Response to Referee #2

We greatly appreciate the effort the referee spent on this review and thank for valuable suggestions and positive comments. The reviewer's specific comments are addressed below (*our responses in italics*).

When first looking at the figures in the paper, it was immediately clear that there were large differences between replicate treatments. They often seem to behave rather different in almost all parameters studied. The authors treat this variation well in the interpretation of the data, but the phenomena is not at all discussed, which seems a bit odd as it is so prominent. Is this common in mesocosm studies? The authors could try to discuss this in a couple of lines.

The idea behind the experimental set-up was to establish a CO2 gradient. In agreement with the other participants of the experiment, who also contribute to this special issue, we categorized the mesocosms into three treatments of low, medium, and high CO2 in our figures and refer to these categories in some passages in the text. To consider the applied CO2 range in our statistical analysis we applied a regression model. We will clarify this and add a short paragraph on variability and replication in mesocosm experiments.

Primary production and bacterial numbers were also determined in this study. At least they are described in the methods section, but the actual data are not directly presented as in the figures. The bacteria numbers are for instance only used to calculate cell specific rates, but seem highly relevant for the paper. The comprehensiveness of the paper would be improved if they would be included, especially because both data are discussed in the text (eg. page 10478 line 22 and page 10481 line 23 for bacterial abundances (which are not seen in Fig 6, by the way?), and page 10491 line 15 for primary production).
It is also rather confusing that it is mentioned that the primary production increased at higher CO2 concentrations (without showing the data), but that the chlorophyll-a concentrations peak at intermediate CO2 concentrations at the end of the experiment.

Since all manuscripts on this experiment are compiled in the BG special issue data on primary production and bacterial cell numbers are easily available to the readers and we want to avoid redundancy in the presentation of different parameters.
Primary production: The data on primary production are in detail presented by Engel et al. (including the relationship of chlorophyll a and primary production). However, we think a short method description that also includes the reference to Engel et al. in our M+M section is helpful to the readers.
Bacterial cell numbers: We agree with the referee that this point needs to be clarified in our revised manuscript. The development of microbial abundances (picophytoplankton, nanophytoplankton, total bacteria and high-DNA bacteria, viruses) are comprehensively described and discussed by Brussaard et al. In addition, we determined total bacterial cell numbers in our subsamples for extracellular enzymes and BPP. Deviations from cell numbers given by Brussaard et al. are minor but cell numbers derived from these measurements were used to optimize our calculations of cell-specific rates. Hence, we added the method description to our manuscript but referred to Brussaard et al. for a detailed analysis of microbial growth dynamics.

I understand that these data are probably derived from other manuscripts in this special issue on the EPOCA experiment, but there are no references to these papers. So, either show these data in the figures or remove the method description and refer to the other papers where the data are presented.

The manuscripts of Engel et al., Schulz et al. and Brussaard et al. are referenced in M+M, results and discussion (Engel et al.: sections 2.5, 3.5, 3.6, 4.3; Schulz et al.: sections 2.1, 3.6, 4.2, 4.3; Brussaard et al.: sections 3.3, 3.6, 4.5) but we will add some brief sentences

about the content of these papers to our result section and check whether more references are needed in the text.

The last two sentences of the abstract are very complex, vague and therefore difficult to comprehend especially the last sentence. It is basically not clear what is stated here and the authors should rewrite these sentences completely making them much simpler and straight-forward.

We will change the last three sentences of the abstract:
"(...) Also primary production and bacterial protein production were positively correlated, strongly suggesting that higher amounts of phytoplankton-derived organic matter were re-assimilated by heterotrophic bacteria at elevated primary production. Since elevated primary production was achieved under elevated pCO2 in this mesocosm experiment, it can be suggested that efficient heterotrophic carbon utilization had the potential to counteract excess autotrophic CO_2 fixation. However, our results also show that beneficial pCO_2-related effects on bacterial degradation activity can be mitigated by the top-down control of bacterial abundances in natural microbial communities." (p. 10468)

The introduction is a bit long and may need some restructuring. The reader now has to wait until the third paragraph to learn about the effects of ocean acidification on bacterioplankton. As this is the primary focus of the paper, I would put the third paragraph first and reduce the general introduction to bacterioplankton (current paragraphs 1 and 2) to one paragraph.

We agree with the referee. The second paragraph of the introduction will be shortened. Furthermore, we will shift sentences about the seasonal development of bacterial activity in the Kongsfjorden (p. 10469, l. 2-11) from the first to the last paragraph. Hence, effects of ocean acidification on bacterial activity will be addressed after some initial sentences about the regulation/constraints of bacterial activity in cold marine environments.

References to figures are typically only given at the end of a paragraph where data are discussed. It would help if they would also be given in the beginning of a paragraph.
We will check the results section and the discussion and add more references to figures.

Other comments:
Page 10470 line 3: 'are amplified in the Artic'
Will be changed.

Page 10470 line 25: 'largely determined by' due to two times affected
The first sentence including "affected" will be omitted. The reasons for this are explained in our next answer.

Page 10471 line 12: this doesn't seem in agreement with the description in the introduction that states that it is mainly affected by Atlantic water. Please clarify.

On p. 10471, l. 12 we state that the Kongsfjorden "is influenced by both Atlantic and Arctic water masses (Hop et al., 2002)."
This is correct but in fact in contradiction to a statement in our introduction (p. 10470, l. 25):
"The Kongsfjorden system in West Spitsbergen is mainly affected by the northbound transport of Atlantic water masses by the West Spitsbergen Current. It is an open fjord system without sill and, therefore, largely affected by mixing processes on the adjacent shelf (Hop et al., 2006)."
The first of these two sentences is imprecise. We will omit this sentence in our revised manuscript and modify/supplement the second one:

"The Kongsfjorden system in West Spitsbergen is an open fjord system without sill and, therefore, largely affected by mixing processes on the adjacent shelf. On the shelf northbound transported Atlantic Water is mixed with Arctic Water and freshwater derived from glacier melt and precipitation. The mixing of these water sources varies seasonally and inter-annually, resulting in a warmer and more saline regime under strong influence of Atlantic Water and in colder and fresher conditions during a state of Arctic dominance (Hop et al., 2006).

Page 10472 line 12: 'to determine enzyme kinetics'
Will be changed.

Page 10473 line 18: First describe how bacterial numbers were determined and then how cell specific rates were calculated. In addition, what is 'as precisely as possible' doing here (line 23), seems normal to measure something as good as possible.

The order will be changed. Furthermore, we will change the wording: "as precisely as possible" does not refer to the measurements but to the calculations of cell-specific rates. To optimize these calculations we measured bacterial cell numbers in subsamples that were incubated to determine BPP and rates of extracellular enzymes (see also our explanations above).

Page 10474 line 9: delete 'proton sensitive', is redundant.
Will be deleted.

Page 10478 Line 24: I don't understand what 'or compensated' means here. Compensated for what?

The sentence on p. 10478, l. 24 is: "After the second chlorophyll a maximum on day 21 bacterial abundances in the mesocosms diverged and the differences between the three high-CO2 mesocosms and the controls were either reduced or compensated (Fig. 6)"
For example, the difference in the beta-glucosidase hydrolysis potential between the controls and the mesocosm at 860 µatm pCO2 was reduced, while the difference between the controls and the mesocosm at 1085 µatm was zero at the end of the experiment. Our wording in the sentence cited above seems to be misleading. We will change "compensated" into "balanced".

Page 10481 Line 5 and 8: BPP
Thanks for careful reading. Will be corrected.

Page 10482 Line 21: 'To directly test the influence' Page 10484
Will be changed.

Page 10484 Line 23: dimension? Probably 'magnitude' or similar or even better just delete.
"Dimension" will be changed into "magnitude".

Page 10489 Line 17: It would be informative of the other effects were also described. Would put things into perspective.

According to the metaanalysis of Liu et al. acidification effects on bacterial extracellular enzymes are of similar sensitivity like cyanobacterial growth rates, CO2 fixation and nitrogen

fixation. Since these parameters were not studied in our experiment we prefer not to add more information on this meta-analysis to keep the discussion straightforward and focused.