We would like to thank reviewer 2 for the comprehensive feedback to the manuscript. Please find our detailed responses to all comments and suggestions below (our response is in italic font).

This paper studies community growth rates of marine copepods estimated by using the artificial cohort (AC) method in the East China Sea and further explore the relations between measured rates to body size, temperature, and food availability in a framework of the metabolic theory of ecology (MTE). Here are some concerns about the approach when comparing the estimated parameters in fitted MTE models with theoretical values reported by Gillooly et al., 2001 with underlying assumptions of no food limitation. In subtropical waters, high metabolic costs associated with relatively high temperature and low biomass of food resources can cause food limited growth. The authors also mentioned that the one of the critical issues to estimate in situ growth rates of copepods is to maintain the natural resource condition over the period of experiments. I would consider the less perfect match between model parameters estimated by using the measured data with the theoretical values could be related to the following: (1) species may have different responses to temperature and body weight in terms of the growth rates. Using data of non-species specific rates could be one possible cause.

We agree with this possibility. Notably, even on condition that we did not identify to species levels, our MTE-fitting results of sub-groups (e.g. broadcaster/sac-spawner) still show different estimates of the temperature and body size coefficients (Table 1). Resolving to species-specific rates could readily resolve the question; however, the trade-off between identification resolution and sample size (or time) still represents a practical obstacle. Additional discussion has been included in Discussion section of revised manuscript:

"we demonstrate that the deviation from MTE could possibly be attributed to both the differences in taxonomy and analytical method in use, and that species-specific examination and improved analytical methods may help to resolve these issues"

(2) The measured rates were likely under food limitation. The assumption of MTE likely wasn't met.

This comment is indeed a crucial concern. In our analysis, we demonstrated that there were probably food-limited conditions at least for some periods despite our efforts to exclude out possible food-limited growth. One of the reasons may lie in the less-than-perfect food proxy, chlorophyll concentration, and this issue has addressed in our Discussion section:

"Substantially, we note one of the caveats of our analysis 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."

Moreover, in a separate analysis we have included explicitly chlorophyll concentration into our statistical model and found that indeed food concentration plays an important role in affecting growth rates. We have included this issue in Discussion.

(3) Another approach could be considered to validate the MTE model. Splitting the measured rates, using one half to build MTE models and then do projections and compare the projected and measured rates using the second half.
To accomplish this validation, we further performed a 10-fold cross-validation on the fitting of MTE models. The reported mean squared errors (MSE) of the cross-validation are similar among groups, indicating similar performance of the fitting to MTE models. Related additions of description are listed as followed:

i) Materials and Methods: "a 10-fold cross-validation (Cudeck and Browne 1983) was performed to evaluate the GLM for each group. That is, we randomly divided the data set into 10 subsamples, and used 9 subsamples to construct the GLM model and the remaining subsample to evaluate the model performance (based on prediction error). The same procedure was repeated 10 times to exhaust the 10 combinations, and the average error was calculated. Small prediction error is considered as good performance.";

ii) Results: "the mean squared errors (MSE) of 10-fold cross-validation were also similar among groups";

iii) Discussion: "Overall, we found a significant correlation between weight-specific growth rate versus temperature and body size as described by Eq. (8), with the MSE of cross-validation similar among groups (Table 1).";

iv) Table 1: additional column "MSE of cross-validation" and table legend "MSE of cross-validation: mean squared error calculated from 10-fold cross-validation analysis."

Page 6 line 10-11, It's not clear how the CTD measurements of temperature were used to monitor the temperature over the incubation experiments
Because our shipboard incubations were all conducted in the black tanks that were continuously circulated with pumped surface seawater, we regarded that the surface seawater temperature is similar to our incubation condition. To clarify these, we expanded on our description in the Materials and Methods section in our revised manuscript:
"CTD measurement of surface seawater temperature if the former measurement was lacking (because incubations were conducted in tanks circulated with surface seawater, see Fig. C1 and "Artificial cohort method" below)."

Page 6 line 20, The two sizes of 50-80 and 100-150 μm may miss some seasonally important species in zooplankton community.
We admit that our choice of size-fraction could reflect only part of the entire copepod community size range and has now been noted in our Discussion section:
"We do note the size-fractions used in this research might still miss some representative species in different seasons, and that we could not distinguish the species-specific patterns due to our relatively low resolution of identification. However, we also note that the biomass of copepod communities in the ECS is dominated by small bodied animals (e.g. Lo et al., 2004) which were well represented in our incubations."

Page 7 line 1, 50 μm may also remove microzooplankton, food resource of copepoditted and nauplii
We agreed that there may be some possible food resources removed by 50 μm sieving. However, such experiment design was fixed methodologically (50 μm is the lower limit of 50-80 μm size-fraction in use) and still reasonable considering feeding spectra of copepods (1 to 2 order of length ratio between copepods and its prey). We

have clarified this in our Discussion section of revised manuscript:
"We also note that by using the 50 µm filtrate of seawater for incubation water and the food source, we did run the risk of removing large food particles. This aspect of our experimental design was determined according our lower limit size-fraction (i.e. 50-80 µm). In addition, it has been reported that copepods generally have a 1 to 2-order (i.e. 10-100 times) length difference with their prey food (Berggreen et al. 1988); therefore, the small copepods in our incubations (50-80 and 100-150 µm in length) were likely utilizing food particles less than the 50 µm as main food resources, and thus our design may not have serious problems of food exclusion."

Page 7 line 14, incubation experiments conducted in dark may reduce the possibility of growth of food resources such as phytoplankton and other microzooplankton.
We actually conducted incubation under dark condition on purpose. The reason is to simplify the quantification of total chlorophyll concentration available to the incubated organisms, and to avoid the confounding bottom-up effects (due to different growth condition for primary producers). Meanwhile, the incubation time is thus determined as only intermediate duration to avoid food limitation. The considerations have been incorporated in the Materials and Methods section of revised manuscript: "The tanks were kept dark to reduce the possibility of growth of primary producers (and thus the confounding bottom-up effect), and the total chlorophyll a concentration in the cubitainors could be simply estimated by initial concentration."

Page 9 line 10, taxon? Or morphtype at here.
Corrected as "morphotype"

Page 10 line 6, not clear based on what to define food limited. Only chla is not sufficient.
Indeed, recognize the need to be conservative with our use of chlorophyll concentration as a proxy of for food condition. Also, the definition of "food limited" by $4 \times K_m$ *was determined arbitrarily. Nevertheless, the comparison among models incorporating different food effects demonstrated that the results were similar regardless of inclusion of "food limited" growth values (comparing Table 2 with Table 3). We have included the consideration of non-chl-a food as listed below:*

i) Materials and Methods: "As a compromise of accessing food condition, we consider the chlorophyll a concentration with Monod equation";
ii) Discussion: "Substantially, we note one of the caveats in our analysis that copepods could utilize various types of food in addition to phytoplankton (as listed in Table A1, e.g. microzooplankton, Turner, 2004). Such condition could obscure the food effects, and we may thus unable to correctly check whether the assumption of MTE (no food limitation) was met. However, since the biomass of most microbial components (sampled as POC) still has a strong correspondence 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."

Page 12 line 4, We consider the following variables (is equation more relevant?)
Corrected as suggestion, "equation" instead.

Page 12 line 23, check figure 4, it should be oncaeid, not corycaeids
We apologize for mis-labeling the color legends. The mistake has been corrected.

Page 13 line 2, check the chi-squared values for 50-80μm, it seems too small compared to the 13.69 for 100-150 μm. If the d.f. is similar the two chi-squared values should be very close to achieve the same significance level

We have examined the calculation again and returned with the same values. Though the d.f. of target variables are identical (both are the station numbers), the two size fractions differ in their sample size (due to different identification resolution; 3 taxa in 50-80 μm and 7 taxa in 100-150 μm) and in turn the d.f. of error and the shape of chi-square distribution. Therefore, 100-150 μm with larger sample size could achieve similar significance level with higher chi-square value.

Page 13 line 13, is the theoretical value -0.25 applicable for all taxon? Don't know why select this as a standard for comparisons in the paper

The theoretical value -0.25 is a universal prediction for weight-specific growth rate according to MTE. Although there were some studies that reported deviated values in different organisms (e.g. squid, Seibel 2007; insect, Irlich et al. 2009), there was no clear prediction for any taxon of copepods. Therefore, we had to compromise using -0.25 as our theoretical value.

Same page line 23, no "f "in figure 6, is it "e"?; also plot the fitted lines (see comment in figure).

We apologize for the incorrect description in the Results section. In fact, no significant relationships were found (both linear and Monod function) between chlorophyll concentration and residuals of each group; therefore, no fitted line is plotted in the figure 6. Accordingly, we made corrections in the revised manuscript as listed below:

i) Results: "The linear or Monod equation as functional response between residuals from regression Eq. (8) and chlorophyll a concentrations was not significant when considering growth rates for the groups of all data as a whole, different size-fractions, or spawning types (Fig. 6, $r<0.1$, $p>0.05$ in all five panels)."

ii) we deleted the following sentence in the Discussion section "We also note the influence of food concentration for all non-broadcaster groups after removing the effects of temperature and body size (Fig. 6)."

Page 14 line 11, no "except for" the rates of harpacticoid nauplii >calanoid nauplii in figure 5.

We only described part of the pair-wise comparison as an example (i.e. growth rates of harpacticoid nauplii were only not significantly different to that of calanoid nauplii). However, since these descriptions offered only incomplete information and probably obscured our topics, we have substituted them with general descriptions in Results section:

"With respect to the taxonomic effect, pair-wise comparison was made and the grouping is shown in Fig. 5 (i.e. the groups with the same alphabet symbols indicate no significant difference in their growth rates)."

Page 16 line 8-9, using two mesh sizes may limit the total range of body size and could be a cause at here

The discussion has been supplemented according to suggestion:

"Indeed, in our data set (about $10^{1.5}$-fold range of body size due to our size-fraction design), the proportion of variance explained by body size was ~21.0%."

Page 16 line13, how did the authors correct the temperature effect? This should be

included in methods
The information has been offered in the Materials and Methods section as below:
"In addition to the GLM, we independently analyzed our data in the form of "temperature-corrected weight-specific growth rate" in relation to body size (M) by OLS, MA and SMA regression: $g' = a_0' + a_1' \ln(M)$,
where $g' = \ln(g) + (E/k_B)T^{-1}$, a_0' *is the intercept,* a_1' *is the allometric coefficient for body size."*

Page 19 line 21-25, the discussions about toxins effect on growth rates jump too much from the results presented at here.
Agreed, and this discussion has been eliminated in order to avoid confusion.

Page 20 line 8-13, these should be results and not discussion.
Rearrangement of paragraph has been made as suggested. Thank you.

Page 21 line 12, add (ANOVA?)
Done as suggested.

Page 22 line 1-5, these models have been tested previously. It isn't a surprise that these empirical models always tend to under or over estimation
Agreed. One example of a study that demonstrates similar results has been added to the Discussion section of the revised manuscript:
"The discrepancy among field data and the empirical model predictions was also reported by other studies (e.g. Madsen et al. 2008)."

Page 22 line 10-17, this is very short for discussion and consider to combine it with others.
Combination of this paragraph with spatio-temporal discussion has been done (consistent with the arrangement of Materials and Methods, and Results section). Thank you for the suggestion.

In Table 1-5, why don't report a1 instead of E to keep the results consistent?
For the convenience of readers comparing our results with the theoretical prediction of MTE, we decided to report the E value (which is also rather informative, i.e. activation energy for biochemical reaction) instead of the origin coefficient a1.

Figure 3. is the station # matched the station # in Figure 1?
No. Because there are some stations where we were able to conduct multiple experiments on separate occasions (different cruises), we noted in the figure legends that "stations are numbered according to Table B1". In order to avoid readers' confusion, additional description in Materials and Methods has been provided:
"Note that because we were able to conduct experiments multiple times on separate occasions at certain stations, we referred to the stations separately by the numbers listed in Table B1 unless Fig. 1 is specified."

Figure 4. Present the three plots vertically with identical x scale
The figure has been rearranged as suggestion.

Figure 5. specify the labels a,b,c,d etc
Such information has been specified in the figure legends of Fig. 5:
"The alphabetical symbols above the boxplots (i.e. a, b, c, d, and e) indicate the

groups by post hoc pairwise comparison; i.e. the groups with the same symbols represent no significant difference in their growth rates."

Figure 6. add the fitted lines using the monod function
Because there is no significant relationship (no matter linear or Monod function) between chlorophyll concentration and residuals of each group, no fitted line was plotted.

Figure 7. this plot is not well presented (some bars are too small to see). Also no data for harpactocoid nuaplii (50-80μm)
We have rearranged the Fig. 7 into 2-panel to clearly present the result. Unfortunately, there were no data available for harpacticoid nauplii in station 9, and we have included the description in figure legend of Fig. 7 for clarification: "There were no available growth rates for harpacticoid nauplii in 50-80 μm size-fraction from station 9."

Reference:
Kingsolver and Huey 2008 is missing
The missing reference has been recovered:
"Kingsolver, J. G. and Huey, R. B.: Size, temperature, and fitness: three rules, Evol. Ecol. Res., 10, 251-268, 2008."