RC1: Anonymous Referee #1

We thank the reviewers for their constructive, detailed, and very helpful analyses of the draft manuscript. Responses to their comments are given below.

Comments:

Abstract:

1. Lines 6-8: As I mentioned in the previous version, I think it's essential not to confound the role of NETs versus decarbonisation. NETs will be only a limited part of what we should do to reduce atm CO2. Please rephrase this sentence to avoid any potential misunderstanding. I appreciate that this concept is now well rephrased in the Introduction, though.
   The sentence has been reworded as follows:
   "In response, new tools are being developed to remove carbon from the atmosphere using negative emission technologies (NETs), in addition to reducing man-made emissions."

2. Lines 13-14: rephrase as "short-term (10 minutes) and longer-term (8 days)" to put already the time into context. Otherwise, there is a lack of information for the reader.
   The sentence has been rephrased as suggested by the reviewer.

3. Line 22: expected x 2. Change term.
   The second use of 'expected' has been changed to 'anticipated.'

Introduction:

4. Line 37: air-sea gradient: citation needed
   The citation for Oschlies et al., 2023 has been added following this sentence

5. Line 68: "without reducing their growth rate to zero (Berge et al., 2010; Hansen, 2002)." Can you make it more explicit that the growth rate is reduced without going to zero? The flow of the text would work better with the subsequent sentence.
   The sentence has been reworded as follows:
   "… that are able to maintain growth rates that, though lower than their maximum are still positive, at pH values above nine and below seven (Berge et al., 2010; Hansen, 2002)."

Methods:

6. Line 136: Diacronema lutheri (formerly Pavlova lutheri): check the text. In Figure 4, for example, you use P. lutheri instead of D. lutheri.
   The figure has been corrected, and the text has been checked to correct all instances of *P. lutheri* to *D. lutheri*.

7. Table 1 is very helpful. Standard errors should be included in the pH values. On top of that, when the pHs were measured? Just after the manipulation? For example, in the Effect of Chronic Alkalization on Growth Rate? Can you provide this information?
   Rather than include too much information in Table 1, thus causing confusion, we have elected to add sentences to lines 195 and 197 describing when the pH measurements were collected for both the transient and chronic experiments.

Line 195: “The first was used immediately for measurement of pH and the serial dilution.”
Line 197: “The pH was measured immediately following manipulation with the NaOH.”

Results:

8. Line 246: do you mean Figure 1d?
   The reference has been corrected to Figure 1d.

9. Figure 2: I found this figure quite unclear. It either needs to be modified, or the caption should be made more explicit. Even after reading the corresponding paragraph (Lines 263-269 and the preceding 254-261), I still find it difficult to follow. Am I missing something in the text? I have re-read it several times, but I still don’t understand, for example, what the three points refer to. Or: “The black lines represent the mean values” refer to exactly?
   The caption for Figure 2 was changed to better describe the data.
   “*Figure 2:* Comparison of the pH dependencies of growth in the dinoflagellates *Ceratium furca*, *C. fusus*, and *C. tripos* under two different growth conditions. Cultures were maintained at a constant pH in semi-continuous culture (Constant pH) and were kept in batch cultures in which the pH was allowed to drift (pH Drift). For each comparison, the symbols are the estimates from fits to Eq. 2 of the threshold pH, above which the growth rate declines (pHTh); the pH at which growth rate is reduced by 10%; and the pH at which growth rate is reduced by 90%. The horizontal black lines are the mean values across species under each growth condition. Growth rates in *C. tripos* did not vary over the range of pH tested when pH was kept constant, although they declined over the same range in the pH-drift experiment. Consequently, there are 2 estimates for the Constant pH condition and 3 for the pH Drift condition.”

10. Caption for Figure 4 or more in general in the text: do you have any explanation for the scatter trends in the growth of T. pseudonana in Figure 4d?
    The most likely explanation for the variance in *T. pseudonana* is limited observations in the period of active growth reflected in the fits. The maximum growth rate was estimated from 3 data points.

Discussion:

11. Line 329: Use always one terminology. In most of the paper, you use “unequilibrated”, so I recommend using this term.
    This has been corrected throughout the text.

12. Line 332: The synthesis of studies from the literature suggests that significant long-term increases in pH to 8.2–8.3: what does “significant” mean?
    The term ‘significant’ has been changed to ‘prolonged’.

13. Line 333: what do you mean by: “However, the conclusion needs to be qualified in the context of OAE, as the experiments were conducted in pH-drift cultures in which CO2 replacement is prevented”. Do you mean that the algae were experiencing some kind of CO2 limitation? I doubt this is the case with an increase in pH of 0.1. Could you please comment on that or rephrase or consider deleting this part since there’s already a nice explanation after this sentence?
    We have elected to delete this sentence to improve reader comprehension.

14. Lines 350-353: can you rephrase? When you talk about "nearby", it took until the end of the sentence that you are referring to the works by Hansen et al.
    The citations have been moved to immediately follow the statement "They were the primary focus for these researcher's studies (Hansen, 2002;…)".

15. Line 368: "Discharge in the nearshore in this scenario, likely into strong lateral flow", the verb is missing.
    The sentence has been rephrased to:
    "In this scenario, discharge in the nearshore will likely be into strong lateral flow."

16. From line 373 onwards, the discussion requires revision. I have a few suggestions and comments:
    a. Lines 373 -383: at times, it was unclear what the authors were referring to. For example, in lines 373-375: "There were reductions in growth rate at high pH in both", I don't see any of these reductions, especially in the transient approach. Am I wrong? Where are the reductions in growth in Figures 4 and 5, as stated in the text (looking at the standard deviations reported in Figure 5, too)?
       We have removed the sentence on lines 373 – 375, "There were reductions in growth rate at high pH in both."
       The following paragraph (referred to in comment 16b) discusses how there are clear reductions in growth rate in the chronic exposure experiments. We have added in reference to the corresponding figure (4b), which shows the decline in growth rate with increasing pH for both species tested. Figure 5 refers to the viability of the species, which was only tested for the transient exposure experiments.

    b. The authors also mentioned Fv/Fm without specifying the species they are referring to, though I assume they refer to the strong reduction in Fv/Fm in T. pseudonana following the 10-minute exposure to elevated alkalinity at pH 9.09. From this point, the authors discuss the results from Gately et al., 2023. When comparing the data from this study with Gately et al., 2023, are you only considering T. pseudonana? Since Diacronema lutheri has a very stable Fv/Fm regardless of the pH.
       The statement "in either species" was added following "…photosynthetic efficiency" on Line 384 to make clear to the reader that there was no reduction in Fv/Fm for either species during the chronic exposure experiment.
       Line 388 was also amended to reflect this: "not statistically different form the controls, as they were in this study for both species (Figure 4b)."

    c. But then, in the subsequent paragraph, from line 385, the authors discuss the different responses in terms of photosynthetic efficiency of T. pseudonana and Diacronema lutheri. I am confused. This part needs to be rewritten and corrected, looking at the data carefully.
       The previous paragraph (which is referred to in comment 16b) is discussing only the results from the chronic exposure experiments, as stated at the beginning of the paragraph. The following paragraph (this comment, 16c) is discussing the results of the transient exposure experiments (lines 391 – 392), and then comparing this with the lack of effect in the chronic experiments (lines 392 – 394).

       We believe that with the changes outlined in response to comment 16b and more explicit reference to the figures, this paragraph will no longer be confusing as written.

17. Lines from 392: the discussion starting at line 392 is also a bit out of context since the topic here is not the efficiency of OAE.
    We are unsure what the reviewing is referencing here, as the sentences following line 392 discuss how crossing the saturation thresholds for calcite and/or aragonite would be detrimental for the carbon concentrating mechanisms in phytoplankton. The next two sentences state that though this is not an ideal scenario from an OAE perspective, it could possibly occur through accidental discharge and thus the data is useful for understanding a 'worse-case scenario'.

18. Lines 398-407 are interesting since the authors discuss previous studies on OAE and the phytoplankton community responses. However, the authors lack the chance to compare the different approaches. Paul et al. 2004 describe an equilibrated OAE approach, while Federer et al., 2023 and Guo et al., 2024 similar to this study, use a non-equilibrated approach. This is an opportunity for the authors to provide a more comprehensive overview of the literature and place their results into this context. However, the differences in the carbonate chemistry perturbations of these different mesocosm and microcosm experiments must be explicitly described.
    While the comparison of the equilibrated vs unequilibrated is important, we do not feel that providing a comprehensive overview of this topic is within the scope of this paper. Instead, we have added the following to lines 406 and 408 to more explicitly describe the perturbation method used by each of the referenced authors.
    Line 406: "Both Guo et al. (2024) and Ferderer et al. (2022), who similarly to this study used an unequilibrated OAE approach…"
    Line 408: "However, Paul et al. (2024), who used an equilibrated OAE approach, …"

19. Last but not least, I suggest the authors read and incorporate (if that's the case) the papers by
    a. Nina Bednaršek et al., under discussion, https://egusphere.copernicus.org/preprints/2024/egusphere-2024-947/
    b. Faucher et al., under discussion, https://egusphere.copernicus.org/preprints/2024/egusphere-2024-2201/

    We thank the reviewer for suggesting these papers and have considered them carefully for addition to this manuscript.