather than “both layers”.

Response: Modified accordingly.

Lines 434 – 435: Could you elaborate on how the regional and seasonal regimes do affect the multi-modal structure?

Response: Thank you for this comment. Because the sampling is insufficient to robustly resolve the full region, season, and severity interaction, we examined mean dissolved oxygen profiles separately by season and by region to assess whether particular regimes may be shaping the aggregated multimodal structure. Please see the new Supplementary Figure S2. The results suggest that both seasonal and regional variability likely plays a role, as spring and summer show higher dissolved oxygen compared to autumn, particularly in TAS and NSW regions, while QLD contributes more strongly to intermediate and lower DO ranges. We added some explanation at lines 451-454 in the updated manuscript.

Line 476: Missing comma after Holbrook and Bindoff.

Response: Corrected accordingly.

Line 501: Should this refer to oxygen saturation rather than DOX?

Response: Modified accordingly at lines 523-528.

Lines 504 – 510: Caption placement should ideally be on the same page as the figure.

Response: Modified accordingly.

Line 525: Consider using “DCM” here for consistency.

Response: Corrected in the revised text.

Line 546: GBR should be defined here rather than later on line 648.

Response: Modified accordingly.

Lines 566 – 569: Could this pattern also reflect autumn stratification breakdown and nutrient entrainment from deeper waters?

Response: We thank the reviewer for this suggestion. We agree that the elevated chlorophyll concentrations observed during autumn may also reflect seasonal stratification weakening and associated nutrient entrainment from deeper waters. We have revised the manuscript at lines 594-598 to acknowledge this.

Lines 576 – 578: Could nutrient limitation toward the end of the bloom period also contribute?

Response: We agree that with the observed seasonal evolution of stratification and chlorophyll pattern, nutrient limitation toward the end of the bloom period could contribute to lower DO observed in autumn. We added additional explanations at lines 606-609.

Line 604: Is oxygen saturation intended rather than DOX?

Response: We thank the reviewer for pointing this out. We clarified this at lines 637-639 that the oxygen saturation is above 100% throughout the water column and higher than non-MHW periods.

Discussion

Since results consistency with hypotheses (2) and (4) is discussed, it may be useful to explicitly link findings back to hypotheses (1) and (3) as well, either here or in the conclusions.

Response: Thanks for pointing this out. We have revised the Discussion section throughout to explicitly restate each hypothesis when referring to it.

Line 668: Do you have a suggestion for why it is different for NSW?

Response: Thank you for this comment. The hydrography of the EAC dominates the NSW shelf year-round. Unlike other regions, stratification in NSW is not driven purely by seasonal heating/cooling but rather modulated by shelf encroachment of the EAC, mesoscale eddies and current instabilities. As shown in Schaeffer et al, (2015), the seasonality of phytoplankton in NSW reflects the persistent influence of the EAC, with summer-spring biomass maxima and reduced winter abundance. We added this explanation at lines 683-688.

Line 716 – Earlier referred to as the Eastern Australian current on line 473 – be consistent with East or Eastern

Response: Modified according to East Australian Current in the manuscript.

Supplementary

Line 6: Add a space after “Panels”.

Response: Modified accordingly.

Fig S6: Also doesn't have the colour coding Fig. 10 has; consider mentioning this in the caption.

Response: Thank you for the suggestion. We would like to clarify that the colors in Fig. 10 refer to the Pearson correlation coefficients as shown in the colorbar, whereas the values in Fig S8 refer to the number of data points. We clarified it in the caption.