Compound high temperature and low chlorophyll extremes in the ocean over the satellite period Response to Monique Messié's referee comments

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0.1 General comments:

The paper by Le Grix et al. is a global analysis of marine heatwaves (MHW), low chlorophyll events (LChl), and most importantly, compound events defined as both occurring simultaneously. The authors characterize these events in terms of intensity, duration, and frequency, describe their spatial and temporal patterns (including seasonal cycle and interannual variability), and analyze their link with well-known climate indices. This is an excellent paper, well-written and easy to follow. The results are novel and this is a welcome study, particularly in a context where MHWs have been extensively studied but their association with reduced oceanic productivity less so. I do have 2 concerns detailed below that should be very easy to address but may significantly impact the results and some of the paper's conclusions.

The authors are certainly welcome to not apply these suggestions, in which case this choice needs to be carefully justified (including within the paper as I expect other readers will have similar concerns).

We thank the reviewer for the positive and encouraging feedback. We have applied most of the suggestions, and if not (i.e., the duration threshold for the definition of extremes), we justified our choice in the main manuscript.

0.2 Specific comments:

One of the strengths of this paper is its continuity with the literature, particularly Holbrook et al. (2019) who analyzed MHWs. Intensity, duration and frequency are defined similarly, and the link with climate indices is conducted following the same method (contrasting event frequency in a positive or negative phase). Fig. 10 is even constructed similarly to Fig. 3b in Holbrook et al (2019), with most colors matching. This makes it easy to compare results

from this paper with results from Holbrook et al., which is very good. There are 2 ways this continuity should be further improved in my opinion, for consistency but also because results may be significantly impacted.

First, I would recommend that a duration threshold be used, at least for MHW and LChl events. Holbrook et al. used 5 days, following recommendations by Hobday et al (2016). Currently, MHW, LChl, and compounds events can be as short as one day. This goes against previous recommendations by Hobday et al (2016) and their qualitative definition of a MHW as a "discrete *prolonged* anomalously warm water event". While no definition exists for LChl in the literature that I am aware of, it does make sense to use a similar definition. While including short-duration events can be justified (and using a given threshold does introduce a bias too), my concern is that the lack of threshold is also certainly the reason for the "heavily skewed" duration distribution mentioned l. 123 that required the use of the 90th percentile, rather than mean, for describing duration patterns. Based on the "heavily skewed" distribution for duration, I am concerned that most of the results (averaged frequency/intensity) are heavily skewed, too, towards short-duration events that can be considered to hardly qualify as MWH, LChl or compound events. Fig. 3d and Fig. 4b suggest that very few LChl and compound events $>$ 5 days may be found over large parts of the ocean (where the 90th percentile is below 5 days); this is OK and an interesting result in itself. I understand that retaining all events justifies the 1% threshold for compound events; however, this threshold could be re-calculated at each pixel as the percentage of time the pixel is in a MHW multiplied by the percentage of time the pixel is in a LChl. The results could then be displayed as not only frequency (as in Fig. 4a), but also LMF relative to this local threshold as defined by Eqn (1). If you need to retain the $<$ 5-day events, this needs to be very carefully justified – in particular, does it makes sense to consider that 10% of days at any location belong to a MHW event, and (another) 10% to a LChl event? Some figures should also be added to clarify how the results are impacted by these short-duration events. In particular, do frequency/intensity patterns change when only retaining events $>$ 5 days?

Different definitions for MHWs have been used in the literature and many definitions (for example Frölicher et al. 2018; Laufkötter et al. 2020 as well as the IPCC SROCC report (Collins et al. 2019)) do not apply a duration threshold. Of course, if the reader wants to compare our results with the Holbrook et al. 2019 study, having a similar definition would be advantageous. However, as we provide Figure A4 with the MHW and LChl events individually, there should be no need to compare with Holbrook et al. 2019. An additional reason, why we refrain from changing the definition, is the fact that there is no evidence that a 5-day threshold for LChl events and compound events is more impact relevant than any other threshold.

We added the following paragraph in the Methods section: "Here, we do not apply a duration threshold as has been done for example in Hobday et al. (2016) for MHWs.

Duration thresholds are rather arbitrary as it is unknown which thresholds are most impact-relevant in particular for LChl events and compound events. Our definition without a duration threshold is consistent with the usage in the IPCC SROCC report (Collins et al. 2019)."

In general, extreme event durations are typically exponentially distributed and therefore have a heavy tail. The few very long events strongly affect summary statistics such as the mean value. We thus consider presenting the 90th percentile a more robust and informative statistic to highlight spatial differences in the event duration. We added the following sentence in the Methods section where we define the duration metric: "Figure A3 in the Appendix shows that the duration of MHWs, LChl and compound events is exponentially distributed."

Second, additional climate indices used by Holbrook et al. should be included, specifically the PDO and NPGO that both have strong footprints in the northern Pacific (Holbrook et al Fig. 3b). While these modes are decadal, they both display positive and negative phases during the 1998-2018 time period so there is no reason why they could not be analyzed. Considering how prevalent the PDO and NPGO are in the Pacific, including them makes sense and may change some of the Fig. 10 results (if Holbrook's results are any indication, I would expect the NPGO to replace the NAO in the north-eastern Pacific, and the PDO to replace the EMI in the north-western Pacific). The NPGO may not have enough negative values over 1998-2018 but the impact of the positive phase could be assessed at least – particularly if the positive/negative phases where compared to the neutral values, rather than the mean (which I believe makes more sense anyways as the mean can be skewed towards positive or negative events).

We agree and have included PDO and NPGO in our study.
The positive phase of PDO is associated with the greatest occurrence of compound events in some parts of the Indian Ocean and of the tropical and northeastern Pacific. The negative phase of NPGO is associated with the largest frequency of compound events in some parts of the North Pacific gyre. NAO is associated with their highest occurrence in the eastern equatorial Atlantic, in the Gulf Stream region, and in a few parts of the northeastern Pacific.

0.3 Minor comments and technical corrections:

l. 87-88: This is mentioned in the discussion, but it would be worth mentioning here that daily satellite chlorophyll cannot be used for this analysis because the data coverage is too poor at the daily scale (notably due to clouds).

We added: "For chlorophyll, satellite data derived from ocean colour cannot be used because the coverage is too poor at the daily scale, notably due to clouds."

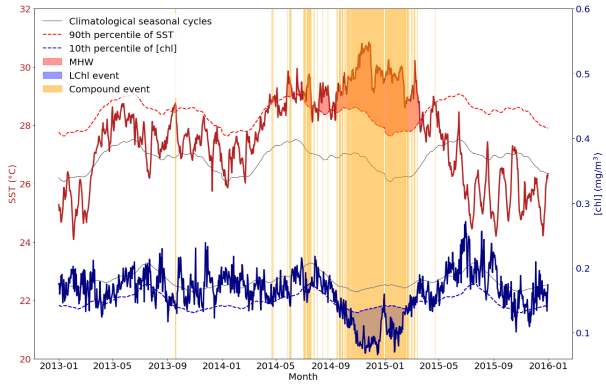

Figure 1: Without smoothing

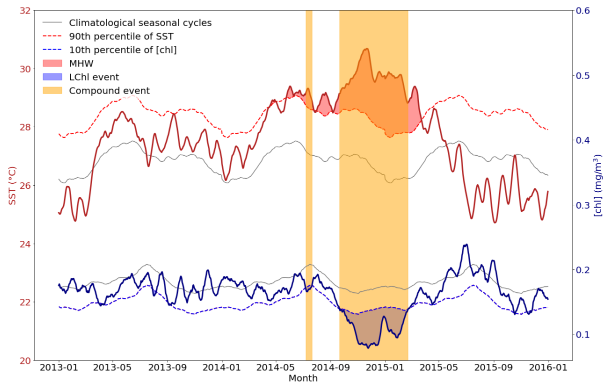

Figure 2: With smoothing

Fig. 2: Were time series smoothed using a 14-day running mean prior to MHW, LChl, and compound event definition? the text only mentions smoothing the daily seasonal cycle. If time series were not smoothed in the calculations, they should not be smoothed in the figure. If they were, please update the text.

We decided to smooth the time series on Fig. 2 to obtain a better visualization of how MHW, LChl, and compound events are defined, even though time series of SST and chl anomalies are not smoothed in the rest of our study. We added a figure with the original data (without smoothing) in the Appendix.

We specified in the caption of Fig. 2 that the smoothing was done "for illustrative purposes", and added the following sentence in the paragraph describing Fig. 2: "Time series of SST and chlorophyll concentration are smoothed with a 14-day running mean to obtain a better visualization of extreme events; for comparison, figure A1 in the Appendix shows results with no smoothing."

l. 123: see comments above regarding the heavily skewed distribution. If you decide to retain short-duration events, at the minimum the distribution should be displayed (eg. box plot) for readers to understand how much of an impact short-duration events may have on the results.

See our response higher up. We have added a figure showing the distribution of event durations in the Appendix. They are typically exponentially distributed with a heavy tail. As all our analysis consider days in an extreme/compound event separately, very long events have a similar effect on our results than many very short events. We thus consider the effect of including all short-term events minor. Note that we do not use the term "skewed" anymore, as it conveyed a somewhat misleading image of the duration distributions.

l. 159: as a suggestion and as stated above, consider comparing the frequency of extreme event days over each climate phase to their frequency over the neutral phase, rather than over the complete 1998-2018 period. How did you attribute events spanning several phases between positive/neutral/negative, particularly events that might be long enough to span both positive and negative phases?

We attribute each day in the time series to a positive, neutral or negative phase, and then compute the frequency of compound event days over the positive, neutral and negative phases. Therefore, we consider separately the days of a common event spanning multiple phases.

Please note that we now compare the frequency of extreme event days over the positive and negative phases of each climate mode to their frequency over the neutral phase.

l. 185 "Similarly to MHWs": I would actually argue, based on Fig. 3c vs d, that MHW and LChl have almost exactly opposite duration patterns over most of the global ocean when excluding the Southern Ocean.

We agree and have replaced "Similarly to MHWs, short LChl events are located in the mid-latitudes" by "When excluding the Southern Ocean, MHW and LChl events have rather opposite duration patterns over most of the global ocean".

l. 190 "MHWs and LChl events often occur simultaneously": In addition to comparing the compound event frequency to the expected frequency, it would be useful to report the percentage of MHWs events that coincide with a LChl event, and the percentage of LChl events that coincide with a MHW event ("day(s)" could be used instead of "event(s)" in this sentence, not sure which would be most informative).

This follows directly from the definition of compound events. At each grid cell, both MHWs and LChl events have a frequency of 10% of days. If compound events occur x% of days, the percentage of MHWs (LChl) that coincide with LChl (MHWs) events is $(x/10) * 100$ %. No changes are made in the manuscript.

Fig. 4: consider using a linear scale, or at least displaying "logical" colorbar ticks (e.g. 0, 1, 2, 3). As it is the results are difficult to visualize.

We now use a linear scale on Fig. 4.

l. 196-204: while the fact that compound events are located in regions where Chl/SST are negatively correlated makes perfect sense, this is still a very interesting result and it was nicely demonstrated.

Many thanks!

l. 210 "There are exceptions however. Some exceptions also occurred. . .": Should one of these 2 sentences be removed? Not sure what you meant here.

We wanted to say that the frequency pattern of compound MHW and LChl events shown in Fig. 4a is not coherent with Fig. 1 everywhere. Compound events are not frequent over all regions where prominent MHWs were associated with negative chlorophyll anomalies. We now write: "There are exceptions however, such as in the northern subtropical Pacific gyre where chlorophyll concentrations were locally high during the 2013-2015 MHW, even though compound MHW and LChl events are relatively frequent ($> 1.8\%$ of all days) there."

l. 217 "Long compound events (> 10 days)": did you mean "where 10% of events last longer than 10 days"?

Indeed, we need to be more accurate here and have replaced "> 10 days" by "where 10% of events last longer than 10 days".

l. 291-300: As acknowledged in the discussion, correlation does not equate causation. Please rephrase sentences such as "El Nino Modoki leads to the greatest occurrence. . .", "The Indian Ocean Dipole is the main contributing climate mode. . .", "The North Atlantic Oscillation is the main modulator. . .". The modes are associated with high compound frequency (as said in other sentences) but we don't know if they drive them.

We now write:
"ENSO seems to be the main modulator of compound events in the eastern equatorial Pacific and in the northwestern part of the Indian Ocean, where El Niño events are associated with the highest frequency of compound event days from 1998 to 2018. The positive phase of PDO is associated with the greatest occurrence of compound events in some parts of the Indian Ocean and of the tropical and northeastern Pacific. The negative phase of NPGO is associated with the largest frequency of compound events in some parts of the North Pacific gyre. The Indian Ocean Dipole is the climate mode associated with the highest occurrence of compound events around Indonesia and in parts of the subtropical Pacific. NAO is associated with their highest occurrence in the eastern equatorial Atlantic, in the Gulf Stream region, and in some parts of the northeastern Pacific. Finally, AAO is associated with the highest frequency of compound events in

some parts of the Southern Ocean."

l. 313 and 320: aren't these sentences contradictory? the first indicate that the eastern equatorial Pacific is an exception to the Hayashida rule, the second indicate that the eastern equatorial Pacific behaves as expected. It almost seems like you consider the eastern equatorial Pacific to be nutrient-rich l. 313 and nutrient-limited l. 320.

The eastern equatorial Pacific is an exception in the sense that even though the background nutrient concentration is high, marine heatwaves are associated with low chlorophyll in this region.
On l.320, we replaced:
"we also identify elevated compound event frequency in the nutrient-limited surface waters of the low latitudes (including the eastern equatorial Pacific) and low compound event frequency in the nutrient-rich surface waters of the Southern Ocean (Fig. 4a)."
by:
"we also identify elevated compound event frequency in the nutrient-limited surface waters of the low latitudes and in the eastern equatorial Pacific, and low compound event frequency in the nutrient-rich surface waters of the Southern Ocean (Fig. 4a). The eastern equatorial Pacific behaves like a nutrient-limited region even though it is nutrient-rich."

l. 321-323: any hypothesis as to what may be at play in these regions?

We added the following sentence:
"There, phytoplankton growth may be limited by other key nutrients (e.g. iron around Antarctica) or increased phytoplankton grazing may lead to low chlorophyll during marine heatwaves."

l. 337-338: this is not what I see when comparing Fig. 10 to Holbrook et al (2019, their Fig. 3b) for the Southern Ocean. Both figures highlight ENSO and the AAO/SAM, and both display a very complex picture. Did you refer to the patchiness and frequency of white pixels, indicating that no clear signal can be identified?

We agree that figures are not so different in the Southern Ocean and replaced "over the Southern Ocean" by "in the western Pacific", where ENSO is for example associated with the highest occurrence of compound events and EMI with the highest occurrence in MHWs.

l. 340: see comment #2. Particularly for the PDO, there are both positive and negative phases during 1998-2018.

We replaced:
"This indicates again that other processes (see above) may affect chlorophyll concentrations in these regions or that MHWs are mostly modulated by climate modes that we omitted in our study (e.g. the Pacific Decadal Oscillation in 340 the northeastern Pacific)

because our shorter period of analysis does not capture their variability."
by:
"This indicates again that other processes (see above) may affect chlorophyll concentrations in these regions, that MHWs are mostly modulated by climate modes that we omitted in our study (e.g. the Interdecadal Pacific Oscillation in parts of the Pacific Ocean) because our shorter period of analysis does not capture their variability, or that some climate modes would be dominant if we used a longer period of analysis such as in Holbrook et al. 2019."

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