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Interactive comment on "Effects of global climate change and organic pollution on nutrient cycling in marine sediments" *by* C. Sanz-Lázaro et al.

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Anonymous Referee #2. The reply of the authors is written after the word "REPLY:", immediately after the comment of the referee.

This is a study that investigates the combined effects of a predicted seawater temperature combined with an organic nutrient input on the biogeochemistry on shallow-water sediment systems. I like these type of studies for a number of reasons: 1) the authors investigates multiple stressors (since single stressor is very uncommon in the real world), 2) intact, natural sediment communities are used, 3) the experiment is designed to take time into consideration and 4) more than 2 temperature treatments were used. However, I do have major issues with the fact that the experiment was conducted in darkness, especially considering: "The aim of this work is to examine the effects of

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temperature rise and organic enrichment on sediment nutrient release". See specific comments in this matter below. Specific comments: #1 The last paragraph in the introduction. You are only looking at heterotrophic processes, i.e. not the entire sediment community and how it is affected by organic input and warming. Please specify the aim better and also try to incorporate this in the rest of your introduction.

REPLY: Generally, this type of experiments are run in darkness, when the focus of the study is the heterotrophic processes. Although we may not be able to predict nutrient release rates in shallow photic sediments based on this experiment, the obtained effluxes can be extrapolated to sediments receiving little or no light. Such sediments are very common in coastal waters in the Baltic Sea area due to eutrophication. We have added to the end of the last paragraph of the introduction: "In these areas, heterotrophic processes in sediments prevail due to the generally low availability of light in the seabed due to eutrophication and high input of labile organic matter (Conley et al. 2009)." We have also added to the sentence of the aim in the last paragraph of the introduction: ". . . derived from heterotrophic processes."

Reference used in this comment: Conley, D. J., Bjorck S., Bonsdorff E., Carstensen J., Destouni G., Gustafsson B. G., Hietanen S., Kortekaas M., Kuosa H., Meier H. E. M., Muller-Karulis B., Nordberg K., Norkko A., Nurnberg G., Pitkanen H., Rabalais N. N., Rosenberg R., Savchuk O. P., Slomp C. P., Voss M., Wulff F., and Zillen L.: Hypoxia-Related Processes in the Baltic Sea, Environ. Sci. Technol., 43, 3412-3420, 2009.

#2 Although you refer to the paper by Valdemarsen et al., 2009 for specific details regarding the methods, I still would like to know at what depth the sediment was collected without looking at this paper. This is important considering your data and the scope of this paper. Since this sediment was collected in July at 1 m depth (at least it was in Valdermarsen et al. 2009), I wonder why the autotrophic community, i.e. benthic microalgae is not even mentioned? How much benthic microalgae was present at the sediment surface?

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REPLY: Since we performed the experiment under absence of light (which is stated in the M&M in subsection 2.2) we found that it was no relevant to measure data related the autotrophic community. We used homogenized sediments and hence microalgae present on the sediment surface during sampling were mixed into the whole sediment cores. We have added in the sentence where we explain the sediment collection: "...at 1 m depth...".

#3 With the approach of only incubating the sediment in darkness you only target heterotrophic processes. However, during light, these shallow sediments often functions as sinks for inorganic nutrient via uptake by benthic microalgae. This is especially true during summer, with far less dark hours than light hours, meaning that the net flow of nutrients (at least nitrogen) during 24 hours might be the complete opposite to your results. Any thoughts on this?

REPLY: We have added to the discussion before the conclusions the following paragraph: "Additionally it should be considered that the experiment was performed in darkness, as the experiment was designed to simulate the generally low availability of light at the sediment surface in relatively enclosed and shallow coastal areas exposed to eutrophication, such as in the Danish coastal areas. Nevertheless, in other areas with more light availability autotrophic processes are also important. This could lead to different results with regards to nutrient release rates from the sediment.".

#4 The surface oxygen (in the top layer of sediment???) was maintained due to bioturbation, even though the sediment was in complete darkness during the entire experimental period. Was any oxygen profile in the sediment performed? If the oxygenized layer was reduced (which I suspect it would since no photosynthesis could be performed by the benthic microalgae) this would have contributed to your results with time even though the top surface was oxygenized?

REPLY: The top layer of the sediment was oxidized as we could see it in the supplementary material in Sanz-Lázaro et al. (2011b) since the sedi-

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ment when oxidized it showed a yellowish color indicating that Fe was oxidized. http://esapubs.org/archive/appl/A021/118/appendix-A.htm This was expected to be because the polychaete Nereis diversicolor bioirrigated the sediment. The water that Nereis introduced in the sediment was oxygenated since there was permanent aeration of the water column to prevent water hypoxia. Reference used in this comment:

Sanz-Lazaro, C., Valdemarsen T., Marin A., and Holmer M.: Effect of temperature on biogeochemistry of marine organic-enriched systems: implications in a global warming scenario, Ecol. Appl., 21, 2664-2677, 2011b.

#5 I would suggest starting the discussion with a brief reminder of the aims and also highlighting the most important results for your study. As it is right now, the discussion come across as a bit boring and to technical and immediately starts discussing the phosphorus fluxes.

REPLY: We have added a paragraph at the beginning of the discussion: “Our results show that temperature rise resulted in different trends of sediment nutrient release of PO43- and NH4+ under both organic and non-organic enrichment conditions. While sediment PO43- release followed a linear trend with increasing temperature, the NH4+ release from sediment show exponential trends, notably increasing when temperature increments were above 6°C.”.

#6 I would like to see a discussion if you believe your results would look the same if you allowed the temperature to vary, i.e. taking daily and weekly temperature variations into consideration. Because during summer it can be several degrees difference between day and night, between days and weeks which might affect your results.

REPLY: Because water has a high specific heat index daily variations in relatively large basins are expected to be low. We think that this effect would be minimum and so we rather not add anything related with this comment to the discussion.

#7 Page 36, line 24, sure, could be true, on the other hand with your experimental

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design you cant really say since NH4 (especially during summer with many light hours in contrast to dark hours) is taken up by the sediment during the day.

REPLY: We have answered this with the first comments, since our experiment is based in conditions of light absence.

#8 Please include in your discussion how your fluxes probably would behave if your they were measured both during light and dark and how this probably changes your conclusions.

REPLY: Undoubtedly, this could have modified the fluxes from the sediment. We think this idea is considered with the paragraph added derived from the specific comment #3. Otherwise we found it would be too much hypothesising to much in something we haven't tested.

#9 Multiple stressor model. What model is used for investigating the multiple stressor effect? Where you find significant interactions are these results synergistic?

REPLY: As stated in the M&M:". . . we did regression models considering temperature the continuous covariate and OM enrichment as a fixed factor." This is to say ANCOVA (analysis of covariance). The interaction between the covariate and the fixed factor tell us if there are synergisms or antagonisms. We found this to happen for PO43- efflux rate but not for NH4+ efflux rate (see table 3). This is explained in the results: "There were significant differences between -OM and +OM treatments at 26°C ($p<0.05$) and the significant interaction term of the regression ($p<0.05$) indicated a steeper temperature response in +OM compared to –OM treatments"; "The interaction term of the regression did not show significant differences. Thus, NH4+ efflux in –OM and +OM cores followed a similar trend with temperature although at different ranges (Table 3).".

We thank the referee for the time taking to comment the Ms and thus participating in improving it.

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