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Interactive comment on "Upper ocean response to two collocated typhoons" *by* D. B. Baranowski et al.

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We are grateful to the anonymous Referee for his or her review. We would like to address his/her major comments below in detail.

Title: Due to suggestion by the Referee the title has been changed. The old title term 'collocated' referred to the proximity of the typhoons in space-time domain. The new title is "Upper ocean response to the passage of two sequential typhoons" which we think is more accurate.

Here we address comment that the study is not particularly-well motivated. The revised version is much better motivated. In particular, we added several important references that help out to put the problem we are addressing in stronger context. Parts of the

introduction have been rewritten to strengthen the motivation. In the bottom panel of the Figure 2 the statistics of sequential typhoons in WPAC is presented using two spatial proximity criteria. This provides convincing argument that the problem is relevant to real forecasting issues and that sequential typhoons existing and evolving in proximity to each other occur several times during every typhoon season.

Here we address comments about the linear drag. The 3DPWP is fully three dimensional ocean model and as such is fully capable of resolving internal waves dispersion. However, in our numerical experiments, the only type of wave forced in the model, were inertial oscillations. The simulation set up did not allow for possible interactions between typhoon forced inertial waves and other waves independent of the typhoon. The decay term was a parameterization introduced in order to force faster decay of the energy associated with inertial motions, independent of pure dispersion. The linear drag parameterization was introduced to account for interactions with all inner ocean processes that could not be forced in our simulation set up.

That being said, we recognize that our primary problem is study of influence of the preceding typhoon on sequential typhoon wake and that the discussion of drag coefficient is secondary to the physical processes that we focus on. Therefore, we decided to limit our model simulations to the original 3DPWP, without additional drag term. Thus, all the results related to the linear drag parameterization in the paper were removed.

Minor comments:

P2259 line 22: Argo based climatology by Roemmich and Gilson uses all available Argo profiles in a given area to produce monthly estimates of the ocean stratification. It does not discriminate between 'typhoon affected' profiles and undisturbed profiles. As the Referee pointed out, high frequency of the storms in this particular area at this time of the year raises difficulty of defining the undisturbed ocean state. But the multiyear monthly average is the best possible dataset to assess such mean, expected state of the upper ocean, even though some of the profiles used to calculate monthly mean

were, inevitably, measured after a passage of a typhoon.

P2260 line 14-15: We agree with this comment of the Referee and removed references to the temperature, thus streamlining the text and its focus on salinity in that part.

P2266 line 29-30: Near inertial currents are represented in the model, but given 1 day temporal resolution of measurements, it is not possible to directly compare between measurements of the MLD and its model representation.

P2272 line 15: in the absence of the preceding typhoon, intensity (magnitude) of the ocean response to the sequential typhoon would be larger according to the results we present.

Other comments:

All other comments were taken into account and are corrected in the new version of the manuscript.

Interactive comment on Ocean Sci. Discuss., 10, 2255, 2013.