

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

Anonymous Referee #3

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The aim of the study is on addressing the spatial and temporal variability of nutrient uptake in the eastern equatorial Pacific. The authors used a coupled circulation-ecosystem model to analyse the physical influence on the ecosystem. Most of the manuscript is devoted to model-data comparison analyses, which forms the main strength of the manuscript.

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.

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Gorges et al 2005 even 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.

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.

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

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

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

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.

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.

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.

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Page 719, lines15-17: Is this shown?

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

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.

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

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).

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

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