

Interactive comment on “The reliability of grazing rate estimates from dilution experiments: Have we over-estimated rates of organic carbon consumption?” by J. R. Dolan and K. McKeon

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These authors make an excellent point - that there is no good reason to assume the growth of protistan grazers is unaffected by dilution, and that this assumption is crucial to the dilution method. Except for an earlier paper by Dolan et al, I don't think this issue has been addressed previously.

The experimental effects of dilution on ciliates that are presented here are convincing, but scant. More experiments of the kind shown in figure 2 would be helpful in resolving this issue, particularly if they involved other grazers (flagellates, dinos, tintinnids, etc.). The particular experiment they reported was dominated by small oligotrich ciliates, which have high growth rates and hence “stack the deck” in favor of a strong

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effect of reduced food on growth. The results may not be applicable to other microzoo communities.

The authors' arguments about the correlation between ciliate biomass and flagellate biomass, what constitutes a "normal" clearance rate, and whether ciliates are a good enough proxy to stand for all micrograzers, etc., were less convincing to me (cf fig 3). In fact, it's a bit frightening how much variability there is in the abundance of protozoan groups in different marine environments, despite consistency within each reported study. This made me wonder if we are indeed all intercalibrated when we sit down at our epifluorescence microscopes.

I disagree with the idea that reported high clearance rates (g) in oligotrophic waters is by itself evidence of the inadequacy of the dilution method. In fact, where food abundance is low, clearance rates should be highest, and in eutrophic waters, where food abundance is highest, the fraction of biomass removed each day by microzooplankton should be lowest, exactly what is found and what these authors take as evidence that the dilution method works particularly poorly in oligotrophic waters.

The analysis provided in this ms supports the idea that there may be something not quite right with dilution results and provides a plausible mechanism for explaining exactly what that is. As such it should be widely read and should stimulate discussion. But in my mind it also suggests alternative hypotheses, and the discussion of some of these would strengthen the paper. For example, maybe we are indeed still underestimating the abundance of micro- and nanograzers. Maybe pigmented flagellates, which are not included in the clearance rate estimates of these authors, are more important than we think (in fact, there is some evidence that they are more important in oligotrophic waters, consistent with the results presented in this ms). In my experience, kleptoplastidic ciliates have smaller threshold values for growth-supporting food concentrations. How does their relative abundance change across the trophic gradient and how might that affect dilution experiments? Are there other grazers we don't know about? Are there other processes that lead to phytoplankton mortality which are

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affected by dilution? These questions and hypotheses should be addressed by the microzoo research community. If they are, this ms will have provided a valuable service to the community. I thus support its publication.

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