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Biogeosciences Discussions

Interactive comment on "Quantifying the structure of the mesopelagic microbial loop from observed depth profiles of bacteria and protozoa" *by* T. Tanaka et al.

T. Tanaka et al.

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Both referees #1 and 2 suggested that we should clarify the model description and improve the interpretation of the obtained results. Since we used the published data and a simple steady-state model for analyzing the fate of bacterial production in the mesopleagic layer, we intended to keep the description of the data and the model as concise as possible in the original manuscript. According to the comments by Referee #2, we have realized that such an intention unfortunately resulted in unclear and confused communication with readers. In the revised manuscript, we have fully improved the sections of "Model" and "Results and discussion".

Model: As mentioned above, we have fully revised the model description. Referee #1 pointed out that HNF egestion was not considered in the model. The predator's yield can be described as "Ingested organic carbon - Loss of organic carbon to respiration - Egested organic carbon" divided by "Ingested organic carbon". Thus, we agree that a significant egestion by HNF contributes to the organic carbon (OC) pool or reduces

HNF growth yield. However because the egestion of organic carbon by HNF seems to be less significant in the OC-limited condition, we did not use the parameter for egestion in the model. To clarify our model assumption, we have added this discussion in “Results and discussion” section.

Referee #2 left many comments and questions. We understand that most of them were due to the model description which was not clear in the original manuscript. Because the manuscript has largely been revised by taking account of the comments, we have not put “point-by-point” reply about the model description and analysis here.

Apart from Referees’ comments, we have improved the model for estimating HNF growth yield. By applying the assumption that ciliate specific clearance rate is 10 times greater for HNF than ciliates into Eq. (2), HNF growth yield can directly be estimated by the regression of the biomass of bacteria and ciliates (Eq. (5)). But this modification does not significantly change the results, and thus the original conclusion. On the other hand, the above assumption is sensitive in our model, we have discussed on the results derived from another assumption that specific clearance rate is same for HNF and ciliates.

With regard to our conclusion in the original manuscript, Referee #2 commented “the conclusions are either vague or uninformative”. Referee #1 already pointed out this (published on 22 August 2004), and then we have improved the incorrect description in the conclusion (published on 21 September 2004): “We agree with the referee #1 that our description “heterotrophic nanoflagellates are the important remineralizers (page 414, lines 12-13; page 415, line 8)” is incorrect. We have improved as “heterotrophic nanoflagellates transfer little of bacterial production to higher trophic levels.” We have also improved “...with HNF as the important remineralizers (page 419, lines 22-23)” as “...with HNF as the potentially important remineralizers of bacterial production”. A sentence “This may challenge... (page 419, lines 23-25)” has been deleted.”

Construction and length of text compared with the number of figures and tables: Ac-

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cording to the suggestion by Referee #2, we have limited to original results and related discussion in the revised "Results and discussion" section with one figure and two tables. Although the data shown in Figs. 2 and 4 in the original manuscript have already been published, Fig. 2 is very important to introduce our model. We have made a new section "Background of the mesopelagic microheterotrophs at the study site" for describing the previous results. On the other hand, we have deleted Fig. 4.

Other comments by Referee #1 (We replied on 21 September 2004)

Page 417, line 23 "...between the three...": We have corrected as suggested.

Page 421, line 7, "Deep-Sea? Missing I or II": In 1987, there was only "Deep-Sea Research".

Other comments by Referee #2

Title: As Referee #2 suggested, we have improved the title as "Analyzing the trophic link between the mesopelagic microbial loop and zooplankton from observed depth profiles of bacteria and protozoa".

Usage of the term "the microbial loop": Our understanding is that the term "the microbial loop" (sensu Azam et al. 1983) addresses that a significant portion of organic carbon produced in the euphotic layer is used by heterotrophic microbes such as bacteria and protozoa, and eventually mineralized. As Referee #2 mentioned, after Azam et al. paper, we have learned that trophic interactions between microbes are much more complicated in the euphotic layer. However, very little is known about trophic interactions in the mesopelagic layer. Thus, we used a simple "food-chain" type model from bacteria to zooplankton except for bacterial mortality (viruses and HNF). With this reason, we use not "microbial food web" but "microbial loop" throughout the manuscript.

Default knowledge of the fate of bacterial production: Our understanding is that significant bacterial mortality by viruses was discovered later than that by HNF predation in the euphotic layer, by which a general consensus of bacterial mortality by HNF has

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been discounted from ca. 100% to a certain extent until now. We therefore need to ask Referee #2 to give us the exact reference(s) explaining as the default knowledge that the same amount of bacterial production goes to viruses and HNF in euphotic and mesopelagic layers, respectively. We cannot see how analysis of ratios in biomass of HNF to bacteria allows estimating carbon flow from bacteria to viruses and HNF.

The description of the study site: "Annual fluxes in the mesopelagic layer" mean amount of organic carbon consumed in the mesopelagic layer or decrease of vertical organic carbon flux between 110 and 1000 m on an annual basis. We have improved the description in the revised manuscript.

Unit of alpha: As is in the original manuscript, alpha denotes specific clearance rate of predator in unit of carbon per day (L nmol-C-1 d-1).

HNF as less important regulators of bacteria in the mesopelagic layer: We agree with the interpretation of Referee #2. We have also obtained the same interpretation from the data shown in Fig. 1 of the revised manuscript: "...that the density-dependent predator-prey relationship becomes less coupled between the three microbial heterotrophs with increasing depth down to 2000 m.".

Figure 3: As Referee #2 pointed out, we agree that it is not necessary to present Figure 3 in this manuscript. We have deleted this.

Interpretation of the regression analysis: We have deleted "Significant linear relationships in both equations (4) and (5) suggest that the assumed food-web structure approximates the microbial loop in the mesopelagic layer of the study site". We have revised the description as "Significant linear regressions were obtained in both Eqs. (4) and (5), while the coefficient of regression was not high for Eq. (4) (Table 1; Fig. 3)."

Bacterial growth efficiency (BGE) at the study site: Our study site is likely independent of atmospheric input and lateral advection, i.e. a semi-enclosed system. It is

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considered that most of sinking POC and exported DOC from the euphotic layer are consumed in the mesopelagic layer. Tanaka and Rassoulzadegan (2002) found that bacterial biomass dominated the heterotrophic microbial community in the 110-1000 m layer. We then assumed that most of OC remineralization can be attributed to bacteria in this layer. Under the assumption that the study site is in an approximate steady-state over multiyear in terms of OC stock, we estimated BGE in the mesopelagic layer by replacing a total amount of OC assimilated by bacteria with the OC flux between 110 and 1000 m (Tanaka and Rassoulzadegan 2004). del Giorgio and Cole (1998, 2000) compiled the published data on BGE which are mostly based on either simultaneous measurements of bacterial production and respiration in relatively short incubations, or diluted bacterial culture in relatively long-term incubations using shallow waters. del Giorgio and Cole (1998, 2000) and Tanaka and Rassoulzadegan (2004) respectively discuss on the methodological uncertainty in estimating BGE. Discussion on differences in growth efficiency between bacteria and HNF is interesting, however, this is beyond the scope of our paper.

Estimate of the error in HNF growth yield: As suggested, we have included estimate of the standard error in HNF growth yield: 1.12(+-0.15)%. We have surveyed published data on growth yield of HNF and ciliates in the revised manuscript.

Table 2: As suggested, we have calculated carbon flow with errors.

Data by Cho et al. (2000): We have compared our estimate of specific clearance rate of HNF with the data by Cho et al. (2000).

Page 418, line 22 (HNF feeding on bacteria produces POC): We have deleted this sentence.

We thank both Referee #1 and 2 for their helpful suggestions.

Interactive comment on Biogeosciences Discussions, 1, 413, 2004.