Dear editor,

Below I outline the edits I have already or will be making for the manuscript, "Calcium carbonate dissolution rate changes under future climate scenarios," for *Biogeosciences*, based on the second reviewer's comments. While Reviewer #1 is focused on how the manuscript could be edited to be improved, Reviewer #2 is focused on describing my shortcomings. It's unclear whether Reviewer #2 looked through the entire manuscript. As far as I can tell, the constructive comments made by Reviewer #2 are: 1) there should still be a discussion section but without the suspended sediment analysis, 2) there should be a description of the CMIP6 and GeoMIP scenarios and more detail given about the potential alkalinity method for estimating calcium carbonate dissolution rates, and 3) there should be edits applied to various aspects of the figures. Merging the main edits I made from Reviewer #1's comments with these, the edits I will be applying to the manuscript if you allow me to submit a revised version will include:

1. A comparison of the calcium carbonate dissolution rate estimates from the present-day period of the CMIP6/GeoMIP output and those from the GLODAPv2 and other observationally-derived data. These estimates compare quite well, suggesting that CMIP6/GeoMIP output has sufficient fidelity to be used to make projections about future calcium carbonate dissolution rates.

2. A comparison of the potential alkalinity-mean age phase space diagrams from two time periods. There is a shift in potential alkalinity towards lower values in the future climate period relative to the present-day period but the slope of the relationship between potential alkalinity and mean age does not significantly change from the present-day period to the future climate period. This suggests the Alk*-

mean age slope-based method I use to estimate future calcium carbonate dissolution rates should be as valid to use as in Sulpis et al. (2021)'s study.

3. A more extensive uncertainty analysis. I increased the uncertainty in the mean ages in the calculations for two of the GeoMIP scenarios that include Carbon-14 by propagating the error in the mean age associated with not having ages less than 200 years (only having Carbon-14-based ages). I increased the uncertainty in the mean ages in the calculations for the remaining GeoMIP scenarios without any mean age perturbation information at all. I increased the uncertainty in alkalinity in the calculations for the CMIP6 scenario because I estimated alkalinity for this scenario. Then I re-estimated the calcium carbonate dissolution rates for each of these scenarios.

4. Carbon dioxide potentially released from dissolution of calcium carbonate. I created a table tabulating the mass of carbon dioxide that could eventually be released according to each scenario's calcium carbonate dissolution rates.

5. Textual and figure edits. The methodology will needed some elaboration considering the above edits and the details that both reviewers want to see. The discussion section will transform from a section about suspended sediments' direct and indirect impacts on sea level into a section discussing the potential impact of calcium carbonate dissolution on the carbon budget based on the new table that I've added. The conclusions will not include as much discussion because that text will be moved to the discussion. The figures will include more readable text and will be pared down in number for the main text (with many being moved to the Supplementary Information).

I understand that the likelihood of having this manuscript rejected is high after a review like this one, regardless of what I write here. Nevertheless, I'm outlining the reasons there are to give me the opportunity to demonstrate that my revised manuscript will be in a (closer to) publishable form. It would be much appreciated if this is allowed. I address each of the reviewer's points below.

1 Reviewer #2 (Comments to Author (shown to authors):

- REVIEW Calcium carbonate dissolution rate changes under future climate scenarios Trossman (2026) The manuscript by Trosman investigates the changes in CaCO3 dissolution rates under different future climate scenarios, including scenarios involving geoengineering interventions. The study builds upon the methodological framework developed by Sulpis et al. (2021). The research topic is relevant, particularly in light of recent studies that have revised our understanding of the distribution of CaCO3 dissolution throughout the water column and have already highlighted detectable impacts of climate change, especially at regional scales. However, the manuscript suffers from severe shortcomings in its framing, organization, synthesis, clarity, coherence, and overall quality. As a result, it is extremely difficult to follow and leaves a very poor overall impression. Both the scientific approach and the way the work is communicated raise serious concerns. Reviewers are not expected to reconstruct the authors' reasoning or conduct an investigation to decipher the content of a manuscript. In my opinion, the manuscript falls well below the standard that should be expected before submission to an international scientific journal. I will therefore limit my review to a few major comments, as the manuscript requires substantial and fundamental revisions before a detailed assessment of the science can reasonably be undertaken.

- **I am doing something novel but not too controversial with this manuscript. I am using an established method for a different time period (the future), which I need to establish is valid and did per Reviewer #1's helpful comments. I needed to modify the methodology with the uncertainty analysis in minor ways and present more quantitative results, both outlined in my response to the previous reviewer's comments. The remaining primary issues here are: 1) is the manuscript written and organized well enough, and 2) is there sufficient supporting evidence to draw convincing conclusions of interest to the community? After addressing the previous reviewer's comments and the few you provided here, the answers to these questions should be closer to 'yes'.**

- Major comments 1. Manuscript content The scientific messages conveyed by the manuscript are not sufficiently supported by the analyses presented and therefore appear insufficiently robust. For example, the conclusion that salinity is the dominant driver of changes in CaCO3 dissolution rates is not convincing due to the methodology employed. Similarly, the geoengineering component remains largely descriptive and lacks the level of synthesis and critical analysis required to provide conclusions. Finally, the section on suspended sediments appears largely disconnected from the rest of the manuscript, making it difficult to understand its role in the overall scientific narrative.

- **In response to Reviewer #1's comments, I have already discarded the suspended sediment discussion because, while initially intended to be an example implication of the results of the study, it is too loosely connected. I cannot provide estimates of calcium carbonate dissolution rates well onto the shelves but previous studies have suggested that about 10% of global calcium carbonate dissolution occurs there. Because the sediments in near-coastal regions are almost exclusively not comprised of calcium carbonate (as they are terrigenous and supplied via rivers), there is little connection with the rest of the study. I do, however, intend to retain a discussion section that is separate from the conclusion section but without the suspended sediment analysis. I already have performed calculations for the mass of carbon dioxide that could potentially be released due to calcium carbonate dissolution (acknowledging that equilibrium chemistry would favor bicarbonate) with uncertainties. I convert the calcium carbonate dissolution rates into a mass of carbon dioxide released as follows, now described in the manuscript: "We sum over all ocean basins and then multiply the vertically integrated calcium carbonate dissolution rates by 44% to get carbon dioxide (as opposed to calcium carbonate) flux in** $\mu mol \cdot m \cdot kg^{-1} \cdot yr^{-1}$**, by 2650 to convert** $\mu mol \cdot m \cdot kg^{-1} \cdot yr^{-1}$ **to** $\mu mol \cdot m^{-2} \cdot yr^{-1}$**, by** 44.008×10^{-6} **to get carbon dioxide flux in** $g \cdot m^{-2} \cdot yr^{-1}$**, and by surface area of the ocean without the Arctic Ocean** ($347.8 \times 10^{12}\ m^2$) **to get** $g \cdot yr^{-1}$ **of carbon dioxide." There will be** a new table included in the manuscript summarizing the results of these calculations, included below for a preview. The caption will say, "Listed is the Pg (10^{15} grams) of carbon dioxide released from the dissolution of calcium carbonate per year ('CO_2'), calculated from the calcium carbonate dissolution rates estimated in this study for each future climate scenario (and Sulpis et al. (2021) for reference) over the global ocean excluding the Arctic Ocean. Also listed is the 95% confidence interval ('Uncertainty') associated with each scenario's estimate. Following the scenario abbreviation conventions of Oschlies et al. (2025), AU is artificial upwelling, M&H is macroalgae farming and harvesting, M&ScR is macroalgae farming with sinking and complete remineralization, M&SpR is macroalgae farming with sinking and partial remineralization, OAE is ocean alkalinity enhancement, and OIF is Southern Ocean iron fertilization. Also listed are the estimates using the GLODAPv2 and other observationally-derived data from Sulpis et al. (2021) and the SSP5-8.5 scenario of CMIP6 models." I will focus on discussing this table for the discussion section instead of the suspended sediment.

Scenario	Sulpis et al. (2021)	SSP5-8.5	AU	M& H	M&ScR	M&SpR	OAE	OIF
CO_2 [Pg yr^{-1}]	25.9	30.3	26.8	25.6	24.9	25.2	35.9	23.3
Uncertainty	(-16.7, 68.5)	(-19.5, 80.1)	(-17.3, 70.8)	(-16.5, 67.6)	(-16.1, 66.0)	(-16.3, 66.7)	(-23.1, 95.0)	(-14.9, 61.7)

- As for the drivers of calcium carbonate dissolution, all I am doing is varying one factor at a time using the methodology that Sulpis et al. (2021) used. I was simply curious about this at first but was surprised to find how sensitive the method is to salinity variations, which is one

reason why I believe the manuscript is worth publishing. I can certainly rephrase these results to only imply that the method itself is most sensitive to salinity variations (as opposed to saying that calcium carbonate dissolution rates are most sensitive to salinity variations, which requires the method to have a high level of fidelity). Here, I include the phase diagrams for mean age and the three variables that explicitly appear in Eq (1) of the manuscript (alkalinity, nitrate, and salinity) at two representative pressure levels (300 dbar and the seafloor). The range of values in each variable's perturbation is based on the range found in the model output. When the perturbation values from the model output are used, the perturbations in calcium carbonate dissolution rates associated with salinity are larger than the perturbations in dissolution rates associated with the other variables.

- **As for the GeoMIP experiments, I am treating those scenarios like the CMIP6 scenario because they have largely been described and understood elsewhere in the literature. I'm trying to understand the range of possible future calcium carbonate dissolution rates and the consequences of those according to which climate scenario we follow. I assumed at first that the GeoMIP scenarios would be best-case scenarios and the CMIP6 scenario would be a worst-case scenario, but that is not the case, which I believe is a publishable result. Only in the respect that the GeoMIP scenarios buffer temperature changes to various extents are they best-case scenarios, but some GeoMIP scenarios, like the ocean alkalinity enhance-**

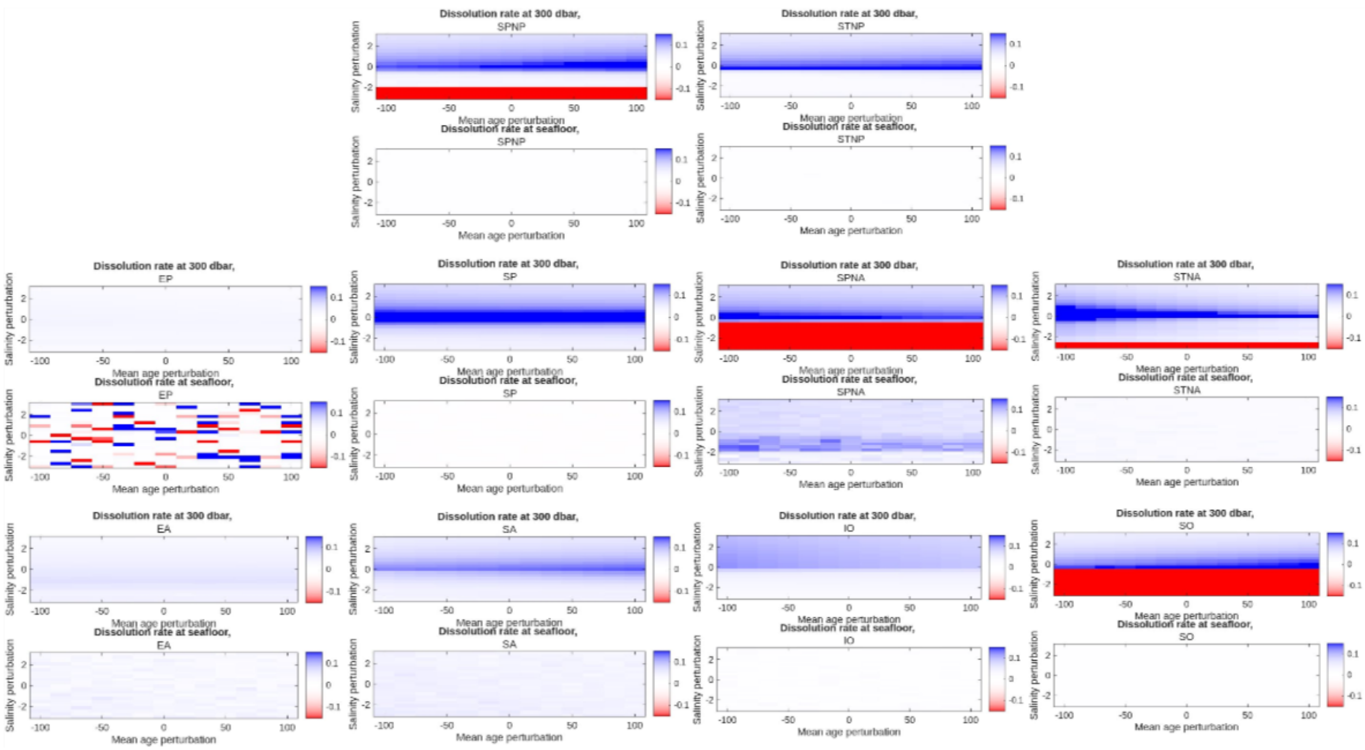

Figure 1: The mean age-salinity phase space of calcium carbonate dissolution rates for a range of -100 to 100 years and -3 to 3 PSU perturbations to the GLODAPv2-based estimates of Sulpis et al. (2021) for each of the ten biomes (Fay and McKinley, 2014). Shown are two example depth levels for each biome: one nearest to the sea surface (top) and one at the seafloor (bottom).

ment one, could actually increase the calcium carbonate dissolution rates above those associated with the CMIP6 scenario.

- 2. Manuscript structure The overall structure of the manuscript requires a thorough redesign. Different sections overlap substantially, making the manuscript difficult to follow and unnecessarily confusing. - - - The Methods section is particularly problematic. It is far too limited to provide readers with a clear understanding of the methodology, while methodological information is scattered throughout the

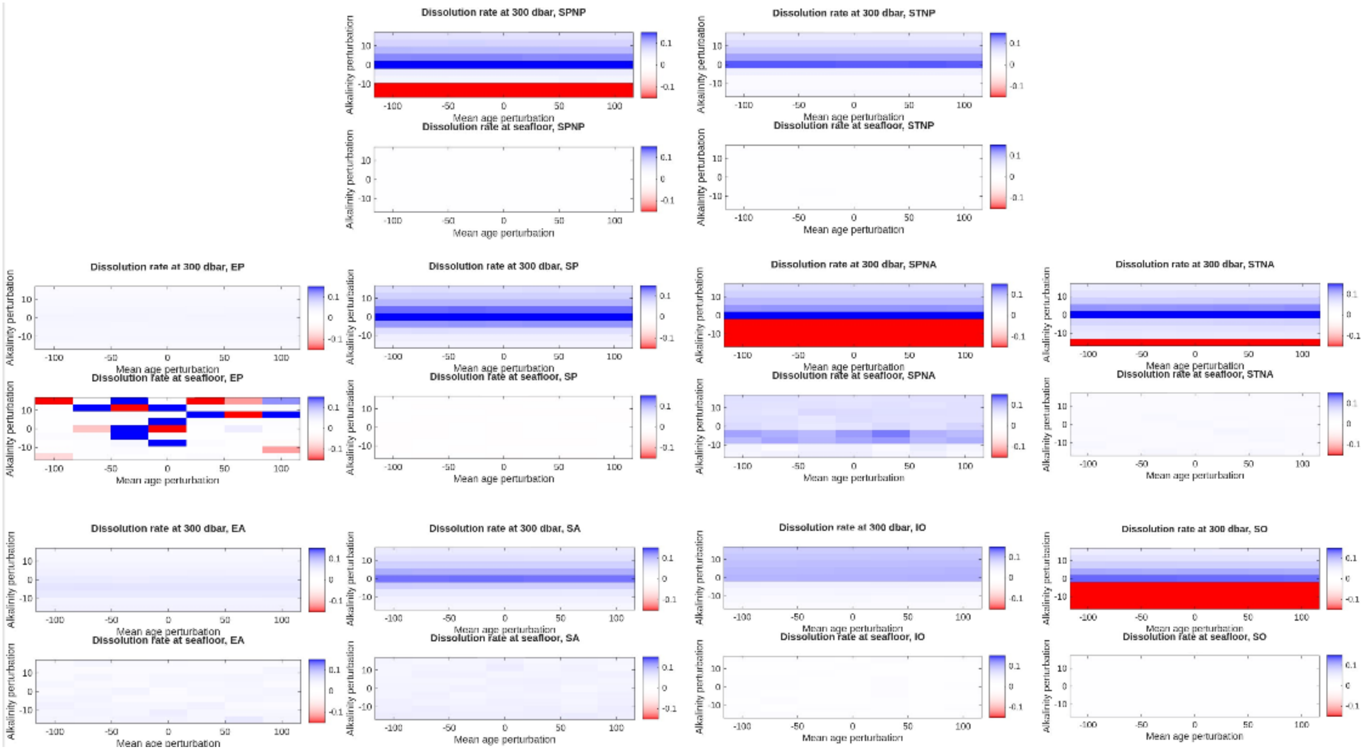

Figure 2: The mean age-alkalinity phase space of calcium carbonate dissolution rates for a range of -100 to 100 years and -15 to 15 $\mu molkg^{-1}$ perturbations to the GLODAPv2-based estimates of Sulpis et al. (2021) for each of the ten biomes (Fay and McKinley, 2014). Shown are two example depth levels for each biome: one nearest to the sea surface (top) and one at the seafloor (bottom).

manuscript instead of being presented in a dedicated section (e.g., lines 88-93, Fig. 2, and lines 210-221). The Results section mixes methodological details, evaluations, results, and discussions without any clear organization. In addition, the distinction between the CMIP6 and GeoMIP analyses disappears from approximately line 170 onward, where both are discussed together despite remaining within the GeoMIP section. The manuscript lacks a genuine Discussion section, while the Conclusions section is excessively long and contains extensive discussion that should instead appear in a dedicated Discussion.

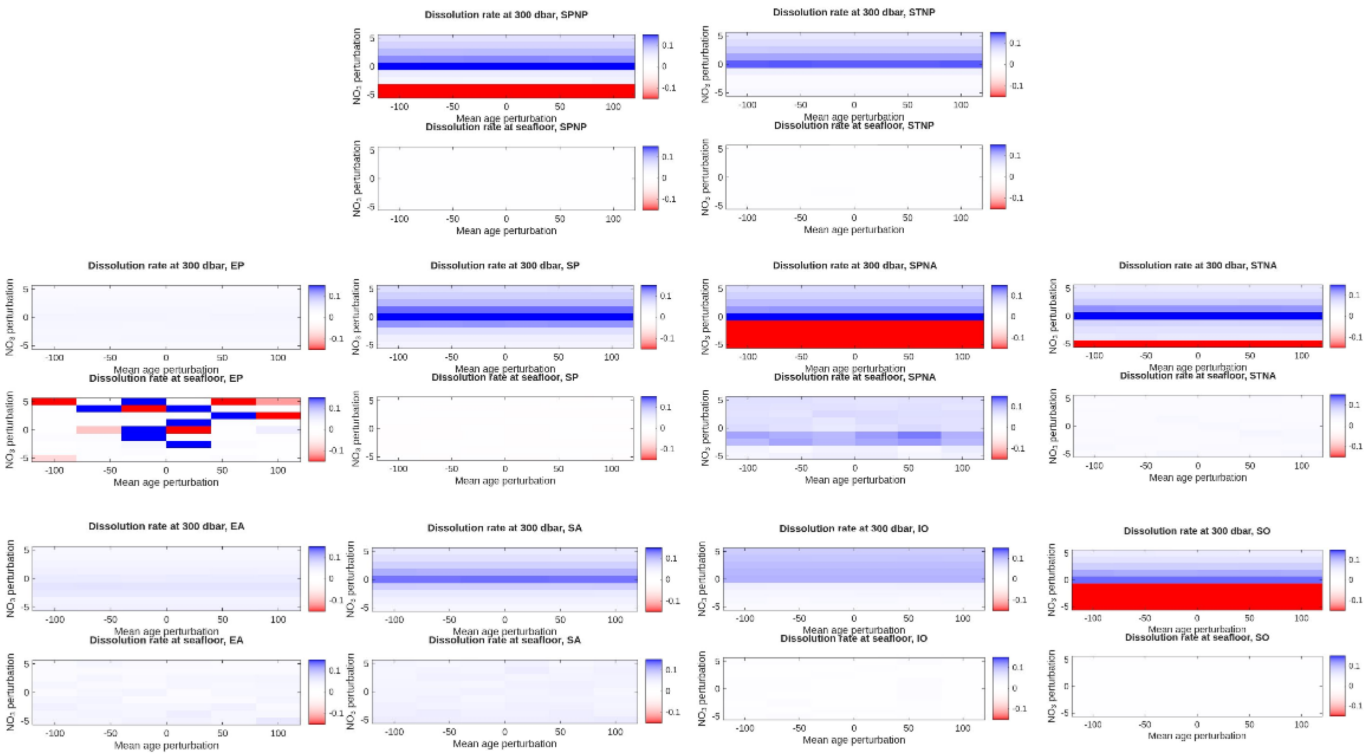

Figure 3: The mean age-nitrate phase space of calcium carbonate dissolution rates for a range of -100 to 100 years and -5 to 5 $\mu mol kg^{-1}$ perturbations to the GLODAPv2-based estimates of Sulpis et al. (2021) for each of the ten biomes (Fay and McKinley, 2014). Shown are two example depth levels for each biome: one nearest to the sea surface (top) and one at the seafloor (bottom).

- **I have already considerably edited the Methods section because of Reviewer #1's comments. Most notably, I needed to demonstrate that the CMIP6 model output can be used to estimate calcium carbonate dissolution rates comparable to those from GLODAP and other observationally-derived data (0.93 correlation - see response to Reviewer #1's comments), and I needed to alter the uncertainty analyses in order to account for a lack of model output (to attain a complete and comparable set of vari-**

ables used in the Sulpis et al. (2021) study). The future perturbation in one variable that I was missing in the CMIP6 scenario-based analysis (the alkalinity) is actually very important to account for so I derived an algorithm to estimate this perturbation based on the other available model output, which of course also has some associated uncertainty. The uncertainties in the mean ages were also important to inflate to more properly account for the lack of any mean age perturbation information in four of the six GeoMIP scenarios. The two remaining GeoMIP scenarios also needed some inflation of the mean age uncertainties, but by a different amount.

- **The reason why the distinction between the CMIP6 and GeoMIP scenarios disappears after a certain point is that I eventually focus on the range of possible future outcomes. Otherwise, I do intend to keep those scenario distinct. With the edits I needed to make in response to Reviewer #1's comments, I have newly quantified the percent of the depths and locations over which particular scenarios achieve the lower and upper bounds of the calcium carbonate dissolution rates and included that in the new version of the manuscript (see figure below). This has been added to the text: "For each ocean basin, the higher end of each envelope shown can be for any of the future climate scenarios (listed here are percent of depths and biomes for each): GeoMIP artificial upwelling (9.4%), GeoMIP macroalgae farming and harvesting (9.5%), GeoMIP macroalgae farming with sinking and complete remineralization (9.1%),**

GeoMIP macroalgae farming with sinking and partial remineralization (11.4%), GeoMIP ocean alkalinity enhancement (13.3%), GeoMIP Southern Ocean iron fertilization (7.5%), or CMIP6 SSP5-8.5 (39.8%)." Similarly, I have added to the text: "The lower end of each envelope shown can be one of several scenarios: GeoMIP artificial upwelling (15.0%), GeoMIP macroalgae farming with sinking and complete remineralization (15.7%), GeoMIP macroalgae farming with sinking and partial remineralization (19.9%), GeoMIP ocean alkalinity enhancement (20.5%), GeoMIP Southern Ocean iron fertilization (15.4%), or CMIP6 SSP5-8.5 (13.5%)." These results suggest that there is no single scenario that tends to be the lower or upper bound on possible future calcium carbonate dissolution rates. It varies by depth and location. For example, in the subpolar North Atlantic Ocean, the ocean alkalinity enhancement scenario achieves the upper bound for the calcium carbonate dissolution rates over all scenarios but not elsewhere.

- **I have added additional figures in the Supplementary Information for all of the above as well as to show that the perturbations in each model variable are different for each future scenario to a greater or lesser extent and provide explanations why. This should address many of your concerns about incompleteness and lack of clarity.**

- 3. Methods The methodological description is far too incomplete for readers to understand what has actually been done, and it is clearly insufficient to ensure reproducibility. Overall, the section lacks clarity, logical progression, and internal consistency. Concepts are frequently introduced without adequate explanation before being used in the analysis. For example, the introduction of seawater excess alkalinity (Alk*), seawater age, and their relationship with CaCO3 dissolution rates is much too brief. Without consulting Sulpis et al. (2021), it is difficult to understand the methodological framework underlying the study. Likewise, the introduction of temperature, salinity, mean age, and dissolved oxygen concentration as explanatory variables comes without explanation regarding how these variables are incorporated into the analysis. The descriptions of the CMIP6 and GeoMIP experiments are intertwined when they should instead be presented separately and clearly. Essential information regarding the climate models is omitted, forcing readers to retrieve this information from previous publications. Conversely, material that corresponds to discussions is included in the Methods, further reducing the readability of the manuscript.

- **I have already expanded upon the methodological description because of the edits I needed to make to address Reviewer #1's concerns. Seven of the eight variables either directly or indirectly influence the potential alkalinity through Eq (1); the remaining variable is mean age, whose linear relationship with potential alkalinity determines the calcium carbonate dissolution rates. Three (alkalinity, nitrate, and salinity) directly influence the potential alkalinity. The remaining four variables (phosphate, oxygen, total carbon, and temperature) that indirectly influence the potential alkalinity are each used in calculating the pH and/or dissolved inorganic carbon that each get used to calculate the alkalinity via**

the CO2SYS Matlab subroutine and/or (in temperature's case) influence the where the isopycnal layers are along which the final Alk*-age regression is performed. I intend to more clearly explain each of the CMIP6 and GeoMIP scenarios for the sake of completeness instead of forcing the reader to refer to other studies.

- 4. Figures The figures are well below the standard expected for publication. There are too many figures and very little effort has been made to synthesize the results into a coherent and accessible story. Rather than helping the reader identify the key findings, the figures largely add to the confusion. Figures suffer from poor readability, including small or missing labels, insufficient resolution, the absence of sub-panel lettering, and incomplete legends that make it difficult, or sometimes impossible, to distinguish the different curves or interpret what is being shown. Overall, the figures require substantial redesign to improve both readability and scientific communication.

- **In order to better focus the results, I have already relegated many of the figures (including new ones I have generated since responding to Reviewer #1's comments) to the Supplementary Information and only included the figures that are the focal points of the manuscript in the main text. I have also increased the label sizes and refrained from using the 'biome' labels (opting for acronyms for each ocean basin instead). I can add sub-panel lettering to make it easier to refer to particular panels as well. The resolution issues stem from my lack of familiarity with the new computers I'm using at my new job. I have been working on a**

solution to this and believe I found one.

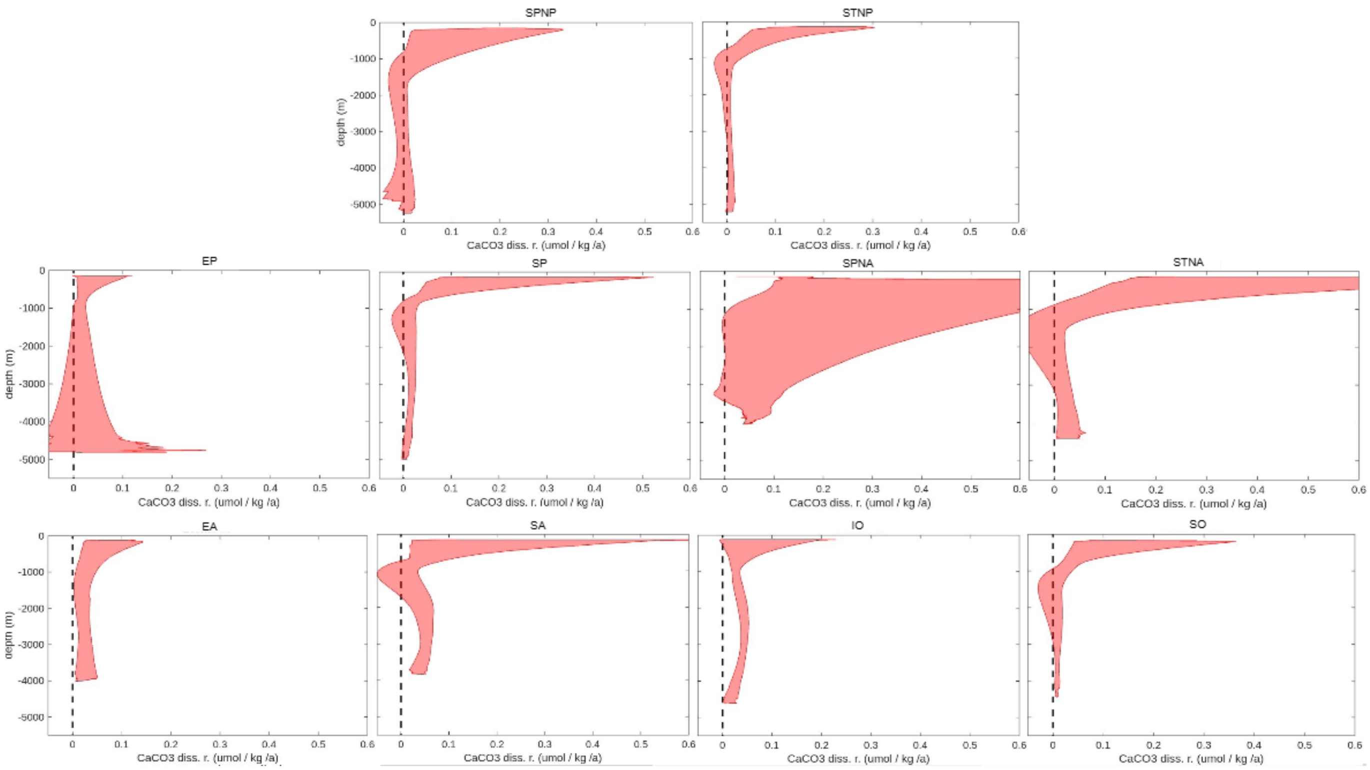

Figure 4: The ranges of calcium carbonate dissolution rates for different biomes (Fay and McKinley, 2014) or geographic regions (SPNP is the subpolar North Pacific Ocean, STNP is the subtropical North Pacific Ocean, EP is the equatorial Pacific Ocean, SP is the South Pacific Ocean, SPNA is the subpolar North Atlantic Ocean, STNA is the subtropical North Atlantic, EA is the equatorial Atlantic Ocean, STSA is the subtropical South Atlantic Ocean, IO is the Indian Ocean, and SO is the Southern Ocean) under the CMIP6 with perturbations in temperature (T), salinity (S), age (Age), dissolved oxygen concentrations (O_2) and GeoMIP scenarios with perturbations in temperature (T), salinity (S), age (Age), dissolved oxygen concentrations (O_2), nitrate concentrations (Nitrate), phosphate concentrations (Phosphate), alkalinity (Alk), and total carbon content (TCO_2). Here, the perturbation to the mean age in two of the GeoMIP scenarios is calculated from Carbon-14 via the empirical relationship described in the main text. The lower bound is 1.645 times the standard deviation of the Monte Carlo estimates lower than the mean of the Monte Carlo estimates within each bin for each biome. Similarly the upper bound is