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Biogeosciences Discussions

Interactive comment on "Dynamics of nutrients, total organic carbon, prokaryotes and viruses in onboard incubations of cold-water corals" *by* C. Maier et al.

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We thank the reviewer for the thorough comments, which improved the manuscript

Anonymous Referee #1

This study investigated the effects of cold-water corals incubated on board on inorganic nutrients, total organic carbon, prokaryotes and viruses. Two deep water coral specimens belonging to Lophelia pertusa and Madrepora oculata collected at depths ranging from 560 to 780 m by several independent box corer deployments in the Rockall margin (NE Atlantic ocean) were utilised for the onboard experiments. Five different time-course experiments were carried out up to 72 hours using triplicate microcosms

for both coral specimens: using natural seawater collected at two different depths and locations, and three "manipulated" seawater typologies (virus- and cell-free-seawater, seawater enriched with viruses or prokaryotes). On the basis of the results the authors provided evidence of a potential major role of cold-water corals through the release of mucus and nutrients on microbial food web dynamics. In general, I found the article well presented, the experiments sufficiently detailed also using an iconographic approach, the results interesting and sufficiently discussed. However I have some points which deserve considerations before the acceptance of the manuscript.

Bleaching procedure to eliminate biofilms from dead corals should be better explained and before claiming the lack of biofilm on the skeleton some SEM images should be provided.

ANSWER: We used a standard methods to remove organic material, i.e. treatment with bleach. We describe in the text in more details what we did: i.e., soaking skeletons over night in common household bleach, then rinsing skeletons under a strong water ray, rinsing in MilliQ to make sure all bleach and organic matter was washed off. Skeletons were then dried in an oven for at least 24 hours. The originally brown skeletons were white after this treatment. We do not have SEM pictures and we do not think that this is necessary, since this is a standard procedure for removing organic matter. Inspection in a bionocular microscope did also not reveal organic material. We have changed "no biofilm" to "no or almost no biofilm" in the M&M section. In addition, there were no differences of parameters between the treatments: 1) seawater only, 2) dead colonies (skeleton) with biofilm and 3) bleached dead colonies suggesting that seawater alone is also ok.

Viral loss due to the use of preservative as in the case of the present study has been extensively documented, but I have not find any mention on that issue.

ANSWER: We did not mention that because we have used an approach that prevents losses of viruses during preservation (brief pretreatment of fixed samples at 4°C, shock

freezing in liquid nitrogen and final storage at -80°C). In the revised version, we mention briefly that this approach avoids losses of viruses.

The authors have rightly recognised that a source of variability in their experiments has been introduced by the use of different micro-colonies and deep waters for incubations, but handling stress as well as the effect of changes in the hydrostatic pressure after sample recovery have been not taken into account. Some comments on this should be also included.

ANSWER: We have added more details on the possible sources of variability in the first paragraph of the discussion section.

The patterns of viral changes in viral-enriched systems shown in Figure 4 need to be better explained. I'm not convinced that differences claimed after 6 hours between the controls and systems containing corals are really significant.

ANSWER: there seems to be a misunderstanding: we do not claim that in all experiments, the differences are significant between controls and coral treatments at T6h. Sometimes, there are no differences, sometimes there are differences ate T2h. The description of changes and statistics in the text is essentially correct. However, we have for the UF experiments confused the two species. Slight modifications were made to increase readability.

Any hypothesis why in all virus-enriched microcosms viruses significantly decrease (by a factor ca. 2) with increasing incubation time.

ANSWER: In the virus-enriched treatments, prokaryotic abundance was low (at least initially). There were likely not enough viruses for a strong viral propagation thus, resulting in a net decay of viral abundance. We have made a note on that in the figure legend.

Although I understand that the authors have probably presented tables instead of figures to save space, I would like to see not only results at the beginning and the end of

the experiments (as shown in Table 2 and 3), but also the overall temporal patterns in the different experiments they carried out. These results can be eventually presented as supporting material and will allow to better appreciate changes in the different variables analysed occurring with time.

ANSWER: We have shown the entire temporal pattern of a single experiment to show the general patterns. There are differences between experiments, partly due to the different treatments, and these differences have also been (partly) explained in the main text. The general patterns are very similar across experiments, there is not much to be learned that is not already in table 2 and 3). This is why we think, it is not necessary to show these data.

Why the authors have not included the temporal patterns of TOC from the variables presented in Figure 2.

ANSWER: We had some problems with the TOC analysis (also indicated in the table legend of Table 3) potentially due to contamination of some glass vials used for storing water samples for TOC analysis. While the general trend was the same in all experiments and short term release rates in the NSW were very similar, some time points were odd (e.g. a reason to use data from T6h rather than from T2h for estimating mucus release). As we have no possibility to confirm whether these data are outliers or not, we prefer not to show them, in order to avoid presenting uncertain data. However, as to the request of reviewer #2, these potential outliers were included in table 4. It is noteworthy that the inclusion of these outliers did not change the trends in table 3. Rates from table 3 were not affected in the first place.

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