

Interactive comment on “Eddy- and wind-sustained moderate primary productivity in the temperate East Sea (Sea of Japan)” by G.-H. Hong et al.

Anonymous Referee #2

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In this work, the authors describe the temporal evolution of hydrodynamic and biogeochemical variables in the Sea of Japan by combining in-situ data with satellite remote sensing data. The approach is interesting and I sincerely appreciate the effort in bringing together so many different sources of information to tackle the difficult question of mesoscale variability and its influence on physical-biological coupling in the pelagic ecosystem. However, after reading the manuscript, I have been left wholly unconvinced about the conclusions presented and quite unhappy with the text itself. Ideas are confusing and the text is repetitive and unclear in many instances. In particular, the main proposal of this work is that ‘The primary productivity in the nutrient-depleted surface ocean was found to be enhanced by subsurface water upwelling where the wind and

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water move in the same direction as the mesoscale eddy’, this is, however, not supported by the presented data and analysis. Henceforth, my recommendation is that, at the present state, this manuscript is not acceptable for Biogeoscience.

Major issues;

1) The main hypothesis presented in this work is that enhanced productivity happens in the periphery of mesoscale eddies when wind and currents direction are coincident. The authors present as proof Fig. 10. Here it could be seen that during April and December 2009 both wind and water movement are coincident (Fig. 10a). At the same time high chlorophyll values are registered at the mooring site (Fig. 10b). This coincidence is claimed as a proof that enhanced upwelling occurs when wind and currents are coincident, which trigger primary production close to the surface. However, from the beginning of the results section (Fig. 3) the bimodal chlorophyll time series is justified as a consequence of the seasonal succession typical of the region (i.e., enhanced production in spring after the winter mixing and again in fall as a consequence of stratification weakening).

2) The authors show that during some parts of the stratification period there is a statistical correlation between water temperature at 40 m depth and surface chlorophyll concentration. They propose that short-scale upwelling events (indicated by the shoaling of the thermocline) trigger punctual increases in primary production during the unproductive season of the year. This is fair for me but, again, Fig. 10a shows that no coincidence between wind and currents direction happen for the time-periods when the correlation is found. This is another proof that the principal hypothesis of the work is not supported by the observations.

3) Also, and related with the previous point. The authors describe the relationship between surface chlorophyll and sea temperature at 40 meter for three specific low-amplitude chlorophyll peaks happening during summer-autumn. However, in figure 3 there are so many other chlorophyll relative maximum during the same time-period that

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do not appear to be correlated with such a drop in temperature. It will be necessary to explain also those 'non-correlated' peaks (even if only by proposing alternative hypothesis). In page 10440 you mention that the correlation between relative chlorophyll maximum and a drop in water temperature did not hold for summer but no alternative explanation is provided.

Minor details;

1-It will be nice to have a map of the studied site as Fig. 1 of the manuscript 2-Description of the mooring is not clear. You mention that there was an ECO fluorometer at the surface and later on (page 10433) another 'stimulate fluorometer' is mentioned. Are they the same? Do you have fluorescence measurements at other depths than surface? 3-It is not totally true that there are no seasonal temperature variations below 40 meters depth. I will rather say that the importance of such seasonality is lower than at surface 4-It is relevant that low salinity is related with low temperature? Do we need salinity at all for the purpose of this work? 5-Data presented in Fig. 5 did not come from the described mooring. Where did it come from? Just a succinct description is provided in the caption but no reference is made in the text 6-Discussion is too vague and confusing that the reader will get lost without a clear 'take-home' message. This entire section should be re-written focusing in the findings of the present work. 7-What is 'UB'? It should be defined the first time it is used in the text.

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