

Dear Dr McKinnon

We are grateful to receive your valuable comments in helping us improve this manuscript. Please find our responses (in italic font) following the copy of your comments.

A strong paper, using a very similar method to that we used in McKinnon & Duggan 2003, but taking the results further by making a comparison of directly-measured growth rates with those predicted by the metabolic theory of ecology (MTE). This comparison is somewhat compromised in that it is only possible in circumstances where food-limitation can be discounted. Food-limitation of growth is inherent in zooplankton communities in waters such as these, and the authors have gone to some length to only use data where they can meet the assumption that growth is not food-limited.

We agreed that the influence from food-limitation should be excluded before compared to the prediction of MTE. We tried to use only growth rates which were classified as “not food-limited”. However, given that copepods could utilize foods other than phytoplankton, our procedure (only based on chlorophyll a) might not be able to identify all food-limited growth. We have included this consideration in Discussion section:

“We note one of the caveats of our analysis is that copepods can utilize various types of food in addition to phytoplankton (as listed in Table A1, e.g. microzooplankton, Turner, 2004). Non-phytoplankton foods could obscure the food effects (chlorophyll a concentration) analyzed in this study, and it may have influenced our ability to accurately test the assumption of MTE that no food limitation was met.”

The authors quote Mauchline (1998) as justification for classifying small calanoids as herbivorous, however I feel strongly that the field has moved on substantially since those times. Though I am not familiar with the E China Sea study area, I feel confident that the phytoplankton is dominated by picoplankton, that microbial processes dominate, and that the most important grazers of primary production in this system are small protists such as heterotrophic nanoflagellates, ciliates and heterotrophic dinoflagellates. There is plenty of evidence that the small copepods are primarily feeding on these protists, and it is highly likely that they are also feeding on detrital aggregates, rather than on phytoplankton per se. Since it is impossible to assay these primary food sources, we are stuck with chlorophyll as a proxy measurement of food availability.

Thank you for your comments. We classified calanoids as herbivores only in a relative and general sense; however we have included more contemporary references describing the mixed diet of small copepods in Discussion section:

“Our finding of a significant correlation between $\ln(\text{growth rate})$ of broadcast-spawning copepods and chlorophyll a concentration (by Monod function before any correction, Fig. E1) is in agreement with the generalization that broadcasters are relatively herbivorous (Mauchline, 1998), although more contemporary studies described the mixed diets for small copepods (e.g. Turner, 2004 and references therein).”

That said, we did find that only the growth rates of calanoids (as broadcaster group in our study) were significantly related to food concentration (using chlorophyll a as a proxy). On the other hand, the diet for other groups is rather difficult to assess probably due to the complexity of their feeding habits. We admit that chlorophyll is not a perfect descriptor of food, and using chlorophyll deserves more discussion for

this concern. Additional descriptions are thus included in our revised manuscript:

- i) Materials and Methods: “As a compromise for assessing food concentration, we consider chlorophyll a concentration with Monod equation”;*
- ii) Discussion: “we note one of the caveats of our analysis is that copepods can utilize various types of food in addition to phytoplankton (as listed in Table A1, e.g. microzooplankton, Turner, 2004). Non-phytoplankton foods could obscure the food effects (chlorophyll a concentration) analyzed in this study, and it may have influenced our ability to accurately test the assumption of MTE that no food limitation was met. However, since the biomass of most microbial components (sampled as POC) still has a strong correlation to chlorophyll a concentration (Legendre and Michaud, 1999), chlorophyll a concentration perhaps could still be interpreted as a proxy or index of food availability rather than just phytoplankton.”*

The assumption that the growth measurements made using the artificial cohort method match those in situ then depends on the assumption that the microbial composition of water within the cubitainers during the 48h of incubation matches those in the ocean, and that the chlorophyll concentration is a fair measurement of this. This is a rather large assumption, and in my view is probably the origin of the less than perfect correspondence with MTE predictions.

Agreed. Although, our intention was to limit the duration of incubations in order to limit food limitation in the cubitainers, the change during incubation was still a concern. Therefore, we agree that our interpretation should include this underlying assumption, and we have now incorporated it into our revised manuscript:

- i) Materials and Methods section: “The environment in the cubitainers was assumed to be similar to in situ condition along the incubation. However, we do acknowledge that the duration of our incubations (specifically the 48-hour incubation) may have led to differences in the types and quantities of food available to incubated animals relative to that in the water column. This incubation time represents a necessary trade-off between potential incubation-effects and allowing sufficient time for growth to be measurable.”*
- ii) Discussion section: “Substantially, we note one of the caveats of our analysis is that copepods can utilize various types of food in addition to phytoplankton (as listed in Table A1, e.g. microzooplankton, Turner, 2004). Non-phytoplankton foods could obscure the food effects (chlorophyll a concentration) analyzed in this study, and it may have influenced our ability to accurately test the assumption of MTE that no food limitation was met. However, since the biomass of most microbial components (sampled as POC) still has a strong correlation to chlorophyll a concentration (Legendre and Michaud, 1999), chlorophyll a concentration perhaps could still be interpreted as a proxy or index of food availability rather than just phytoplankton.”*
- iii) Discussion section: “However, we still note our assumption of no container effect along incubation.”*