



EGUsphere, referee comment RC3  
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## **Comment on egusphere-2022-31**

Anonymous Referee #3

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Referee comment on "Upper-ocean response to the passage of tropical cyclones in the Azores region" by Miguel M. Lima et al., EGU sphere,  
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In this manuscript, the authors use satellite-derived data and historical Tropical Cyclone (TC) data to evaluate the typical ocean responses, in Sea Surface Temperature (SST) and Chlorophyll-a (Chl-a), to the passage of TCs in the Azores region, which is located north of the main TC area in the North Atlantic. They find that the mean ocean responses are a decrease of 1.554 °C in SST, and an increase of 0.026 mg/m<sup>3</sup> in Chl-a. They also find that the intensity of the ocean response is strongly related to the TC wind intensity and, to a lesser extent, the TC-covered area, with little relation to the translation speed. Finally, the authors analyze in more details the cases of 2 TCs, Hurricanes Ophelia (2017) and Nadine (2012): during Hurricane Ophelia the decrease in SST happened early in the TC history, while the increase in Chl-a happened toward the end, suggesting decorrelated processes, while during Hurricane Nadine the shape of the TC track and its translational speed impacted the ocean response.

The manuscript is concise and generally well written. It is a nice contribution to the state of knowledge related to TCs in the Northeastern Atlantic, in a region that has so far received little attention with respect to TCs, whereas it is expected to be more exposed in the future, as strong TCs are expected to venture further northward under the current climate change. However, I find that the approach using interpolated satellite-derived data, which are presented as providing an advantage, raises serious questions. In addition, I find the presentation of the methodology to estimate anomalies confusing. Finally, I find that the analysis of Hurricane Nadine is not very convincing. As a result, I recommend publication, with Major Revision, in order for the authors to modify their approach or to better justify for it, to improve the description of the methodology, and to reconsider or improve the analysis of Hurricane Nadine. Please see the detailed comments and suggestions below.

## Detailed comments

First, the authors present their use of interpolated, gap-free data as “particularly relevant in the context of this study” (l. 88), to which I strongly disagree. The presence of clouds during TCs masks the area and period of the most intense air-sea interactions, when the winds are the most intense. Interpolating from observations taken when and where there is no cloud, i.e. when and where the TC is absent, does not allow estimating the ocean state under the TC. Interpolation simply does not give access to missing observations. I suggest the authors to perform the same analysis with original, non-interpolated data. Their results should be similar, without anyone questioning the impact or role of the interpolation on their results.

Second, I find the presentation of the anomaly estimates confusing (l. 145 to 175 and Figures 2 and 3). The authors first mention that they “analysed daily anomalies registered between 30-days before and 30-days after the passage of a TC” (l. 149-150). With respect to what are these anomalies estimated? These anomalies are then apparently used to estimate the periods to consider before and after the TC to estimate anomalies (l. 150-155 and Figure 2). Figure 3 seems to present anomalies that are used to identify periods from which anomalies are going to be estimated. Overall, it seems that there are 2 types of anomalies, but this is not clear. This whole section of the manuscript needs to be clarified.

Finally, I find that the analysis of the Hurricane Nadine case (2012) is not very convincing. Figure 7 shows very small differences in the mean SST and Chl-a anomalies estimated from the various levels of overlap. Are these differences significant? It seems very hard to conclude anything based on Figure 7. I suggest the authors to perform a more thorough analysis of that hurricane, or of another TC, in order to bring more convincing results to their study.

## Suggestions

- l. 11, I suggest: “during and after the passage of a TC”, not only after.

- l. 22, “while dynamical mixing played a more important role in the later stage”: that analysis is not found in the main manuscript.

- l. 33-35, I suggest: “In his seminal study, Price (1981) shows, through both observed and numerical modelling data, the evolution of sea surface temperature (SST) etc.” The

grammatical structure used by the authors is not correct, or not idiomatic, in English.

- l. 39-40, I suggest: "Additionally, several model-based works focused on either the effects caused by the TCs, or the interaction of the TC with its own cold wake (e.g., Chen et al., 2017; Zhang et al., 2019)."

- l. 41-42, I suggest: "Due to the upwelling of colder water, transport of nutrient-rich water from the sub-superficial layer may also occur (Kawai & Wada, 2011)."

- l. 52-54: In the Bay of Bengal, surface salinity associated with river discharge is thought to play an important role.

- l. 86-90 and l. 93: Please see my detailed comment about interpolated data.

- l. 100, I suggest: "up to 2020", since the most recent full hurricane season is now 2021.

- l. 111: full tracks.

- l. 112: "the right panel showing a zoomed view..."

- l. 124: The authors can refer to Figure 1 here.

- l. 126-128: This repeats the end of the previous paragraph.

- l. 128-129: "with fewer and less intense tropical storms..."

- l. 138: "...isotach, tropical depressions were not considered (the exact partition of intensities is given etc.)"

- l. 139: "The correction for some missing..."

- l. 140: Figures 6 and 7 are mentioned before Figure 2. I suggest not to mention them here, but to mention the name of the corresponding section instead.
  
- l. 143: "considered area uses histograms, in which..."
  
- l. 149: Anomalies with respect to what? Please see my detailed comment about the anomaly estimates.
  
- l. 158: "Schematic", not resumed schematic.
  
- l. 159: "The colour coding is as follows:..."
  
- l. 161: "Individual daily anomalies", not induced.
  
- l. 163: Figure 7 is mentioned before Figure 3. I suggest not mentioning the figure.
  
- l. 168: Same as previously mentioned: with respect to what are the anomalies shown on Figure 3 estimated?
  
- l. 171, I suggest: "period where the effects on the oceanic variables are clearly visible."
  
- l. 173: "too short", instead of too small.
  
- l. 174, "both cases": Are the authors referring to both variables?
  
- l. 174-175, "those extra periods varied both in number and location": It is not clear what the authors are referring to here.
  
- l. 177: Same as previously mentioned: with respect to what are the anomalies estimated?

- l. 184, I suggest "both variables present a large impact..."
  
- l. 192, I suggest "weak" instead of "weaker". Otherwise, the authors have to specify weaker than what.
  
- l. 199: "Fig. 4d shows", not "Fig. 4d, shows".
  
- l. 203: later, not latter.
  
- l. 204-205, "Powerful TCs induced a more varied distribution of anomalies": what exactly do the authors mean?
  
- l. 205-207, "Do note that these different distributions do not represent the same geographical areas, since they are analysing different observations associated with the location of each TC as it moves along its storm-track": Why are the authors mentioning this here? How should that affect the analysis of the histograms?
  
- l. 232, I suggest "with a dashed line representing a regression non-significant at the 95 % confidence level, and a solid line representing a regression significant at the 95 % confidence level."
  
- l. 235: "amount of sampling data within the region".
  
- l. 235: "Red squares".
  
- l. 238, I suggest "study region" instead of "sector".
  
- l. 251, I suggest "in total (grey)".
  
- l. 253: "maturing (Fig. 6a, squares), and mature hurricane (Fig. 6a, stars)."
  
- l. 254: ", and the last one, 7".

- l. 257: "therefore, the sum of all three phases will result in the bigger histogram."

- l. 262-263, "with the last phase weighing the most in the general distribution (as was seen for the chl-a)": Isn't this due to the number of observations, related to the size of the storm? How does that affect the results or analysis? I am not sure this information is very relevant.

- l. 263, I suggest: "The highest SST impact..."

- l. 263-265: That sentence is relevant to SST only. The authors could check how robust their analysis is, maybe by looking at the SST values along the track and how they support or not strong TC air-sea interactions. Also, the authors do not analyze the Chl-a signal, which is different from the SST one. A brief analysis is included in the abstract, it should appear here in detail.

- l. 269: The caption is in contradiction with the text. In soft grey it should be all data, not "no overlap", based on the text (l. 272-273) and on the map on Figure 7. Based on the text, in dark grey should be 3 overlaps or more, and in black 5 overlaps or more.

- l. 274-277: As mentioned in my detailed comments, the differences seem small. Are they significant?

- l. 277-278: That sentence is not very clear. How was the TC intensity deemed irrelevant?

- l. 278-281: As mentioned in my detailed comments, the results derived from Figure 7 are not very convincing, which makes it hard to support the hypothesis mentioned here.

- l. 284-286: As mentioned in my detailed comments, the use of interpolated data raises some serious questions. I suggest revisiting the analysis using non interpolated data.

- l. 287-288, I suggest: "the existence of a bio-physical response after the passage of a TC was identified from the analysis of chl-a and SST datasets, which etc."

- l. 288: I suggest removing the parentheses for Chl-a and SST.
  
- l. 289: I suggest removing "considerably", it seems a bit exaggerated.
  
- l. 291: "anomalies in the order of 0.026 mg/m<sup>3</sup> in chl-a..."
  
- l. 293: "... oceanic responses, which agree with..."
  
- l. 294-296: There is a contradiction in that sentence, with a relationship that was first confirmed and then not confirmed. I suggest rephrasing into something like: "In agreement with previous studies (refs), the TC translation speed was also found to be associated with anomalies in both variables, although the relationship was not significant at the 95 % statistical confidence level in our study."
  
- l. 299-300: Can these various effects and their induced anomalies be documented with the database assembled by the authors?
  
- l. 309: I disagree with "It is evident through this analysis", see my previous comments.
  
- l. 312-313: I disagree with that sentence. The TC-induced changes should be identified on non-interpolated data. The most relevant quality control of the interpolated data would be an evaluation using independent data collected under the TCs, which is of course challenging.