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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 is a good paper providing important new data on the structuring of pelagic ecosystems. The minor revisions suggested by the referees should not impose problems.

In preparing your revised manuscript, I only want to draw your attention to the comments of reviewers Chust & Irigoien and Thompson on the apparent neutral structuring of tinitinnid communities. Since "neutral" shows up in your title, you should take special care on this matter. In addition to testing for dispersal limitation by analyzing similarities between communities, you should also revisit the model fitting. For example you reject a geometric fit because the slope of your modelled geometric series is usually

too steep. However, you only use the most common species to derive your slope. Is this feasible? Also, does the neutral model really predict a log-series distribution or rather a zero-sum multinomial (Olzweski 2004, Olzewski and Erwin, 2004; Wagner et al., 2006)? And then your argument that the log-series fit increases with low species-richness (p. 573) does not really refute Magurran's argument that low sample sizes impose problems on separating models of relative abundance distribution (see also Wagner et al. 2006, supplementary material).

References

Olszewski, T. D. 2004. A unified mathematical framework for the measurement of richness and evenness within and among multiple communities. Oikos 104:377-387.

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Wagner, P. J., M. A. Kosnik, and S. Lidgard. 2006. Abundance distributions imply elevated complexity of post-Paleozoic marine ecosystems. Science 314:1289-1292.

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