Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2020-412-RC2, 2020

Interactive comment on "Compound high temperature and low chlorophyll extremes in the ocean over the satellite period" *by* Natacha Le Grix et al.

Monique Messié (Referee)

monique@mbari.org

Received and published: 10 December 2020

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).

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?.

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).

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).

Fig. 2: Were time series smoothed using a 14-day running mean prior to MHW/LChl/compound event definition? the text only mention 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.

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.

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?

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.

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).

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.

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.

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

l. 217 “Long compound events (> 10 days)”: did you mean “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.

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.

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

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?

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

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