RC2 specific comments:

This manuscript investigated the microbial responses to OAE approaches based on mesocosm experiments. The data presented are valuable in promoting the understanding of OAE impacts. My major suggestion for the authors is that, the discussion part could be expanded and polished a bit more, so that readers can get some take-home messages more easily. Right now, all figures are in the Results section, and they are all time-evolution of measured values. It is hard to extract key points from these figures. More in-depth analysis of these data would be helpful.

We would like to thank the reviewer for taking the time to offer these suggestions. We have addressed the comment on the discussion section by expanding it and by dividing the original section "4.1 Responses to the carbonate chemistry conditions and mineral type", and its content, into two separate sections: "4.1 Response to the carbonate chemistry conditions: Non-equilibrated TA gradient" and "4.2 Response to the two simulated mineral additions: Calcium- vs silicate-based." We hope this restructuring provides clearer take-home messages for the readers.

Regarding the statistical analysis, it is challenging to statistically detect the signal of the response, partially due to the experimental setup (comparing 5 treatments against 5) and partly because the observed effects are quite mild. For instance, there is no noticeable effect of the TA gradient (and the associated changes to pCO_2 and pH) on the absolute values of any of the parameters included in this study. In fact, we conducted ANCOVAs, amongst other tests, to determine if there was a statistically significant effect of the mineral type, of the TA gradient, or of the combination of both, during separate phases within phase II, using averages. However, the data did not meet the assumptions of normal distribution or homogeneity of variances, even after transformation. Thus, even if significance was found in some cases, these analyses were not included.

Given these challenges, and the main take-home message (the found delay in the measured production rates and the chlorophyll a concentration after the nutrient addition, and in response to the TA gradient), we plotted the slopes of daily linear models over time, alongside presenting time-evolution figures of measured values. This approach illustrates that the low TA treatments responded sooner than the high TA ones. We have also addressed the comment on statistics by providing the plots of all the daily linear models employed to produce the mentioned figures as supplementary materials.

Another concern is that the manuscript seems quite colloquial. For example, Line 378-381, "The experiment was started under post-bloom conditions (Figure 2). Nutrient concentrations were low when the treatments were applied. Thus, an initial response in the microbial community production and respiration rates to the TA manipulation was likely concealed by the nutrient limitation. Actually, after a mixing event was simulated on day 26 and 28, a response could be discerned."

Also, Line 386, "In the past, prior to the emergence of ocean acidification as a focal point in scientific inquiry…", Line 459-460, "This difference between the controls was unexpected. This was probably a random response caused by the mesocosm effect…" etc. These do not sound like scientific languages, and I would recommend that the authors revise their expressions throughout the manuscript.

We regret that our manuscript seemed colloquially written. We have addressed these comments by re-writing the listed sentences as follows:

Line 378-381: The experiment commenced under low inorganic nutrient concentrations (Figure 2), characteristic of post-bloom conditions. Thus, an initial response in the microbial community production and respiration rates to the TA manipulation was likely concealed by the nutrient limitation.

Following a simulated mixing event on days 26 and 28, a discernible response emerged. The results from the current study show a delay in bloom formation with increasing TA, when such manipulation is non-equilibrated.

Line 386: In the past, before ocean acidification emerged as a central focus of scientific research…

Line 459-490: The observed discrepancy between the controls was unanticipated and may have resulted from a random response attributable to the mesocosm effect.

This type of correction has also been applied, to the best of our abilities, throughout the whole manuscript.

Some minor comments:

Figure 1, I don't see the Fjord data but it is in the legend. I think the authors was hoping to add the legend to Figure 2, as Figure 2 has the data, but the legend does not have 'Fjord'.

We want to thank the reviewer for noticing. The legends have been exchanged as suggested.

Line 358-360, "The latter are both being…" needs to be corrected.

This comment has been addressed by re-writing the start of the first paragraph of the discussion as follows:

"The experiment described herein involved the application of two distinct alkalinity gradients utilizing two ocean alkalinity enhancement (OAE) strategies based on different minerals: hydrated lime and forsterite. Forsterite is the Mg endmember of olivine, which occurs commonly in nature, while hydrated lime is produced through the calcination of limestone (Renforth and Henderson, 2017). *Both minerals have been considered for OAE implementation; however, experimental studies on their potential impacts on natural communities remain unaddressed."*