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# *Interactive comment on* "Global representation of tropical cyclone-induced ocean thermal changes using Argo data – Part 1: Methods and results" *by* L. Cheng et al.

**L. Cheng et al.**

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We thank Reviewer 1 for his/her careful review of our paper submitted to Ocean Science. We appreciate the thoughtful and constructive feedback on the paper, which has helped to significantly improve the manuscript. We have addressed all concerns in the revised manuscript, as documented below in our point-by-point responses.

Major Comment

This paper by Cheng et al. explores ocean thermal responses to tropical cyclones using data collected by Argo floats. The analysis shows interesting features, including differing ocean responses to tropical cyclones of different intensities. The paper is



well written. The results presented in this paper (Part 1), however, mostly confirm findings from previous studies. In addition, it is not worth devoting so much space to the description of the footprint method and the estimation of background variability (sections 2.2 and 3). It may be better to put them in the appendix. After having a rough look at Part 2, I would like to suggest the authors combine the two parts into one paper. A combined paper would fit better within the scope of Ocean Science, and would present the interesting results in a concise way.

Re: In response to the reviewer's major concerns, this analysis represents the first global representation of the upper-ocean thermal response to tropical cyclones using in situ sub-surface ocean observations (Argo floats). While we agree with the reviewer that our results generally confirm previous observation-based estimates, we think this analysis represents a fundamental advance in our understanding about the importance of tropical cyclones on upper ocean heat content, given the myriad of limitations and simplifications of previous observation-based estimates which are primarily either regional scale assessments and/or use simplified approaches based on surface observations. In addition, this analysis has enabled a much more comprehensive and systematic view of small-scale features and spatial-temporal variability compared to past case studies that analyzed a limited number of storms. By merging the two manuscripts together (in accordance with both reviewers' recommendation), we think the novelty and relevance of the results are much more apparent in the revised manuscript (and we have added additional discussion within the abstract and text to illustrate these points). In addition to merging the manuscripts, we have also moved the discussion about background variability to the appendix as the reviewer suggested.

Specific Comments:

(1). Lines 20-21 on page 2836: They can also be affected by horizontal advection, see e.g. Huang et al. (2009).

Re:We fully agree that they can also be affected by horizontal advection. We have updated this statement in the revised manuscript.

(2). Line 15 on page 2848: Do the authors mean the turbulence that is generated via stirring but not related to shear instability? Please clarify.

Re:Thank you for pointing out this poor wording. It is correct that both processes can generate turbulence, and we have clarified this point in the revised paper.

(3). Lines 3-17 on page 2848: Please shorten this part, as it is not necessary to describe these well-known processes in detail.

Re: We have shortened this section as the reviewer suggested.

(4). Lines 20-21 on page 2848: Please compare Fig. 10 with previous case studies, e.g. Fig. 1b shown in Price (1981).

Re:We have updated this sentence as the reviewer suggested and included the Price (1981) reference.

(5). Line 10 on page 2849: I don't understand why geostrophic adjustment may be one physical mechanism. Please explain it.

Re:The statement in our original manuscript was speculative, and we decided to remove it from the revised manuscript to avoid confusion.

(6). Lines 12-18 on page 2851: The authors' two explanations for the stronger warming seem unreasonable. Probably it is better to remove the climatological seasonal cycle first before any further analysis.

Re:The two explanations in our original manuscript are speculative, and we have removed them from the revised manuscript. Regarding the seasonal cycle, examination of background variability does not indicate any significant effects of the seasonal cycle, which is largely why we decided not to remove it in our original analysis. In addition, removal of the seasonal cycle typically involves subtracting a monthly climatology from each individual profile, which implicitly assumes all of the data observed in a spe-



cific month are referenced to a common monthly climatology. This assumption is valid when analyzing inter-seasonal, inter-annual or decadal scale ocean variability, but it is not necessarily appropriate for examining TC-induced changes on several days scale.

(7). Line 9 on page 2852: "observational" should be "modeling"?

Re:Change made as reviewer suggested.

The revised manuscript is attached as a supplementary file (figures are all included for clarification), and we highlight the revisions according to the referee's comments in blue:

Please also note the supplement to this comment:
http://www.ocean-sci-discuss.net/11/C1446/2015/osd-11-C1446-2015-supplement.pdf

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Interactive comment on Ocean Sci. Discuss., 11, 2831, 2014.