Ocean Sci. Discuss., 12, C1277–C1280, 2015
www.ocean-sci-discuss.net/12/C1277/2015/

Ocean Science Discussions
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Interactive comment on "Impact of vertical and horizontal advection on nutrient distribution in the South East Pacific" *by* B. Barceló-Llull et al.

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Received and published: 4 December 2015

This study is mostly about examining the significance of mesoscale vertical velocities. The significance test chosen by the authors involves a comparison of the roles played by horizontal and vertical mesoscale velocities in the evolution of an initially smooth subsurface nitrate field.

The general idea of the manuscript is interesting and the subject of vertical velocities is of major importance. The manuscript is also well-written and clear for the most part. Having said that I am not convinced that the specific Lagrangian experiment carried out by the authors is meaningful. I am in turn not convinced by the significance of the quantification the authors provide based on that experiment (that "... vertical velocities

explain approximately 30% of the nitrate distribution"). I also disagree on some specific conclusions that accompany this result. Overall it seems to me that the authors focus their study on the effect of (a small fraction of the) vertical velocities on nitrate but their simulated nitrate dynamics has nothing to do with that of the real ocean (initial distribution and dynamics are two different things). I therefore recommend a major revision of the manuscript, with some more specific comments to help the authors below. My suggestion would be to redesign the experiment so that the simulated nitrate dynamics is more directly related to that of nitrate in the real ocean. However, there may be other ways to revise the manuscript and make it more scientifically significant.

Major comments:

- As discussed for example in the manuscript conclusion (p 2268 l7-10), what matters is the consequence of vertical velocities in terms of introduction of nutrients into the euphotic layer. With this in mind, I do not find the experiment protocol particularly well designed. The choice of a uniform depth range for the release of the particles implies that over most of the domain particles are situated well above the euphotic layer and nutricline, in places with almost zero lateral and vertical gradients. This choice does not seem judicious because both horizontal and vertical advection will only have minute consequences on the nitrate field, hence the authors compare the relative roles of two terms that each have a negligeable role in the nitrate budget (note the tuning of the colorbar in figures 6 and 7 to display some contrasts in the southwestern sector of the domain). I do not understand why the authors have not released their particles about the nutricline to obtain a fair comparison between all parts of their domain ?

- In contrast, nitrate depletion in the euphotic layer is ignored in the study whereas this term is critical to establishing some asymmetry between upward and downward nitrate fluxes. Speaking of that, a key time scale for nitrate evolution in the real ocean is that for depletion by primary production. This scale varies depending on the situation but it is frequently considered to be of the order of a few days. Although it is presumably longer in the oligotrophic SEP it is a pity that vertical velocities are weekly averaged

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and hence, do not resolve the presumably larger w fluctuations on time scales of one to a few days that are susceptible to interact with primary production. In the event that the authors would model the nitrate evolution with a term accounting for primary production in the upper ocean this would be a remaining limitation of their study.

- the statement that "the vertical velocity explains about 30 % of the nitrate distribution" (p2267 lines 9 and 24) is litterally false. The vertical velocities are responsible for creating nitrate variance or changes whose level after 30 days is 1/3 of that created by horizontal advection alone. Nitrate distribution remains controlled by their initial condition to a very large degree.

A prototypical example of the confusion that plagues the manuscript is the statement made in p2268 lines 7-14 about the fact that small vertical velocities in the SEP make an important contribution to marine ecosystem growth: "This is demonstrated by our analysis since the most important contribution of the vertical velocity on nitrate distribution is seen to be localized to the eastern part of the SEP which is characterized by moderate vertical velocity values and a high vertical nitrate gradient"). Note first that, indeed the south-eastern Pacific is the only place where particles release positions straddle the nutricline. Irrespective of the vertical velocity intensity this is thus the place where some vertical advection effects can be expected and I do not see what this proves except the fact that advection can only act on existing property gradients. In addition, the results obtained by the authors do not show nutrient injection into the euphotic layer, they show positive and negative changes in nitrate which must approximately cancel out (see the noisy red and blue patterns in Figure 8). It is only by computing fluxes that the role of vertical velocities on ecosystems can be demonstrated. On the other hand, a nutrient sink would be needed to deplete nutrients in the euphotic layer and create some asymmetry between upward and downward fluxes.

Specific comment:

p2262: dynamic heigth dh should be defined in the text.

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p2263: primed density should also be defined.

p2265, lines 15-16: "Further, this result does not give any indication of the sign of the vertical motion, only its magnitude.". What sign ? The vertical velocity sign a changes rapidly with time. So I do not believe that this remark is useful.

Interactive comment on Ocean Sci. Discuss., 12, 2257, 2015.