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Biogeosciences Discussions

Interactive comment on "The "neutral" community structure of planktonic herbivores, tintinnid ciliates of the microzooplankton, across the SE Tropical Pacific Ocean" *by* J. R. Dolan et al.

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This work provided an important set of information about tintinnid assemblage in a hardly investigated area, such as the Pacific Ocean. They also go beyond a simple list of species or the study of diversity parameters and explore the different models of abundance-species distribution and test neutral theory of diversity. I think this article is very interesting and should be published with some corrections and better explanation of the methodology used.

The main issues about this work are methodological, and related with my own experience in the study of tintinnid assemblages. The methodology used to concentrated

water sample (using a 20 μm mesh Nitex screen - pag. 565), could seriously underestimate the concentration and relative abundance (and for sure its rank order) of tintinnid species with nail form and LOD $<$ 20 μm (At least 5 of the 9 common species reported in figure 9-genus Salpingella & Cantheriella- have this problem). In this sense, when studying tintinnid diversity metrics, size class LOD should begin with 20 μm and not with the smallest value registered by the authors (12 μm - pag. 566) or, on the other hand this problem should be addressed in some way in the methodology section; like statistical comparison of density for each tintinnid species under suspect of possible underestimation in sample water before and after concentrate it, in order to support the values registered (either absolute & relative abundance of these species). This situation could also affect the species abundance distribution pattern which is one of the main concerns in this study.

Secondly, it should be clarified in this paper if the exact depths (i.e: 5m, 10m, 40m, DCM) for Chl and tintinnid samples were the same in each station or not. (There are differences in the sampling bottles used, and the number of depths studied for Chl and tintinnids, 10 and 6 respectively). If they were not, the correlation of Chl and tintinnids (both measured as average values in the water column of each station, Figure 2 & Table 5), and the relationships among tintinnid diversity metrics and phytoplankton pigment parameters (pag. 570) might not reflect the actual situation due to dramatic changes known for local scale of Chl and mainly in tintinnids distribution (Kils, 1993), and then the third question (pag. 565, line 5) should be avoided or reformulated in order to avoid a possible wrong conclusion.

Thirdly, the author states that tintinnid communities are structured by dispersal limitation due to a lack of competition, however “dispersal” in microplanktonic organisms mainly depends on hydrological conditions (main currents, presence of oceanic fronts and upwelling, presence of transition zones, etc.) of the study area. Hydrological conditions also determine the presence of tintinnid food (phytoplankton). I think that the authors should discuss hydrological conditions at mesoscale level in order to support

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the idea of “dispersal limitation”

Specific corrections

Pag. 564 & references section (pag.576) Agatha et al. 2004 or 2005? Correct the wrong one.

Pag. 564 & references section (pag.578): Verity 1984 or 1987? Correct the wrong one.

Pag. 571 cited Dolan et al. 1999 and then Dolan 2002, but both of them are not included in the reference section.

Pag. 573 cited Preston 1962, but not included in the reference section.

Pag. 578 referenced “Murray, Barber et al” but not cited in the main text.

Pag. 578 "Van Heukelem & Thomas..." not referred in the correct place.

Table 3 (pag 581): I think it is more easy to interpret if pChl, nChl & mChl are cited as % of total Chl a.

Table 4 (pag 582) The correlation matrix is symmetric so remove all values from down left hand of the matrix, and check the Rho value between # spp vs H is not the same for H vs # spp.

Figures 6 & 7 (pag. 590 & 591) are not cited in the main text.

Interactive comment on Biogeosciences Discuss., 4, 561, 2007.