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Interactive comment on "Similar patterns of community organization characterize distinct groups of different trophic levels in the plankton of the NW Mediterranean Sea" *by* V. Raybaud et al.

Anonymous Referee #2

The idea that inspired this manuscript is certainly appealing, because it aims at pointing out similarities in the structure of biotic assemblages (please do not use "community" if you are focusing on subsets of taxonomically related taxa) across multiple trophic leveles (and time/space scales).

The way this idea has been translated into a data analysis procedure, however, is not completely convincing. In fact, the comparison between trophic levels is based on very basic diversity measurements (species richness and Shannon index) as well as on relative-abundance vs rank curves. While the latter has a potential (although not fully developed), species richness and Shannon diversity index are very sensitive to

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taxonomical problems, sampling efficiency, sample size. etc. and therefore they are not really suited for comparing heterogenous assemblages.

As for taxonomical problems, it is obvious that sometimes identifying species or higher level taxa is not an entirely objective task. I'm pretty sure that most copepod species are actually discrete biotic entities, which react in a different way to biotic and abiotic pressures, but I'm not sure that the same applies to some Ceratium species or to some tintinnids, because in some cases their taxonomy is driven by morphological details only, which are sometimes independent of real genetic differences. So, what are the Authors really comparing? Is species diversity or maybe taxonomic skills or taxonomic opinions? BTW, this is a general problem that basically has no solution, but in this case it certainly has a sinigficant impact on data analysis.

As for problems related to sampling, it obvious that they also play a role, and the Auhtors addressed some of them, e.g. by analyzing species accumulation curves. However, even in case samples fully represented the real assemblage structure for the three selected trophic levels (copepods, Ceratium and tintinnids), the latter are inherently different from each other when it comes to the number of species, which in turn affects not only species richness (that is obvious), but also the Shannon index. Therefore, both species richness and Shannon index cannot be compared among different groups of organisms. In the case of Shannon index, comparing evenness would have been more correct, as eveness varies in the finite interval [0,1].

However, it would have been much more appropriate to compare different assemblages by taking into account properties that are invariant relative to the scale of observation and to the level of taxonomic detail. This is not a trivial task with species assemblages, but during the past 15 yeasr some work has been done with fractal models and relative abundance distributions (see, for instance, Mouillot et al., 2000. The Fractal Model: a new model to describe the species accumulation process and relative abundance distribution (RAD). Oikos 90:333-342).

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This approach is probably still questionable, because the search for evidences of a universal order in the fuzzy assemblage of all the biotic entities is strongly biased by the way thoise entities are defined, but at least it relies upon a specific hypothesis. In fact, it assumes that relationships between biotic entities can be described (and modelled, if necessary) in a very general way thanks to relative abudance vs. rank curves. From this viewpoint, comparing three groups of organisms that are not only different in trophic level, but also in their inner heterogeneity and in the way taxonomic problems affect their composition, seems acceptable, although the ecological insights that can be obtained from a single case of scale invariance in the relationships between three (subsets of) trophic levels are not obvious.

As for minor details, I spotted a number of glithces and a few major problems, but I don't think it is really useful to point them out, given the overall weakness of this manuscript in its present form. Some of them, however, suggest that Authors are not completely familiar with the topics they discussed. For instance (see page 4903), the Akaike Information Criterion (AIC) is not related to any test, as it is only aimed at comparing model performances (and, please, cite the original reference) not at testing something (e.g. the deviation from a theoretical model). And it is not a Bayesian approach, although it is quite similar (looking at the way it is computed) to the Bayesian Information Criterion (BIC). Moreover, what is the purpose of citing a Microsoft Excel function when it comes to computing values from a normal distribution? Would someone cite the SUM() function for 2+2=4? Finally, why the Shannon index is defined as a "metric"? In an ecological context, a metric can be either a distance or dissimilarity coefficient that has metric properties, or a variable that is monotonically related to a general property that is to be defined by means of a multimetric index, but I cannot see a good reason for defining as "metric" the good old Shannon diversity index.

The bottom line, in my opinion, is that this mansucript can be accepted subject to major revisions.

Interactive comment on Biogeosciences Discuss., 5, 4897, 2008.