Overall
The aim of the study is worthwhile, as it focuses on a relatively understudied coccolithophore (compared to model species such as *Emiliania/Gephyrocapsa huxleyi* and *Gephyrocapsa oceanica*). The authors investigated whether elevated pCO_2 impacts *Helicosphaera carteri*, assessing coccolith morphology and particulate inorganic and organic carbon (PIC and PIC, respectively). The authors claim that the results of this study suggest that *H. carteri* may have a constant contribution to the rain ratio under ocean acidification.

However, there are major weaknesses in how the data is presented and interpreted (or not used in the discussion) that lead me to recommend that this version of the manuscript be rejected.

First, the authors only include the impact of pCO_2 levels in the interpretation of the data, excluding the rest of the carbonate chemistry data presented in Table 1. Since coccolithophores are particularly dependent on carbonate chemistry, this oversight significantly detracts from the rest of the manuscript. See comments in Discussion for more details.
Second, the authors do not accurately represent the results of statistical analyses on a number of occasions in the Results. There are also occasions where a sentence contradicts a previous statement. This needs to be corrected. See comments in Results section for specifics.
Lastly, the authors use figures/tables that present the same data repeatedly. This does not add evidence to support their interpretation of the data. It would be better for the authors to choose which figure/table best presents the data and eliminate the other.

Abstract
Line 23-25: The authors state "In this study…whether high pCO_2/low pH does affect the morphology of *H. carteri* coccoliths…". But again, a central weakness of the manuscript is that the results and discussion only focus on the pCO_2, ignoring the rest of the carbonate chemistry.

Introduction
There are some word choice and grammatical issues, but overall, the introduction does a good job providing the rationale for the experiments.

Materials and Methods
Carbonate chemistry:
- DIC levels decreased under elevated CO_2. To better replicate OA conditions, wouldn't it be better for DIC levels to remain similar (or even increase) under elevated CO_2 compared to the control CO_2 condition?
- Table 1: This should be in the results. The atmospheric CO_2 levels influence the carbonate chemistry, which can impact coccolith morphology. In addition, the

standard deviation for pH is given. What is the error for the variables (DIC, TA, etc…)? This results here are underutilized throughout the rest of the manuscript.

Minor:

- Be sure to include the manufacturer info for each instrument.

Why not directly measure PIC and POC?

Results

- Lines 199-205: The authors state that there was a "slight change in the proportion of malformed coccoliths ~295 and 600 µatm of CO_2" (Line 199). This is not supported by the data presented. The average ± standard deviation percentage of malformed coccoliths are not different between the 295 and 600 µatm CO_2 treatments [other statistics (e.g., unpaired t-tests) are not provided]. The authors can still highlight the high variability of malformed coccoliths in the 600 µatm CO_2 treatment.
- Figure 2: The data presented here are misleading with the standard deviation not depicted, which would show overlap between the two CO_2 treatments. Additionally, these data are already presented in Table 2. It is unnecessary to show the same data in a Table and a Figure. The authors should decide which presentation of the data is best for the manuscript.
- Line 205-207: The authors state that "All coccosphere were intact" but then use the next two sentences to mention the small number of collapsed coccospheres detected. This is contradictory. These lines should be edited to resolve this contradiction.
- Lines 220-225: The authors state there are changes in cellular POC, cellular PIC, and PIC/POC, but then state that the changes are not statistically significant. This is contradictory. Just state that there was no significant difference between the two CO_2 treatments. Also, remove the methods for the unpaired t-tests. It is already in the Materials and Methods.
- Line 227-8: What are the units? Include the units for the values.
- Line 229-232: Again, the authors use "a non-significant change" instead of stating that 'no change/difference was detected' between protoplast and coccolith size.

I recommend adjusting how data are presented in the text by changing specific references to the following format: 'average±SD (t-test p-value<X; Table X)'.

Discussion

Major comments:

The authors only include the impact of atmospheric CO_2 levels in the interpretation of the data, excluding the rest of the carbonate chemistry data presented in Table 1. Coccolithophores require HCO_3^- as a substrate for calcification. The authors show a drop in pH and [HCO_3^-] when CO_2 increased from 295 to 600 µatm (Table 1), but do not include these variables when interpreting the data. This leads to an incomplete interpretation of the data. What about the lower pH? Is it possible that the variability in malformed

coccoliths at 600µatm CO_2 is due to the combination of lower pH (unfavorable for calcite precipitation), lower HCO_3^- concentration (substrate for calcification), and Omega > 1 (calcite formation still slightly favored)?

Some line-by-line comments:
Line 254-255: This statement is not true. See comments on Results for details.

Line 266-274: I don't understand the point of Lines 266-270.

Line 272: "…the negative effect of carbonate chemistry". This phrase does not make sense.

Line 275-276: "the effect of proton inhibition" The authors do not present any data on protons (e.g., pH) from the previous work cited.

Line 278-279: "less sensitive to acidification" The authors did not include any carbonate chemistry data (aside from atmospheric CO_2) when referencing coccolith malformation documented throughout the literature. It is inappropriate to make a claim about sensitivity to acidification without showing the relevant acidification data.

Figure 4: What about the pH in the other experiments? And other carbonate chemistry parameters (i.e., HCO_3^-, CO_3^{2-}, DIC, etc…)? It is difficult to interpret comparisons when only CO_2 µatm is included since the carbonate chemistry of the growth medium can varying based on the buffering capacity in seawater.
Figure 4b-c: Is this just showing the data from panel a) again?

Figure 5: What were the other relevant conditions in the other study, aside from pCO_2?

The rest of the Discussion focuses on comparing the findings in the manuscript to previous work. This section will need to be revised accordingly after the issues identified throughout the manuscript are resolved.