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Biogeosciences Discussions

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 #1

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The aim of this paper is to address the processes impacting nutrients concentration and uptake in the eastern equatorial Pacific at intraseasonnal time scale. An offline simulation, validated by data from the Equatorial Biocomplexity 2004 and 2005 cruises as well as Primary Production data from two Vertically Generalized Production Models. Tracks of TIWs have been found and their impacts on nutrients concentration and uptake are discussed.

However, despite a thoroughly made validation of the model and discussion of its performance, the results of the simulation are mostly described, and processes are not really identified nor discussed. Moreover, some assumption are made without any

justification or notice and are not consistent with the dynamical properties of Tropical Instabilities Waves (TIWs) and the associated Tropical Instability Vortices (TIVs). Finally, validation figures are sound and usefull, but the other figures that should give information about processes at play do not add much to the discussion.

The main point of the study is therefore hard to pinpoint: is it (1) that TIWs have an impact on nutrients concentration and uptake variability or (2) that TIWs associated vertical velocity induced an overall increase in term of nutrients concentration and uptake?

If 1, then this is not really a new result as it has been shown, both in the model and in the data, than TIWs would impact the variability of nutrients, Chl and planktons (e.g. Menkes et al., 2002; Gorgues et 2005; Vichi et al. 2008; Evans et al. 2009;Strutton et al., 2010...). Therefore an impact on nutrients uptake is common sense.

If 2, there is not enough material to prove this in the paper and some hypothesis discussed in previous studies are not discussed (further details in my general comments). In conclusion, I can not recommend publication of this manuscript in its present form. This study would need several new diagnostics and extensive rewriting to be publishable.

General comments:

All along the manuscipt, the authors seem to assume that a patch of high nutrients concentration or uptake is undoubtedly the result of an increase due to passing TIWs.

Indeed, the Hovmuller diagrams shows TIWs tracks clearly visible in Si uptake, but this can be the results of the horizontal advection acting on the equatorial upwelling and the front between rich equatorial water and depleted fresh water in the north. No enrichment by the TIWs could be involved, just an horizontal redistribution. For example, does the pattern of upward nutrients flux match the high concentration in figure 7 or in figure 5? A plot of the contour of the fluxes over the concentration would

answer that question.

So without any proof, the assumption of an increase due to passing TIWs is, at best, hazardous. Indeed, in their 2005 paper, Gorgues et al. (not cited) showed that the most prominent effect of TIWs is to horizontally redistribute nutrients and plankton. The overall effect of TIWs in this study was a very slight decrease of chl concentration in the eastern Pacific equatorial band due to TIWs. No fertilization effect has been stressed. Vichi et al, 2008 (not cited) showed that a fertilization due to vertical advection of TIWs would happen only if the fericline is shallower than the vertical scale of each individual TIWs (which is not always the case). Evans et al., 2009 (cited in this paper) argue that only weak TIWs induced input of nutrients through vertical advection and only if the depth of the thermocline is sufficiently shallow. In their abstract, Evans et al. 2009, stated that "Given the variability associated with TIW intensity and season, generalizing TIW effects has proven difficult. . .".

Note that Gorgues et al., 2005 and Vichi et al., 2008 are studies of the impact of TIWs on nutrients and plankton, in the same area using similar tools (biogeochemical models) and are not cited nor discussed...

The authors also showed an increase of nutrients uptake in the period of maximum activity of TIWs compared to period with weak TIWs activity. They conclude that TIWs are responsible for this increase in nutrients uptake. But the maximum activity of TIWs occurs at the same time as the seasonal maximum of the equatorial upwelling. Therefore it is not possible to conclude, in this study, whether TIWs or seasonnal upwelling variability is responsible of this increase...

Discussion about the differences in nutrients uptake between Si and N (NO3 and NH4) does not bring much groundbreaking informations. Indeed the limiting nutrient in the model and in the eastern equatorial Pacific is Si . But this nutrient limits only the growth of the diatoms in the model (see the annex). So variability in Si reflects directly on the diatoms/total phytoplankton ratio and therefore the Si uptake/ N uptake. . . A

quantification of this ratio Si uptake/N uptake is also hazardous as the authors state that the diatoms contribution to total biomass is overestimated in their model.

As a final point; the model does seem to represent adequatly the eastern equatorial Pacific, but the iron is not explicitly modeled. Isn't it disturbing to not model a nutrient which has been recognised in this area as a major limiting nutrient for phytoplankton growth rate,biomass and new production by several studies (Martin et al., 1994; Price et al.,1994; Kolber et al.,1994; Coale et al., 1996; Behrenfeld et al.,1996; Landry et al.,1997; Aufdenkampe et al.,2002).

Technical details:

Figures:

Fig2: would it be possible to do the same plot but using as reference the VGPM PP estimate?

Fig4: figure caption refers to signs that are not present in the figure.

Fig5: vertical velocity associated with the 2 TIVS passing would be usefull for some of the authors statements.

Fig7: it would be interessant to be able to colocalised the flux of nutrients with the surface nutrients concentration. Also at what depth do you take the vertical velocity use to calculate the flux?

Text:

References of TIWs as 'frontal features' (convergence) are made across the paper, which does not fit with the assumption made by the authors of an effect of TIWs through enhanced upwelling (divergence).

p702, line 5: replace 'also', I would recommend 'partly'

P702, line 8: 'these processes', I guess you are referring to 'the patterns of spatial and

temporal variability'. Those are not 'processes'

P702, line 9: replace 'combined' by 'validated'

P702, line 13: 'First time' is a bit exagerated and 'increased NO3' is either to vague or an assumption

P703, line 10: 'Chl concentrations' are not nutrients, please rephrase

P703, line 12-13: Please complete the reference as I suspect that Palacz et al, 2011 are not the first and only authors who have documented the source of nutrients in the Eastern Equatorial Pacific.

P704, line 19: COSiNE allows to do Iron budgets?

P704, line 20 to the end of the paragraph: I would insert this phrase somewhere in the next paragraph.

P705, line 10: same as p702, line 8.

P705, line 15-18: false statement, some biogeochemical model have already been used to work on the effect of TIWs on biogeochemistry (see general comments)

P705, line 18: replace 'combining' by 'comparing'

P714, line 10: "strong upwelling at the leading or trailing edge". Usually, in TIVs, downwelling occurs in the leading edge and upwelling in the trailing edge. Do you have any evidence of a strong upwelling in the leading edge of a TIVs?

P718, line 8: replace 'come from' by 'are'

P718, line 20-22: again assumption. See Kennan and Flament, 2000: colder SST does not correspond to strong upwelling in tropical instability vortices. See their figure 23 (cold water at the leading edge, i.e. the downwelling region). A plot of vertical velocity over the sst or chl in figure 5 is at least necessary if you make that statement. 'Higher Chl' higher than what?

P719, line17:the 'maximum of TIWs intensity' happen to be at the same time than the maximum intensity of the equatorial upwelling. How do you disentangle the impact of TIWs and the impact of seasonal variability of the equatorial upwelling on NH4 uptake ???

p720, line2-4: Again, how can the authors make such a statement? How can they disentangle the impact of TIWs and the impact of seasonal variability?

P723, line 26-29: No evidence has been shown whatsoever than TIWs, on their own, supply large significant amount of Si. Pure speculation.

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