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Biogeosciences Discussions

Interactive comment on "Zooplankton diel vertical migration and downward C into the Oxygen Minimum Zone in the highly productive upwelling region off Northern Chile" *by* P. Tutasi and R. Escribano

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Response to reviewer 1

Referee #1 Received and published: 29 April 2019

General comments: Tutasi & Escribano have collected a valuable dataset on zooplankton vertical distribution off Chile and have used it to estimate the zooplankton-mediated vertical carbon flux. As such, this is a valuable contribution to ongoing efforts to better constrain the different components of the biological carbon pump in the ocean.

However, as is I cannot recommend publication in Biogeosciences because of several shortcomings in the analyses and presentation of the results.

R: We appreciate this general comment. However, we think that eventual shortcomings can be solved by providing some further support to our analyses and also adding some missing information to the MS that we recognize as important to sustain our findings and conclusions. From here and thereafter we have sorted the reviewer comments as to reply separately to each one of them.

Most importantly, it is not stated in the paper how the major outcome of the study (the active carbon flux, with a mean of 678 mg C1 BGD Interactive comment Printer-friendly version Discussion paper C m-2 d-1) is calculated based on the biomass of the different functional groups. The authors claim that this term includes respiration, faecal pellet production, and mortality, but they never present how they estimated the different terms.

R: We estimated C flux based on previous works dealing with similar zooplankton groups. We agree with the reviewer that more detailed references should be provided to support the approaches and estimates. Therefore in our revised version we are including the following paragraph in the Methods section:

"To calculate the active C flux at each sampling station we used a daily respiration fraction of 0.12. This respiration rate was estimated by Hernández-León and Ikeda (2005) for total zooplankton biomass at temperatures ranging between 13°C and 18 1C for mid-latitude areas. We used this value considering a similar temperature range between surface water and within the OMZ (Fig. 2). The daily contribution of egestion rate to C flux was assumed to be 0.09 of migrant biomass as suggested by Escribano et al. (2009). This estimate was derived from a combined biomass of large-sized copepods and euphausiids which are the major groups contributing to migrant biomass. The contribution of mortality to C flux was assumed to be 0.08 day-1 of migrant biomass as a conservative estimate suggested by Ohman and Wood (1996). We thus estimated

total C flux as, C Flux= (0.12*(MB) + 0.08*(MB)) / 2 + 0.09*(MB), (3) where Rz, Mz, and Ez, are estimates of fractions of total migrant biomass (MB) for a 12-h period."

Were environmental data (temperature, depth, oxygen?) included in the scaling of metabolic rates? Up to now the presentation is only comprehensible in terms of abundance distribution.

R: Our estimates are based on parameters derived from ranges covering the temperature range observed in our sampling stations. We did not scale values for depth or oxygen concentration and the eventual effects of these environmental factors cannot be discarded, and so that we have included this as an important point for Discussion. For this, we raised the issue saying that further work (possibly modeling) is needed to asses as time-varying potential effect of an oxygen gradient on respiration, and physiological studies suggest a depressed metabolism upon low-oxygen, but ETS estimates (the suitable method to measure respiration at depth) do not account by such effects. With respect to oxygen or depth effects on egestion rates we have no available information and because animals may produce fecal pellets rapidly after feeding, but they can keep production at depth we reduced estimated rates in about 50% as suggested by Escribano et al, (2009). This information is now provided in Methods.

Even for biomass the "published regressions" that are used to convert from image-derived biovolume of the different taxonomic groups are mentioned, but not cited.

R: We agree that such information is important to be provided, but we actually did not use published regressions to estimate biomass, so this sentence has been removed in our revised version. Instead, we are now adding a Table (as Supplemental Material) with all the conversion factors between dry weight and C content.

To assume a density of 1 and a constant water content of 90% across taxa evokes a large error (and actually contradicts the results of the cited paper, Matsuno et al. 2009 who used this paper based upon the results by Yamaguchi et al. 2005 and found that it did not fit well).

R: The issue of converting body size (length, area, volume) into biomass and C content has received attention in several published works, and revising the literature does not lead readers to a unique Method or approach. We agree that different animals may exhibit different body densities and water content, and this variability can also occur with different life-stages. A body volume converted to wet body mass with density=1 has been widely assumed in many studies using taxa-specific masses (e.g. Yamaguchi et al. 2014 DSR-Part I, Sato et al. 2015 Zool. Studies, Donoso et al. 2018 JGR). This unique relationship is sustained by an early work of Wiebe (1975) who reported the relationship based on a large and varied zooplankton community. Also, the average water content for zooplankton as approximately 10% has been largely used in several published works (Yamaguchi et al. 2014, Sato al., 2015). It may be possible that some specific taxa may introduce underestimates or overestimates of body masses, but not exact conversion factors do exist for each taxonomic group, so average conversion rates appear as a conservative approach. The issue has received much discussion in several papers and no agreement or unique approach is currently adopted. In any case, we have now considered the issue to be part of the discussion.

Why not use taxon-specific regressions for direct conversion from image area to carbon such as those pubslished by Lehette and Hernandez-Leon 2009? I therefore recommend major revisions of the paper, where it is absolutely critical to resolve the above-mentioned issues. Since this will involve generating new figures, tables, and rewrite the results section, it might be more practical for the authors to withdraw the contribution and resubmit as a new MS.

R: Lehette and Hernandez-Leon (2009) provide estimated regression equations for different zooplankton taxa between scanned area and biomass (C content). This is a direct relationship between size and C content which is actually the key parameter needed for estimates of migrant biomass and flux of C. We do not question such relationships by Lahette and Hernandez-Leon, but they are not suitable for our taxonomic composition and do not account for variable C-specific contents (which are

independent of size). Instead, we used taxa-specific size and C content conversion factors previous published works which are suitable for our taxonomic groups and for the study region. We are now providing this information as Supplemental Material for readers to assure we are using correct factors. I hope my suggestions help in the process. Specific comments The quality and conciseness of the text is very different between different parts of the paper.

R: We have fully revised the text to improve the manuscript

The introduction as well as the materials and methods section are reasonably well written (except that in the M&M the calculations for biomass and active flux are entirely missing).

R: We have greatly modified the M&M providing now the required and detailed information regarding the estimates of C contents by taxa and the estimates of C fluxes.

I have added some suggestions to a marked-up version of the pdf. Both the abstract and the discussion need some work, but the largest room for improvement is in the results section, and includes the quality of the text, figures, and tables.

R: We thank the reviewer by his(her) valuable comments and suggestion to improve the MS. We have considered all the comments and suggestions from the marked pdf file.

Please find some specific comments below. Throughout the results, the text needs to be shortened and rewritten. As a start, delete all meaningless filling words such as that some variable “showed to”, “appeared to”, etc. Also, the taxomomic group names are sometimes a bit awkward, e.g. for “Egg Fish” (I assume this is because in your sorting there is also Egg Other) fish eggs would C2 BGD Interactive comment Printer-friendly version.

R: We have now revised and re-written Results.

Discussion paper be more natural, for Nauplius Larvae simply nauplii would be shorter,

and Ctenoforos and Ictioplankton in English would be Ctenophora and Ichthyoplankton , respectively.

R: Agreed now changed

I have not marked up the results text in the pdf because I feel they really should be rewritten, and also I recommend many changes of the tables and figures that will affect the text. All figures, tables, and text: I cannot follow the decision to define the “most important” zooplankton by number instead of biomass. Neither Acartia nor nauplii contribute substantially to total biomass, let alone migrant biomass. On the other hand, salps , chaetognaths, decapods and euphausiids do (Table 5).

R: We thank the reviewer for this comment. We have followed the suggestions and revise all graphs and tables. This has also implied recalculations of our estimates as it will be evident in the revised version of the MS.

It is unclear how the authors deal with uncertainty (i.e. variability between replicate net hauls) and spatial variability (differences between stations, which may be related to productivity differences and/or OMZ characteristics).

R: Our new Tables are now including standard deviations fro our estimates derived from the replicated samplings. In Methods we now explain how mean values and their associated errors are estimated when replicates measurements are available.

In table 6, a single estimate is presented with some error. There should be a table summarizing the results of the statistical comparison between stations.

R: A new Table is now presenting our estimates of migrant biomass with their associated standard errors, while statistical testing is described in the text.

Vertical zonation: the zonation as indicated in the hydrography plots (Fig. 2) does not match at all with the one indicated in figs 3 and 5, and is again different in figs 4, 6, 7 and 8. In fact, you call the 150-400m stratum “OMZ-LC” in the latter but according to Fig 2. this would be OMZ-UC. However, I think your Multinet depth intervals were well

chosen for the given conditions. Why not just call the five depths Oxygenated Layer (0-30m), Oxycline (30-90m), Upper OMZ (90-150), OMZ Core (150-400) and lower OMZ (400-600), then add a table with the mean and range in oxygen and temperature for the respective depth intervals at the three stations? From the plots it seems this should work.

R: We agreed with the reviewer and consider that is more clear using same pre-defined strata coinciding with sampled layers. Therefore, all the graphs have been redone in according to sampled strata.

Also please add the multinet depths to Fig.2 as horizontal lines (Figure attached). It makes little sense to use the variable definition according to Paulmier at al. if you cannot resolve it with the net anyway (because you never know where exactly in the depth layer all the specimens were caught within a given stratum).

R: Agreed, done

Hydrography of station T6: Initially I had assumed this nearshore station was only 350 m deep. The lack of CTD data needs to be noted somewhere (I assume gear failure), please clarify. Agreed. We have clarified this in Methods and avoid referring to oxygen condition below 350 m Also, it is unclear to me how the vertical zonation was done for this station (according to Figure 2, OMZ-LC is absent and OMZ-LW is present in the anoxic core). Because O2 data are lacking from the lower OMZ boundary (i.e. it is undefined where the water column begins to re-oxygenate), it is not valid to classify the two lower zones at all (unless you follow my recommendation above and assign them to the respective nets, arguing that the 400-600 m interval includes the OMZ base, which can be shown from other observations).

R: Agreed, as explained above

Table 1: As is, a lot of space in this table is taken up by redundant information (Lat, Lon, Sampling Depth). Since you made an effort to stay well out of the migration times

at dusk at dawn, I also think the times are not of crucial importance. I recommend to move this table into a supplement and add to the methods text that you sampled four day/night pairs at T3 and T5 and two D/N pairs at T6. By the way, the nomenclature of the stations makes little sense to the reader, why not just call them either stations 1-3, or north inshore, north offshore and south inshore?

R: OK Table 1 I now supplemental material. We prefer to keep and T3, T5 and L6 because they are part of a Research Cruise from which other studies will refer to same stations.

Table 2: It makes little sense to use daily means (day- and nighttime data combined) for the vertical zonation data (because of DVM). Also, absolute integrated values would have been more meaningful to the reader than relative. Actually, I would have found it most informative to have a table with all taxa and total abundance (ind m-2) as well as biomass (mg C m-2) at the three stations. The vertical distribution can be shown in a figure (Fig 4).

R: We agree with this comment and now made all estimates with integrated values in the 0-90 m layer to estimate migrant biomass. We now do not combine day and night values. The suggested Table is also included in our revised version

Table 5: this table is informative and to me the key result of the paper. Error estimates should be added based upon the replicate sampling at each station.

R: We agree and now added standard errors

Table 7 (and related text): here you make an effort to relate zooplankton abundance/biomass as well as DVM-mediated flux to primary production, which is a nice idea, but the “10 000 mg C m-2 d-1” value (which seems to be taken from the Daneri paper, although I am not quite sure from where and why) seems a quite random choice and does not account for station differences. How about using satellite-derived PP instead (I know cloud cover is an issue in the region, but maybe an monthly mean for

the respective station?), or was there a fluorometer mounted on the CTD to be able to compare integrated chl-a values between stations?

R: We agree with the comments and estimated net PP from satellite for the same sampling period and for stations. We used these satellite estimates of PP to assess the proportion being potentially exported by active transport.

Figure 4, 6, 7 and 8: To plot the different stations in one vertical distribution plot is visually misleading. First, the color codes are not well discernible (except Fig 8), but more importantly the depth distribution is not well represented. I suggest to use one plot per station, to make the y-axis (Depth) linear and to make the bars as wide as the depth layer. In this way, the area of the bar will represent the integrated biomass (or biomass difference) in the respective layer. Either simple bars with error bar can be used or stacked bars if several groups shall be represented.

R: We agree with this comments and we made new figures following these suggestions Overall I recommend to show biomass, not abundance, and to focus on the groups that are important biomass wise (Fig.5), not abudance-wise. Figure 9: It is unclear to me what information this figure should convey. Caption says grey bars represent major zooplankton groups. There are no grey bars. Why do “non migrants” have a positive rate throughout? What is the red dashed line? Why are there no error bars? Also, it is virtually impossible to visually compare stations, because the shown taxonomic groups vary between panels.

R: We agree that this Figure was somehow confusing and decided to build a new Figure using only biomass and not abundance (new Figure 5 attached as example) .

Technical corrections I have added some corrections to a marked-up version of the pdf. These are not comprehensive, because I think these type of corrections will be done in the second review stage after a substantial rewriting.

R: We have revised and considered all suggestions and corrections from the marked

text.

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2019-127, 2019.

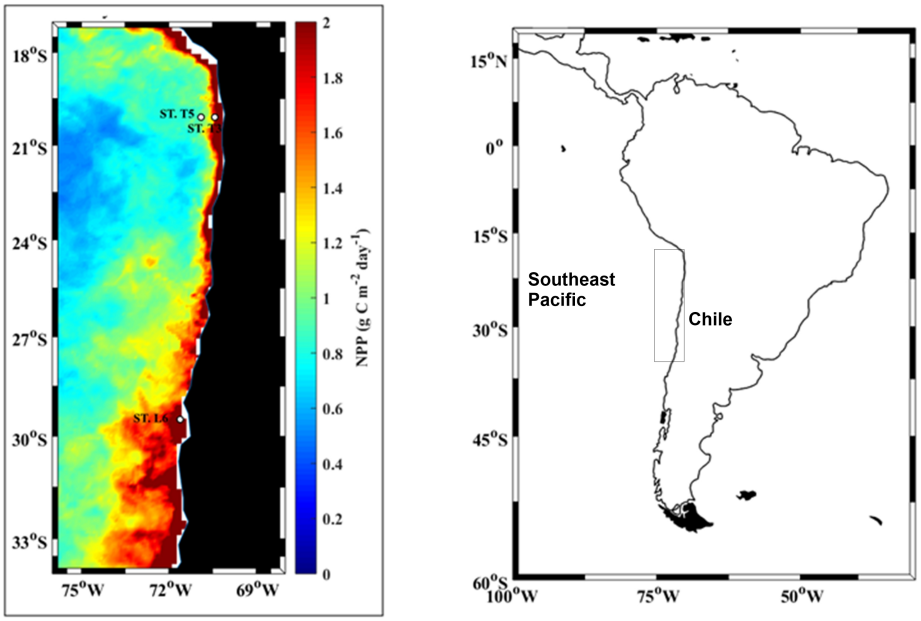

Fig. 1.

Figure 2

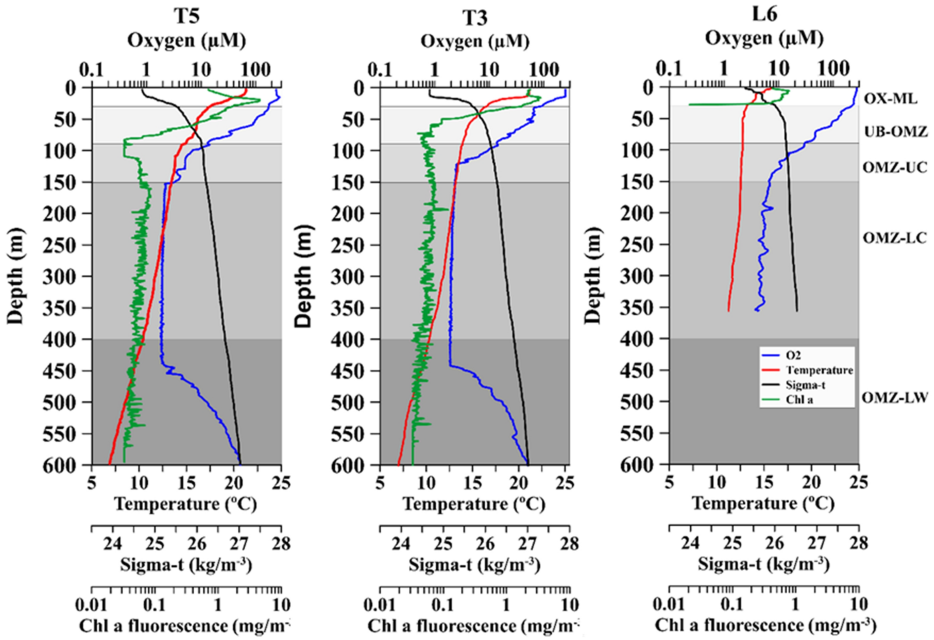

Fig. 2.

Figure 3

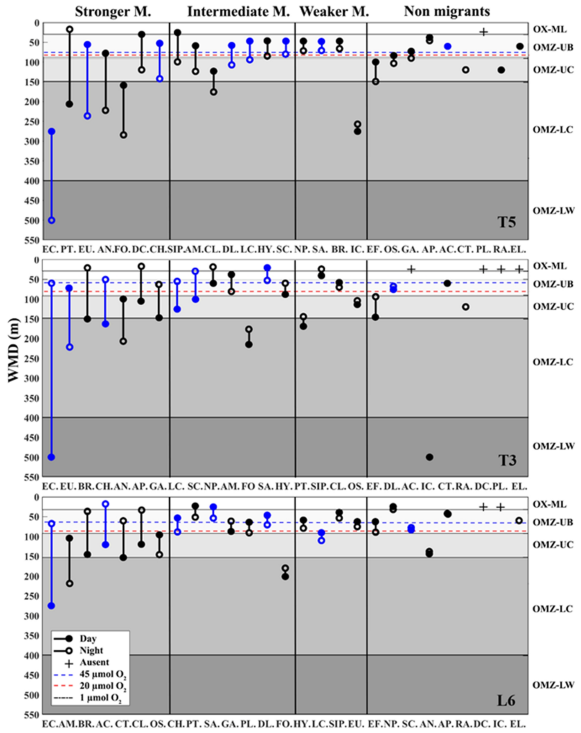

Fig. 3.

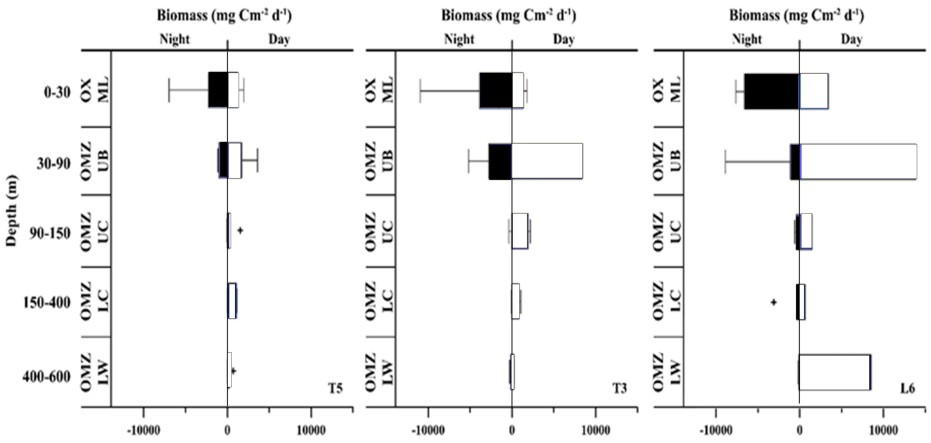

Fig. 4.