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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

Anonymous Referee #1

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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. 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

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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. 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. 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. 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). 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 abovementioned 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. 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. 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). 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. 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 be more natural, for Nauplius Larvae simply nauplii would be shorter, and Ctenoforos and Ictioplankton in English would be Ctenophora and Ichthyoplankton , respectively. 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).

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). 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.

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. Also please add the multinet depths to Fig.2 as horizontal lines. 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).

Hydrography of station T6: Initially I had assumed this nearshore station was only

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350m deep. The lack of CTD data needs to be noted somewhere (I assume gear failure), please clarify. 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).

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?

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).

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.

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?

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. 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.

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.

Please also note the supplement to this comment:
https://www.biogeosciences-discuss.net/bg-2019-127/bg-2019-127-RC1-supplement.pdf

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