

Interactive comment on “Spatial and temporal variability in nutrients and carbon uptake during 2004 and 2005 in the eastern equatorial Pacific Ocean” by A. P. Palacz and F. Chai

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We thank the Referee for the constructive criticism and valuable suggestions on how to improve the manuscript. Below we address Referee's general comments as well as the more technical comments.

General comments:

"i) There are numerous papers in the literature that focus on influence of TIWs on the nutrient and ecosystem dynamics in the eastern equatorial Pacific that the authors fail to discuss. Papers by Friedrichs and Hofmann, 2001, Salihoglu et al 2007, Gorges et al 2005, Vichi et al 2007 are some of the papers that I can think of. Gorges et al 2005 even

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suggest that TIWS result in decreased productivity, contrary to what the others show, because the authors do not discuss these, discussions of the paper remain weak."

We thank the Referee for pointing out these papers to us. Also in response to comments from Referee 1, we have modified this part of the paper in the revised manuscript significantly and related our discussion to the findings from each and one of the papers suggested by the Referee.

"ii) The main conclusion on TIWs increasing NO₃ and Si concentrations should be supported by some mathematical/statistical analyses rather than just looking at the plots. For example Menkes et al 2006 did this by filtering data at TIW scales. First of all it should be proven that fluctuations in model vertical velocities (and in nutrient injections) are the results of TIWs, this could be done by some simple filtering. Then PP can be linked to this."

We have followed the reviewer's suggestion and performed filtering on the data. In the revised manuscript we include periodograms of all key physical and biological time series. We also perform filtering on SST and vertical velocity fields using a 20-35 day period window. These analyses are now part of the significantly improved discussion on the precise correlation between physical and biological changes.

"iii) Authors talk about downwelling Kelvin waves but why they do not mention upwelling Rossby waves?"

Considering the relatively short time period under investigation, we prefer to limit our analysis to TIW-scale processes. However, we acknowledge the fact that the potential effect of Rossby waves and not only downwelling Kelvin waves needs to be mentioned in the context of all physical mechanisms at play in this region.

Detailed comments:

"Abstract: Line 13: I find authors claim very strong that TIWs are for the very first time linked to increased nutrients as Friedrichs and Hofmann, 2001, Salihoglu et al 2007,

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Gorges et al 2005, Vichi et al 2007 and others already suggested that."

We agree with the Referee that links between TIW activity and increased nutrient concentrations had been already shown. We thank the Referee for pointing out these missing references to us. They are now included in our discussion in the revised manuscript. Our intention was to emphasize that no direct links were made between TIW activity and nutrient uptake rates by phytoplankton. Referee 1 has suggested that moving from nutrient concentrations to phytoplankton uptake is "common sense." We do not agree with that statement because there is a considerable difference between nutrient fluxes and nutrient stocks (or reservoirs) and their availability to phytoplankton (see for instance Xiu et al., 2011 and our response to Referee 1's comments). Vichi et al. (2008) linked Fe fluxes to TIW activity and phytoplankton biomass but on a temporal and spatial average that does not enable a description of the variability in these responses. We have rephrased the statement in the abstract to illustrate more clearly how this study can be distinguished from previous findings.

"Introduction: More should be given on TIWs here as this forms the main aim of the study."

We agree with the Referee and have added more information on TIWs in the Introduction section.

"Page 709, line 10: It is not necessary to repeat this type of info in the text as this is already given in table and figure captions."

We agree. The text has been removed.

"Page 711, line 9: According to table 3 there is a considerable decrease in Si uptake in Sep 2005 compared Dec 2004 whereas nitrogen (NO₃+NH₄) uptake increases considerably. Model does not capture this decrease in Si uptake, I think this should be discussed, together with its implications on PP."

This issue has in fact been discussed extensively in the paper (section 3.4. Phyto-

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plankton species composition). We concluded that the model overestimates the diatom population in the community causing an overestimated Si uptake. At the same time, cruise observations noted a large outburst of mixotrophs (heterotrophic dinoflagellates) which account for the large increase in total nitrogen uptake at the expense of diatom-driven Si uptake. While both community structures might provide very similar PP flux estimates, they are driven by very different phytoplankton dynamics. Nevertheless, we have modified the text to address the issue of opposing Si and total N trends even more clearly.

"Page 718: The discussion on TIWs start abruptly, it can be useful to have separate section on TIWs. Also it is not sufficient to look at the figures (e.g. Fig 5) to link the biological dynamics to TIWs, this should be proven by some statistical analyses."

We agree and have corrected the structure accordingly.

"Page 719, lines15-17: Is this shown?"

A reference to the figure has been added.

"Page 722, line 17: What is meant by top-down control, zooplankton grazing?"

Yes, we meant zooplankton grazing. It is stated explicitly in the revised manuscript. Also, we add new data on mesozooplankton biomass during the time of passing TIWs. This is in order to explain why under some circumstances, an increase in upwelling supply and surface concentration of Si does not trigger a visible response in diatom biomass.

"Page 722, line line 24. I suggest avoiding the usage of terms like "extremely well" when comparing model and data, as these comparisons are subjective. Data shows an increase in PP whereas model shows a decrease from December to September."

We agree and have removed that statement.

"Page 723, first paragraph. These read more like results and don't belong to the con-

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clusions.”

We have modified the conclusions section according to reviewer's suggestions.

“Page 723, line 26: Authors should be very careful when claiming that these really are the first direct evidence that TIWs alter the nutrient limitation in this region (see comments above).”

We agree with the Referee. We will modify all such statements to precisely refer to results and conclusions which vary from the ones included in all references suggested by all three Referees.

“Fig 4, captions: Lines in the fig are colour coded, whereas the captions refer to symbols.”

Corrected as requested.

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