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

Anonymous Referee #2

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Summary:

This manuscript presents observations from ARGO floats at roughly the same location in response to two typhoons whose passages occur around 1 week apart. In addition, results are interpreted in the context of a numerical ocean model. The article is fairly well written although the English grammar could be improved a bit, including the typical misuse of noun articles common among non-native English speakers. While results are quite interesting, the manuscript could be improved significantly by motivating the study better and correcting the modeling approach in a couple of areas. I am recommending resubmitting for publication after addressing these issues.

Major Comments:

First, the title is very misleading. Obviously, the two typhoons were not “collocated”, which is a physical impossibility. The observations are fairly collocated in space, but certainly not in time. Consider revising for accuracy.

The study is not particularly-well motivated. The authors briefly mention on lines 21-22 that the response of a leading typhoon may impact a trailing storm, and in the previous paragraph discuss statistics for typhoons with recurring tracks. However, they do not provide any information whether a systematic multi-storm impact actually exists. The study would be more relevant if some information on intensity was provided that warrants such a study.

The 3DPWP model as presented in the cited references is three dimensional (as the name implies) and non-linear advection terms are retained. Thus, it is not clear why the need for the additional linear drag term (p 2262 line 12). The model should handle 3D processes, including internal wave dispersion, which is the physical reason for the temporal decay of wave energy at a fixed location. In short, the additional decay term is unnecessary for the model. Estimating decay rates in observations is useful for interpreting the dispersion of wave energy, and does not represent an additional dynamical process.

Minor Comments:

P2259 line 22: Given the high-frequency of typhoon activity in this area, how “undisturbed” is the Argo-based climatology? How much of the climatology is contaminated by typhoon-induced motions/temperatures/etc ?

P2260 line 10: “typhoons” should by “typhoon”

P2260 line 14-15: It’s not clear why the passage of two previous typhoons (Sinlaku and Nuri) would result in an initially warmer ML prior to the Hagupit-Jangmi pair.

P2261 line 17: Change “repeating” to “repeated”

P2266 line 25: Change SST’s to SSTs

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P2266 line 29-30: Internal (near-inertial) waves should absolutely be represented in the 3-D model.

P 2269 line 21: Change “Hagupi” to “Hagupit”

P 2270 line 15: Insert “a” before “decay”, and “to” before “1”

P2270 line 16: Insert “a” before number

P2270 line 17: Insert “the” before “linear”

P 2270 line 25: Energy is transferred by energy flux, not momentum flux

P2272 line 15: What was the actual intensity response? Is the hypothesis confirmed or not?

P2272 line 22: Sentence beginning “We show...” The assertion that the initial kinetic energy is important but has not been previously studied is untrue. While not contextually identical, recent studies (e.g. Uhlhorn and Shay 2012,2013) examined the importance of a pre-existing reservoir of kinetic energy on upper ocean response to a tropical cyclone.

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