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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

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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:

"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)."

We are grateful to the Referee for pointing this lack of clarity to us. We agree that we have not made the point of the TIW analysis part of this study clear enough, nor did we indicate sufficiently how this study differs from previous ones that consider the effect of TIWs on biological production. Consequently, we have modified this part of the paper substantially to refer in detail to previous publications on this topic and to emphasize that the goals of this analysis are twofold:

1. To examine patterns of model nutrient and phytoplankton response to TIW activity in the absence of plankton advection, and compare the results with cruise and remote sensing observed patterns of productivity. This study is thus a synergy of a model sensitivity study and model-data comparison used as a reality-check on the proposed link between TIW activity and variability in biological production. This paper takes advantage of one of the main functions of computer models, that is to 'turn off' one of the factors in play (here horizontal advection of plankton) and inspect how the system behaves in its absence. In a way, Gorgues et al. (2005) used a similar approach when they 'turned off' TIW-scale processes affecting mean biogeochemical budgets in this region. Here the aim is to verify whether in situ growth of phytoplankton (especially away from the equator) can occur during TIW events as opposed to phytoplankton biomass being advected away from the equator. We recognize that we did not emphasize this point in the original manuscript. Our results are matched with cruise data when available and with satellite primary productivity data on a full spatio-temporal domain. In the revised manuscript, we strengthen the quantitative aspect of the comparison with satellite data. Moreover, we strengthen the link between changes in the physical and nutrient fields caused by TIW dynamics and the corresponding biologi-

cal responses. These results, constrained by the limitations and assumptions of this approach, suggest that in absence of horizontal advection of plankton, the model can simulate patterns of primary productivity similar to the observed, even away from the equator. In the revised manuscript we include an extensive discussion that relates our results to findings of Gorgues et al. (2005) and Vichi et al. (2008) among many others whom all three Referees pointed us to. Using a more in-depth analysis of TIW dynamics we now also better support our claim that TIW-related perturbations in the physical field can fertilize the surface waters locally and stimulate in situ growth of phytoplankton.

2. To illustrate patterns of variability in instantaneous biological flux/rate responses to TIW activity and not the mean effects of TIWs on primary productivity, as for example in Gorgues et al. (2005). In the revised manuscript, in line with our model-data comparison, we focus on narrow spatio-temporal windows relevant to the EB cruise domains and periods of most intense TIW activity. This allows us to examine the variability in instantaneous biological flux/rate responses rather than time and spatial average effects on production or phytoplankton biomass that have been already performed extensively, as the Referee correctly stated. In our view, this study is a valuable attempt to provide a link between TIW activity and phytoplankton activity from a flux rather than a reservoir (stock) perspective used in Strutton et al. (2001), Ryan et al. (2002), Gorgues et al. (2005) and others before us. We recognize the fact that a link between TIWs and nutrients and chlorophyll was made long before, but we disagree that this makes vertically-integrated instantaneous phytoplankton nutrient uptake rate estimates coincident with passing TIWs redundant. Finally, this is the first time to our knowledge that we can track TIWs in a model and at the same time compare model rate/flux estimates with those measured in situ. Several papers from the Equatorial Biocomplexity special issue emphasized the importance of direct biological rate/flux measurements as opposed to inferring this information from standing stocks (e.g. Kaupp et al. (2011), Parker et al. (2011), Krause et al. (2011)). The significance of distinguishing between flux and standing stock estimates of biological production was also recently described

for example by Xiu et al. (2011) who studied the effect of eddy-induced iron fluxes on biological productivity vs total production in the Gulf of Alaska. Majority of previous modeling studies that analyzed variability in biological fluxes in response to TIWs did not evaluate their results with in situ and/or satellite measurements. The ability to perform a 'reality check' in our study is considered an important aspect of this study.

"All along the manuscript, 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."

We agree with the Referee that we have not provided sufficient results that could distinguish between the two processes. In this model exercise we assume that there is no horizontal advection of plankton. Therefore, we can also assume that all phytoplankton uptake rates correspond directly to the model depth-integrated phytoplankton biomass. This is not a realistic setting but one that allows us to test if in the absence of advection, patterns of productivity can be matched with the observed. On the other hand, the model does resolve advection of nutrients and it is thus possible that increased nutrients are due to their horizontal redistribution as the Referee suggested. In the revised manuscript we introduce new diagnostics to distinguish between vertical and horizontal nutrient supply during TIW events and discuss our results with respect to findings of Gorgues et al (2005), Vichi et al. (2005) and Evans et al. (2009) among others.

"No enrichment by the TIWs could be involved, just a 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."

We agree with the Referee that we could not distinguish between the two process based on the results presented. In the revised manuscript, we follow the Referees suggestions and analyze the relative position of surface nutrients and nutrient upwelling fluxes. We have replaced Figures 5 and 7 with new ones that will illustrate these points more clearly.

"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)."

We thank the Referee for pointing those papers to us. We have improved our discussion by referring to those findings extensively in the revised manuscript. Here we would like to briefly mention how we consider our study different from the two mentioned. The results of both Gorgues et al. (2005) and Vichi et al. (2008) reveal important mechanisms through which TIWs affect patterns of biological production but on very different scales. Gorgues et al. (2005) looked at long-term mean budgets which average over a lot of the variability that we want to focus on in this study. Vichi et al. (2008) on the other hand base their discussion on iron – one nutrient whose budget in the equatorial Pacific is extremely poorly constrained. Theirs is essentially a single-variate analysis of a multi-variate problem (where more than just iron affects rates of phytoplankton growth and primary production as a whole). In the revised manuscript we want to emphasize that this is not the most adequate approach to describe a full phytoplankton community response to TIW events. We base our study on nutrient concentrations and nutrient uptake rates that are carefully evaluated with EB cruise data. This includes silicic acid which is co-limiting diatom growth in this region (Brzezinski et al., 2011). Finally, we extend the discussion onto the role of initial biological conditions at the onset of TIW events, e.g. state of mesozooplankton that was shown to determine the biological response to iron enrichment experiments (Tsuda et al., 2007; Fujii and Chai,

2009).

"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"."

We agree with the statement completely. In the revised manuscript we look carefully at case studies that demonstrate this variability and place our results in the context of the conclusions derived by Evans et al. (2009). We have also modified our conclusions to reflect the fact that our results are supporting the fact that biological responses to TIW activity cannot be explained by a single mechanism.

"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..."

We thank the Referee for bringing our attention to these papers. We have revised the paper extensively to refer to their findings and to comment on the differences and similarities in chosen approaches and obtained results.

"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..."

We agree with the Referee. We have made an attempt to separate these processes by filtering out TIW-scales of variability only following the suggestions of Referee 3. In the revised manuscript we compare the filtered fields with unfiltered ones, and focus on much narrower spatio-temporal windows to constrain the analysis to a single TIW season (e.g. Fall 2004).

"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 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."

We strongly disagree with the Referee. This discussion is key to explaining why it is possible to obtain similar values of primary productivity while misrepresenting new production (or production controlled by a limiting nutrient). If this is the case, then it is essential that all models used to study the role of TIW on biology have a correct representation of the phytoplankton community composition. If models used by Vichi et al. (2008) and Gorgues et al. (2005) had a disproportionately large population of diatoms (as does CoSINE, especially in Fall 2005), then they would have potentially overestimated the role of iron limitation in controlling total primary production. None of the current models distinguish between diatoms and heterotrophic dinoflagellates within the large phytoplankton size class, and in light of the EB cruise results, are likely not to capture the rapid changes in nutrient cycling during and immediately after a passing TIW. Although NH4 uptake cannot be related to TIW-induced nutrient supply, its strong contribution to total production cannot be disregarded when matching patterns of primary productivity with TIW-induced perturbations in the nutrient fields.

"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)."

We agree with the Referee that Fe is a very important limiting nutrient in this region. However, in the absence of sufficient data to validate iron cycling in this region, adding this element to the model would not necessarily provide a more realistic representation of the biogeochemical dynamics in response to TIWs. If Fe and Si are in fact co-limiting diatom growth in this region (Brzezinski et al. 2011), then a lot of the dynamics can be explained by the role of Si regulation. Moreover, there is a lot more Si data to evaluate the model with (Dugdale et al., 2007; Parker et al., 2011; Krause et al., 2011). On the other hand, we agree that only a combination of Fe and Si limitation would provide a complete setting for modeling these processes, and it is the goal to implement iron cycling into the CoSiNE model in the near future.

Technical details:

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

It would be possible to do this however we prefer to maintain the EB values as a common reference. We do however include the VGPM PP estimates as additional points in the target diagrams which allows to compare all 3 estimates at the same time.

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

This figure (which came from an online run that included advection of plankton) has been replaced by another one that includes a broader range of diagnostic variables from the offline run that excluded advection of plankton. Vertical velocity is plotted next to other parameters in all new figures.

"Fig7: it would be interesting 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?"

Vertical velocity was so far calculated at 75m depth because that is on average the depth of maximum vertical velocity in this region during 2004-2005 (Palacz et al., 2011) and because that is also the depth over which we integrate biological fluxes. In the revised manuscript we follow the Reviewer's suggestion and look at spatial matches and mismatches between surface nutrients and vertical fluxes at 75m but also look at the variability in the depth of maximum vertical velocity during passage of TIVs at 2N.

"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)."

We agree with the Referee. We have corrected these statements.

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

CoSiNE does not allow to do iron budgets itself. The study we refer to combined ROMS-CoSiNE physical and nutrient fields with estimates of iron concentration from all measurements during EB04 and EB05 cruises. This approach allowed to constrain iron budgets over this narrow time scales under some large assumptions. We modify the statement to make it more accurate.

"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?"

In the revised manuscript we zoom in on individual TIVs to inspect this more carefully. We agree with the Referee that downwelling prevails at the leading edge. However, we observe a significant shoaling of the depth of maximum vertical velocity that coincides with maximum increase in surface nutrients. We add a discussion on this in reference to all the papers cited by the Referee.

"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???"

We agree. We have removed these statements and provide more analysis that can potentially disentangle the different scales of variability.

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

We agree that we did not support our statements sufficiently. We have changed this part of the manuscript significantly to address this and all others comments in detail.

In addition, we will modify the manuscript to account for all other minor technical comments the Referee pointed out.

New references:

Fujii, M. and F. Chai (2009): Influences of initial plankton biomass and mixed-layer depth on the outcome of iron-fertilization experiments. Deep-Sea Research II, 56, 2936-2947.

Tsuda, A.,Takeda, S., Saito, H., et al. (2007).Evidenceforthegrazinghypothesis: grazing reduces phytoplankton responses of the HNLC ecosystem to iron enrichment in the western subarctic Pacific (SEEDSII). Journal of Oceanography 63, 983–984.

Xiu, P., A.P. Palacz, F. Chai, E.G. Roy, and M. L. Wells (2011): Iron flux induced by Haida eddies in the Gulf of Alaska. Geophysical Research Letters, 38, L13607, doi:10.1029/2011GL047946.

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