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

**C. Maier et al.**

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

Anonymous Referee #2

The paper by Maier et al. provides data from a series of ship-board experiments on the effect two species of cold-water corals on the concentrations of nutrients, total organic matter, microbes and viruses in the surrounding water. Two experiments are carried out with natural seawater, three experiments with altered concentrations of microbes or viruses. The laudable attempts by the authors to cover a wide scope of parameters, manipulations and controls, are thwarted, however, by the low, and partly insufficient

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amount of replication in the experiments (N=3, corals; N=2, controls).

ANSWER: The reviewer seems to be mistaken. We have used 3 types of controls in duplicates each (see figure 1). In every replicate with corals we used 3 colonies thus, we used 9 colonies per experiment and species. Also, for natural seawater (untreated seawater), the experiment was carried out twice, which adds up to  $2 \times 3 = 6$  replicates and  $2 \times 6 = 12$  controls. Moreover, the three additional experiments, where seawater was manipulated, showed the same general patterns as the experiments with natural seawater (i.e. additional  $3 \times 3 = 9$  replicates and  $3 \times 6$  controls = 18). Thus, 45 micro-colonies per species were used. And these patterns were similar for the two species. Thus, we do not agree that our data set is characterized by low replication; most experimental studies with CWC corals were done with much less replication.

A more fundamental concern not critically addressed by the authors, however, is the finding that virtually all of the parameters investigated show a neutral (nitrate) or negative (all other variables) mass balance for the corals. Unbalanced losses of both, inorganic and organic materials are difficult to reconcile with a healthy and growing organism, unless we invoke some combination of parameters (e.g. uptake of dissolved inorganic carbon+ dissolved organic nitrogen balancing the DIN+TOC losses) not covered by the study or supported by the literature, or a metabolism based on stored materials (e.g. large zooplankton eaten prior to the experiments). As such, the reported direction of fluxes and magnitude remain at best fragmentary, or artefactual at worst

ANSWER: This study was designed to study some interactions between corals and micro-organisms, not to assess a mass balance. Release of nutrients and mucus has been documented before for other coral species and it is obvious that such losses have to be balanced by uptake of matter. Among those mechanisms are uptake of zooplankton and corals were indeed fed before the experiments during the acclimation phase (this info has been added to the revised version); DIC uptake also occurs in the *L. pertusa* holobiont (unpublished data). *L. pertusa* can stay healthy and calcify

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for some time, even it is not fed. The direction of fluxes is clear from our data: corals release mucus and (some) nutrients. But this is of course only true for a part of the system, i.e. we have not studied uptake and we did not intend that and we did not claim that. Thus, we think we have provided strong evidence that the direction of the flux is likely correct for the compartment investigated. It is true that the magnitude of the fluxes can be biased due to incubation and experimental manipulations. However, this is also true for all other papers on experimental manipulations (including all mucus release measurements from corals). In addition, high TOC concentrations and stimulation of microbial activity has also been found in in situ studies in CWC reefs thus supporting the general direction of (some) fluxes.

These two major issues need to be addressed by the authors for the paper to be acceptable for publication.

ANSWER: As outlined above, we think that the first issue is a non-issue. The second issue has certainly some validity, although we reject the allegation that we have attempted to make a mass balance. The evaluation of rates and net fluxes is an inherent problem of all experimental approaches. We have also -particularly for nutrients- discussed the implications in a cautious and not in a matter-of-factly way. To further stress this cautious interpretation, we have added a note of caution at the end of the introductory paragraph of the discussion.

It is not clear from the information provided in the ms if the corals were subjected to heavy siltation in the box cores, if they suffered aerial exposure when transferring to aquaria or glueing to their holding plates, how they were held in the process, etc.

ANSWER: Micro-colonies were taken mainly from large colonies in boxcores with little sediment content. They were handled with forceps and if necessary briefly exposed to air for glueing on slides using EPOXY and for photographing. Corals were fed with freshly hatched Artemia (but not during the experiment). This information has been added to the revised version of the manuscript.

Given the limited time for the corals to recover between sampling and experiments (2 days), much of the leaching of materials could thus also be attributed to insufficient healing of the lesions inflicted from breaking the colonies, handling the branches, etc.

ANSWER: Coral including *L. pertusa* can show rapid repair (e.g. Maier 2008). Repair, i.e. sealing lesions should occur rapidly (and easily in such simple 2-cell-layer animals, which are well known for their repair and regeneration capacity). If not, they would be impacted and killed by prokaryotes. So, the notion that are 2 days recovery time (or more, since most corals were kept for 3-7 days before use in experiments) are limited, does not seem to be supportable. Up to date, there is no comprehensive study on how long it takes for cold-water corals to recover sampling stress. There are only statements that corals need to recover etc, but without real evidence. Our group has now extensive experience working with living cold-water corals with both, short-term on board and long-term aquarium experimentation and there is no evidence, that corals perform worse during on board experiments (shortly after sampling) than in the laboratory. Further, results of TOC release are comparable to studies carried out by Wild et al. (using an aquarium colony) and we can therefore assume, that the rate reported in this study is reasonable. Also, we only used healthy looking corals without epibionts; no colony died in the experiments; all colonies had extended polyps in the experiments.

Although the paper is well written for the most part, some restructuring is needed: parts of the Materials & Methods need to go to the Discussion, parts of the Results into the M&M, etc., as highlighted in the ms, attached. There are a number of minor comments/suggestions in the ms.

ANSWER: A considerable restructuring has been performed (see attached pdf with answers to comments made directly into the pdf file).

Please also note the supplement to this comment:

<http://www.biogeosciences-discuss.net/8/C2476/2011/bgd-8-C2476-2011->

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