

Response to Reviewers

Report #1

Thank you for your thoughtful addressing of the issues raised in my initial review. I have listed below only a few minor points to be clarified:

Thank you very much for helpful suggestions and for re-evaluating our revised manuscript.

Line 446: This normalisation is commonly called the continuous severity index, or 'severity' for short (established by Sen Gupta et al. 2020 – see their methods section, an extension of the discrete severity categories from Hobday et al 2018), I suggest you update your paper to use this terminology to avoid confusion with other studies.

Thanks for your suggestion. We have updated the manuscript to use maximum severity instead of scaled maximum intensity (in contrast to Sen Gupta et al. we do not calculate the continuous index, but only its maximum).

Line 431 – correct to “in the 2010s”

Corrected

Line 468-469: I find this a little cryptic – ‘more similar’ to what? Can you rephrase this sentence.

We refer to the similarity between MHWs detected with the fixed and detrended baselines close to the surface. This was added to the text.

Line 501: Change “extend” to “extent”

Corrected

Line 504-505: Is it not more correct to say here that the mixed layer MHW's are driven by the residual terms, of which the air-sea heatflux is likely to be the largest (as far as I can see, you don't look at the relative size of the terms in the residual).

Yes this is true. We did not look at the vertical and horizontal contributions to the residual term.

Therefore, we have changed the sentence according to your suggestion.

We are still very confident that the vertical mixing is much more important than the lateral diffusion across the lateral boundaries and dominates the residual term. This is because for a single depth level the area of the domain is much larger than the cross-sectional area of the lateral boundary. Also, the vertical temperature gradient is much larger than the horizontal temperature gradient. Note that the air-sea heat flux affects only the first model level directly. Heat gained in the surface model layer is mixed downward, such that it is part of the residual term in all levels below.

The sentences now read:

“Within the mixed layer, MHWs detected with the detrended baseline are almost exclusively driven by the air-sea heat flux (uppermost vertical level) and the residual term. Note that the air-sea heat flux as defined here only contributes to the budget of the uppermost model level. Downward mixing of this heat to deeper levels is part of the residual term and likely the dominant contribution throughout the mixed layer.”

Line 527: I'm struggling to see a minimum at 1300 m, please clarify?

Indeed, the minimum is not at 1300 m, but the vertical temperature gradient becomes small compared to the upper water levels as it declines towards a minimum in approximately 1600 m depth. We have clarified this in the manuscript.

References:

Hobday, A. J., Oliver, E. C., Gupta, A. S., Benthuyssen, J. A., Burrows, M. T., Donat, M. G., ... & Smale, D. A. (2018). Categorizing and naming marine heatwaves. *Oceanography*, 31(2), 162-173.

Sen Gupta, A., Thomsen, M., Benthuisen, J. A., Hobday, A. J., Oliver, E., Alexander, L. V., ... & Smale, D. A. (2020). Drivers and impacts of the most extreme marine heatwave events. *Scientific reports*, 10(1), 19359.

Report #2

I congratulate the authors for the substantial changes they have performed in the manuscript. I believe the updated version is more coherent and especially the discussion section is more clear than before. Therefore I recommend publication, after a few very minor comments below are addressed

Thank you very much for your detailed comments on our first manuscript version and for re-evaluating the revised manuscript.

Minor comments:

Lines 110-113: They better moved after line 99 because there meaning is closer to that paragraph.

We have moved the paragraph to line 99.

Lines 191: Is there a reason why the fixed43yr was not used here for the calculations of ocean heat content anomalies? If so, this has to be justified somewhere. Also it has not been used anywhere in the analysis after that (e.g. in the Cape Verde analysis) and no justification was provided for this.

We have not used the fixed43yr baseline to reduce the number of subplots, to save computing time and because it does not provide much additional information. The 43yr baseline was only introduced to study the impact of including more years when calculating the MHW threshold. As the differences between 30yr and 43yr fixed baselines are overall small and using 30 years is the more common choice, we have only used the fixed30yr baseline after the comparison in section 3.1. We have added this statement to line 191.

Line 624: "These MHWs are not be necessarily caused by.."

This sentence was corrected.

In addition to the changes mentioned in our response above, we have applied a few minor changes, correcting typos or improving grammar. All changes are marked in the tracked changes pdf document.