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Interactive comment on "Eddy-induced Track Reversal and Upper Ocean Physical-Biogeochemical Response of Tropical Cyclone Madi in the Bay of Bengal" *by* Riyanka Roy Chowdhury et al.

Anonymous Referee #1

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Recommendation: Substantial revision

General comments

This paper studies the upper ocean physical and biogeochemical response to Cyclone Madi (2013) in the Bay of Bengal from the analysis of multiple atmospheric and oceanic data. The topic is interesting. My understanding is that there are many studies regarding the topic: the upper ocean physical and biogeochemical response to cyclones. Some of the studies have been referenced in this manuscript. Therefore, my concern

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is the novelty of this paper.

There are a few discussion points on this study.

1) I don't agree with the conclusion that "oceanic eddies affect the translation speed of Madi". This study lacks discussions on what factors do affect the movement of cyclones. In addition, there is no information regarding the atmospheric steering flow and wave-1 asymmetry of the inner core structure that is very important in understanding the movement of storms, although the authors showed the horizontal distributions of 500-hPa winds in Fig. 6. There is a cause-result confusion on the authors' conclusion that the motion of the cyclone is mainly controlled by cold eddy when the cyclone stagnated over the cold eddy without any considerations of the variation of environmental steering flow.

Looking at Fig.5, there seems to be no interaction between Madi and mid-latitude westerlies. I don't understand the role of vertical wind shear shown in Fig. 5 in the evolution of Madi. According to Fig. 6, the movement of Madi seems to be affected by high-pressure area east of the cycle, moisture transport along the edge of the high-pressure area, and high-pressure area west of the cyclone with dry airs after the recurvature (north-easterlies pointed out by Bhattacharya et al. 2015). In my opinion, the synoptic variations did affect the movement of Madi rather than the cold eddy underneath the cyclone. In that sense, I agree with Bhattacharya et al. (2015).

On the other hand, the impact of cold eddies on cyclone intensity is well known. What are new findings regarding TC-ocean interactions in this study?

2) I do not understand how to calculate net primary production (NPP). Anyway, the study of the upper ocean biogeochemical response to Cyclone Madi is one of the topics of this study. As for the sudden increases in total CO2 flux, Bates et al. (1998) and Nemoto et al. (2009) have already reported it from the analysis based on the observations. In addition, Wada et al. (2011) conducted numerical simulations by an ocean general circulation model to clarify the mechanism and relation to the relative

position to the storm center regarding the variation of the sudden increases in total CO2 flux. My question is what are new findings in this study regarding the relation of oceanic eddies to the sudden increases in total CO2 flux. This also applies to chlorophyll-a concentrations. In the abstract, the authors only show the number of folds regarding rapid increases in chlorophyll-a concentrations, NPP and total CO2 flux. Without any information on these background values, readers cannot understand the importance of these extreme events.

In that sense, the authors did not present in-depth analysis on the time series of the vertical profiles of water temperature, salinity and chlorophyll-a concentrations. These profiles may show the ocean response to a storm occurred on the right-hand side of a moving storm (in the Northern Hemisphere). Certainly, sea surface temperature in the mixed layer decreased due to vertical turbulent mixing, but sea surface temperature in the thermocline increased during the passage of the storm and then decreases. There is no discussion regarding this oceanic response and its relation to variation in chlorophyll-a concentrations although they have been already studied.

I hope the above mentioned discussion points will be clear.

Specific comments

3) I suspect the calculation method (equation (3)) of ocean heat content (OHC) in this study. First, there is no information on the oceanic dataset in the manuscript. At least, water temperature from the surface to 300-m depth is needed in equation (3). Second, 3.574x1011 (Q) / 1.026x103 (density) / 3.87x103 (Cp) / 300(m) $\sim$ 300 K. This means that water temperature should be 300K from the surface to 300-m depth so that this result is not consistent with Fig. 11(a). Therefore, Fig. 3 only shows information which is the same as the horizontal distribution of sea surface temperature.

4) In equations (1) and (2), there is no information regarding how to calculate wind stresses.

5) In the abstract, there are many descriptions regarding a lifecycle of Madi. It seems that the authors intentionally increased the number of characters of the abstract.

6) The reference of O'Brian et al. is not correct. Maybe this paper shows the results of idealized numerical experiments regarding the upper ocean response to a vortex.

7) In general, vertical wind shear is calculated as an areal average. How did the authors calculate the vertical wind shear? What did the authors intend to analyze the vertical wind shear?

8) How did the authors calculate weekly composite of daily chlorophyll-a concentrations? Because the moving speed of Madi is relatively slow, there seem to be a lot of data missing areas.

9) In equation (5), salinity data are required. However, there is no information on salinity data used in this study.

10) In Table 1, the category of the intensity of Madi is not based on the Saffir-Simpson scale.

11) It is very hard to see black arrows in Figs. 3-6. The marks in all figures tend to be small.

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