Review of "Physical and biogeochemical forcing of oxygen changes in the tropical eastern South Pacific along 86 W: 1993 versus 2009" by Llanillo et al., 2012.

The manuscript by Llanillo and coauthors describes changes in water mass composition and biogeochemistry along a meridional section crossing the South Eastern Tropical Pacific (SETP) Oxygen Minimum Zone (OMZ) between two opposite phases of ENSO.

OMZs are especially important for ocean biogeochemistry (they are hotspot of N-cycle transformation) and ecosystem (they limit the habitat of many marine species). The distribution and oxygen depletion of OMZs respond to changes in ocean circulation that occur on multiple timescales, from seasons to decades, including possible changes linked to anthropogenic warming. Characterizing the response of OMZs to these climatic forcing is critical, especially in light of the predicted deoxygenation as the oceans warm up.

Thus, the study of Llanillo and Coauthors is timely and welcome, since it investigates the physical and biogeochemical changes of a major OMZ arising from the strongest mode of variability in the equatorial/tropical ocean. The Authors start from observation along two occupations of the meridional section to calculate the contribution of different water-mass end-members, and further quantify the role of water-mass redistribution and biogeochemistry in driving the observed oxygen and nutrients variations. The picture that emerges is qualitatively consistent with the physical changes expected for ENSO-type variability; the study adds a quantitative understanding (with some caveats) to the picture.

The manuscript is well structured and written, it presents new data and analyses, and represents a useful contribution to a relevant topic. Overall, I feel that some points could be further discussed and clarified to improve the paper and increase the confidence in the results shown.

Major points:

(1) I wonder if the OMP technique as implemented by the Authors produces an estimate of the error associated with the water mass fraction that could be included in the figures (similarly to the white shadings in Fig. 9). This would help focusing the attention to the parts of the section where the OMP results are most robust/reliable.

(2) The characterization and quantification of the water mass contribution within the oceanographic section relies heavily on many of the OMP assumptions. The uncertainty stemming from these assumptions could be addressed in more detail. Among these are: (a) choice and number of end members; (b) spatial variations in end member properties (c) temporal variations in end member properties; (d) errors due to the choice of the fixed stoichiometric ratios. I understand that point (a) is limited by observations (4 conservative

tracers, once remineralization and denitrification are factored in, allow for 'unmixing' of at most 4 water masses). However, I wonder if the results of a Montecarlo-type perturbation (that the Authors mention in page 17591), or some other approach, could be used to asses the robustness of the methods with respect to the uncertainties in (b-c), and possibly be considered for an extended error estimate as discussed in point (1).

(3) I was a little bit surprised to find out that the ESSW end member is a water mass that falls right within the domain of the study. This is somewhat strange, as if possible one would rather include an end member that originates at the boundary of the region considered - if not at the surface. Not surprisingly, the result of the OMP analysis is that the ESSW makes up the bulk of the water-masses inside the section - up to 80-90 % where the properties are closest to the end-member itself. That also brings into question the usefulness of this end member for the biogeochemical estimates, as it imposes 'preformed' properties within a large volume of the section domain, preventing an analysis of their origin (see Fig. 6). Furthermore, I wonder if the choice of this dominant 'internal' end member might bias some of the biogeochemical conclusions. I am worried of possibilities similar to the following case: let's say that water mass structure does not change between the two occupations, and only biogeochemistry changes (e.g. export/remineralization). By picking the EESW end member, and assuming its preformed biogeochemical properties are constant, any biogeochemical change in the bulk of volume dominated by EESW would show up as a change in overall water-mass contributions, even if that was not the case to start with. Obviously this is a limit example that would not apply exactly to this study (water mass do change between different phases of ENSO). Yet it suggests a potential source of error that might be difficult to address.

(4) I'm not particularly convinced by the assumption of a constant stoichiometry for silicate. As discussed in point (1) adding silicate data to the OMP analysis allows 'unmixing' of an additional water mass. However silicate dissolves with a typical e-folding depth-scale of ~1500-2000 m, compared to ~200 m for organic matter. Therefore silicate would hardly have show a constant ratio with P and N during remineralization/dissolution. This assumption should be at least discussed - if not explicitly addressed in the OMP method.

(5) The Authors should discuss the possible uncertainties/biases arising from the choice of breaking down the OMP analysis at the 450 m depth horizon. I am somewhat concerned that some of the sharp features that are seen in Fig. 9, especially 9a-d, where the changes show a rather abrupt sign change right around 450 m, might be related to this choice. This could be a coincidence, but I wonder if the same features would show up if the break were moved up or down in the water column.

(6) I agree with the importance of showing the results in physical (z) space, especially the biogeochemical changes between occupations (Figs. 7-9). This choice is obviously more intuitive, and more relevant for OMZ volume, nutrient storage, and overall biogeochemical rates. However, the same analysis in density space would shed light on the relative role of isopycnal heaving (possibly the largest source of variability across ENSO phases) versus in situ biogeochemical and remote water mass transport changes. See discussion in pp. 17598, ll. 16-25. This could be shown as supplementary material if space is a concern.

(7) The change in the contribution of PDW (Fig. 9) is somewhat puzzling. I would intuitively expect the PDW fraction to increase during a La Nina/La Vieja phase, as denser waters move upwards and upwelling strengthens. I wonder about the robustness of this result.

(8) In the discussion of the biogeochemical changes the Authors often refer to "increase/ decrease" or "strengthening/weakening" of remineralization and denitrification. I am uncomfortable with this terminology as it could lead to interpreting the results in terms of increase or decrease of remineralization/denitrification rates. However the method results can't be interpreted in terms of biogeochemical rates - but only in terms of biogeochemical signals signals. For example, the tongue of increasing nitrate deficit observed in the southern side of the section at ~200 m depth (Fig. 9e) does not necessarily mean that denitrification increased across the two occupations. In fact it might just be the signal of increased advection of waters from the peruvian shelf - where I assume that this nitrate deficit originates to propagate offshore - even with no change in overall denitrification rates. Saying something about biogeochemical rates would require at least an estimate of the water mass ages. This should be made clear in the discussion of sections 4.3 and 5 (e.g. pp. 17600-17602) as well as in the Abstract.

Minor and technical comments:

(9) I think it is important and useful to show the oxygen (and possibly nitrate) sections especially before discussing the biogeochemical components and the changes in Figs. 6, 7, 9. Perhaps this can be done as supplementary material.

(10) I would rather refer to the "advective" component directly as "physical" component. As the Authors recognize (p. 17599, ll. 23-24) it includes isopycnal heaving, diffusion, advection, so just referring to it as 'advection' - especially in Fig. 9, is somewhat misleading.

(11) Abstract. It could be useful to add a succinct explanation of the OMP technique for readers not familiar with it.

(12) Table 1. Is the "Mass conservation" column necessary?

(13) Possibly show a figure equivalent to Fig. 6 but for the zonal sections in 2009 (if not in the main paper, as supplementary material). That would shed some light on the origin of some features seen in Fig. 6, for example the shallow (~200 m) denitrification tongue originating at the southern side of the section.

(14) p. 17589, l. 18. I don't understand this reference to "calcification" in the context of nutrient and oxygen cycling.

(15) p. 17593 ll. 12-13 it looks from Figs. 4-5 that ESSW (not just STW and SAAW) represents a big contribution even in the shallower layers.

(16) pp. 17595, ll. 13. I would question the use of the term "ventilation" in this instance, especially considering that the EESW end member falls inside the section and is already extremely oxygen-depleted. Intuitively ventilation should refer to surface end members.

(17) pp. 17595, ll. 21. Please clarify what is meant with "ancient denitrification".

(18) pp. 17599, ll. 26-30. The increase of AAIW doesn't seem to univocally result in an advective oxygen gain. For example the match is not that clear in Fig. 9a: around 7°S, ~350 m depth, where the maximum increase of AAIW is observed, advective oxygen is actually seen to decrease. Also see point (5).

(19) pp. 17602, ll. 9. I am not sure what the Authors mean with "a natural negative feedback of the ocean circulation against the long term trend of expanding OMZs". The signals that the Authors describe can be easily accommodated within the framework of interannual/ multidecadal variability, which could also be the driver of the observed OMZ expansion.

(20) pp. 17602, ll. 15. Please clarify "differently" (e.g. non-stoichiometrically?)