Dear Reviewers,

We are very grateful for the time and effort you have invested into reviewing our manuscript. Your constructive feedback has been extremely valuable and we believe it has substantially improved the quality and clarity of the manuscript. We have carefully considered all comments and have done our best to address each of them.

To respond to the reviewers' suggestions, we have made several key revisions to the manuscript:

- We now state more clearly in several places that our results are only valid for open-ocean OAE.
- We have clarified the methods section describing the exponential particle size distribution, and we have added a supplementary figure showing the PSD as a function of particle diameter.
- Particle sizes are now consistently characterized in terms of diameter rather than radius, in line with the literature. We also explicitly state that PSDs are characterized using mean volume diameters to avoid confusion.
- We have added a supplementary figure showing the temporal evolution of the distribution of added alkalinity along the water column.
- The discussion of life-cycle emissions from comminution has been expanded.
- We have added a discussion about turbidity implications of fine particle powders.
- We have revised the conclusions paragraph to improve clarity and emphasis.
- We have clarified that the instabilities proposed by Yang and Timmermans were determined for much higher particle concentrations than considered in our study.

A detailed point-by-point response to the comments of reviewers 1 and 2 (following the reviewers' order in the decision letter) is provided below. All line numbers and figure references in our responses refer to the originally submitted manuscript.

Yours sincerely and on behalf of all co-authors,

Friedrich Burger

Response to the Reviewer 1

General Comment

The manuscript examines the effectiveness of slowly dissolving alkaline minerals at reducing atmospheric CO2 concentration in a global deployment scenario. A shrinking core model is used to calculate the depth profile over which alkalinity is deposited and this alkalinity flux is then coupled to an earth system model to examine the dynamics with which the alkalinity reaches the surface and causes an additional CO2 flux (relative to the counterfactual scenario). In itself, the paper sets out to explore a very straightforward question and uses reasonable approaches to arrive at an answer. The conclusion is that very finely ground olivine rock would be necessary for this approach to work efficiently.

However, what i feel is missing is a more comprehensive discussion about the realism of both the examined scenarios (open ocean, world wide, deposition of <2um olivine dust), the consequences of the conclusion that only very finely Olivine would work and the wider context of OAE development today, which is entirely focused on coastal deployment.

I am not aware of any commercial or other large scale effort to spread finely ground olivine in open sea, precisely because of the issues raised in this paper (which have been known for some time) and other issues (such a turbidity, see below). However, coastal deployments of Olivine and Brucite are being actively developed and they do not suffer from the issues raised in this paper - this distinction should be made very clear in the conclusions and in a sentence in the abstract, because the manuscript focuses on a variant of OAE which is not actually being actively pursued by the industry and that context should be made clear.

I recommend expanding the discussion of these issues in the last few sections of the paper. The reader should not be left with the impression that Olivine cannot be feasibly used for OAE in general (it can, but only in shallow ocean deployments) or that open ocean OAE isn't feasible (there are faster-dissolving alternatives to olivine, such as Ca(OH)2 and Mg(OH)2). These scope limitations are only briefly mentioned in Section 4.3 but I think they deserve much more visibility.

Response: We thank the reviewer for their positive assessment of our analysis and for their constructive suggestions to further improve the manuscript. In response, we have clarified that our study examines the effects of particle sinking in an open-ocean deployment scenario, we explicitly state that our conclusions regarding feasibility do not extend to coastal deployments. Although OAE deployment is currently discussed predominantly in a coastal ocean context, we believe the focus of this paper remains relevant. Several modeling studies (Keller et al., 2014; Schwinger et al., 2024, Nagwekar et al., 2026) assume open-ocean OAE deployment, and olivine is frequently considered as a potential feedstock (Köhler et al., 2013; Keller et al., 2018; Nagwekar et al., 2024). We hope that the revised text now clearly communicates the motivation for our analysis and the scope of its conclusions. We also thank the reviewer for the suggestion to address potential effects on seawater turbidity. We have included a discussion of turbidity changes in the text. Detailed replies to the specific points raised are given below.

Specific Comments

L28 Some other studies also examined large scale (GtCO2) scenarios based only on coastal releases such as Feng et al. (2017) and He et al. (2023). It would be worth discussing these in contrast with the uniform alkalinity addition studies mentioned.

Response: We now write: "Global modeling studies of OAE commonly assume instantaneous and complete alkalinity release at the surface of the open ocean (Keller et al., 2014; Nagwekar et al., 2024; Schwinger et al., 2024; Tyka, 2025; Zhou et al., 2025) or coastal ocean (Feng et al., 2017; He et al., 2023)." We also clarify in line 31: "Outside shallow and well-mixed coastal waters, slowly dissolving coarser particles sink quickly, releasing alkalinity at depth rather than near the surface."

L35 Also mention limitations due to turbidity and associated ecological problems.

Response: We prefer not to discuss turbidity at this point, as ecological impacts from OAE-caused turbidity are not sufficiently documented from the literature we know. However, we add a paragraph on turbidity in Section 4.3 (see comment below).

L78 What happens when z0 exceeds the depth of the bathymetry at that point ? Is the alkalinity assumed to become buried or does it all immediately dissolve in the bottom cell ?

Response: We assume the latter, as stated in line 171f: "Alkalinity release that is prescribed below the ocean bottom cell is added to the bottom cell."

L95/ Eq (7)/ Fig 1b The exponential particle size distribution seems like an odd choice to me for several reasons. It doesn't have any upper size cutoff - are we assuming occasionally giant chunks of olivine get through the grinding process ? My assumption was that during crushing processes there are sieves which only allow particles below a certain size to pass - is that not the case ? Either way, if particles larger than some size are effectively useless to OAE, any real-world process would presumably just sieve these out and feed them back to the grinder. This might be interesting to discuss somewhere ?

Response: Thank you for raising these questions regarding our PSD choice. In response, we have revised the section describing the exponential PSD in the methods (see below). From our perspective, introducing an explicit upper size cutoff is not necessary for the chosen distribution. The exponential PSD, a special case of the Weibull or Rosin-Rammler distribution with shape parameter 1, is a light-tailed distribution. This is in contrast to the power-law type distribution shown in Figure A5, for which an upper cutoff would indeed be required in an analytical representation. Moreover, when the exponential PSD is parameterized in terms of particle radius or diameter rather than volume, it corresponds to a Weibull distribution with shape parameter 3, again highlighting its light-tailed character (see new Supplementary figure below showing the PSD as a function of diameter).

Regarding the possibility to separate and regrind larger particles: while we cannot assess the practical feasibility of such procedure, it would clearly enhance OAE efficiency by shifting the PSD toward smaller particle sizes. We have therefore added the following sentence in line 363: "As such, separating and regrinding the largest particles in the mineral powder would, if possible, improve OAE efficiency."

It seems from Fig A5 that the exponential PSDs are poor approximations compared to the power-law like relationship obtained from Renforth 2012. Why not use a powerlaw function instead ? Would be good to have a better justification for the theory developed in light of the experimental data presented.

Response: The purpose for introducing a particle size distribution in our analysis is to test the effect of dispersion around a central particle volume on OAE efficiency. The Weibull or Rosin-Rammler distribution is a common model for mineral particle size distributions (e.g., Jillavenkatesa et al., 2001). As such, it represents a plausible light-tailed PSD to test the effect of particle size dispersion. The heavy-tailed power-law like PSD from Renforth 2012, on the other hand, spans orders of magnitude in particle size, which is thus not suited to test the effect of dispersion around a well-defined central particle volume.

Also because particle mass is a cube function of the radius distribution, the mean particle size maybe is not a good metric to describe the actual mass distribution, which is arguably more critical ? Therefore comparing uniform 1um particles to a exponential PSD with a mean particles size of 1um seems odd and perhaps leads, by definition almost, to the observed differences in dissolution properties.

Response: We agree that this would indeed be problematic. However, we do not characterize the three PSDs by the arithmetic mean particle radius, but by the mean volume radius, i.e. the radius corresponding to the mean particle volume across the PSD. To avoid confusion, we now refer to the PSDs by their mean volume diameters of 1.7 μm, 2.6 μm and 3.4 μm. In line with common practice in the literature, we now report particle sizes in term of diameter rather than radius.

I believe in the material processing world, quantities like P80 are used to describe grind sizes, which means a size at which 80% of the mass passes a sieve size, not 80% of the number of particles. Grinding costs are also usually expressed in terms of this, not in terms of mean particle size. I worry that using mean particle size unnecessarily distorts the picture.

Response: We agree. P80 refers to the sieve size, where 80% of the particle mass passes. The particle size distribution in equation 7 is a number distribution, i.e., $f(V)\mathrm{d}V$ characterizes the fraction of the total particle count in the particle volume interval $\mathrm{d}V$ rather than the fraction of total particle mass in $\mathrm{d}V$. Defining a PSD in terms of particle number is also a common choice (e.g. Jillavenkatesa et al., 2001) and better suited to characterize particle dispersion around a mean volume than a PSD in terms of particle mass. Formulas to calculate P80 from Eq. 7 were given in lines 97-99 in the previous manuscript version. This section is now revised and we explicitly state the corresponding mass distribution. As a function of diameter, it is given by a generalized gamma distribution with shape parameters p=3 and d=6, from which P80 can be directly calculated as the 80th percentile. We have revised the paragraph on the exponential PSD, now reading:

"Now, we consider the case where the mineral particles are not uniform but follow a certain particle size distribution (PSD). To test the effect of particle size dispersion, we here assume an exponential PSD for the distribution of particle number over particle volumes, with the probability density function given by

$$f(V) = \frac{1}{V_d} e^{-V/V_d}. \tag{7}$$

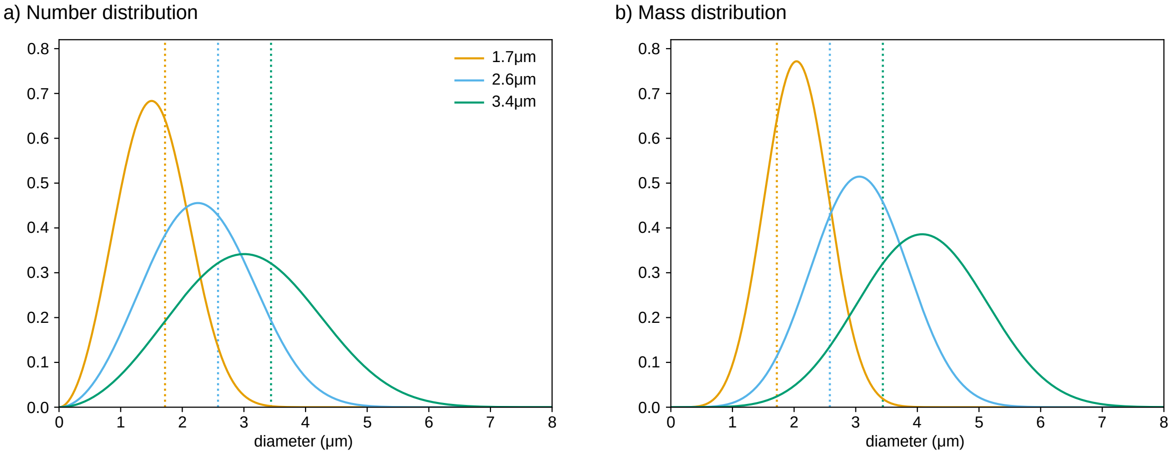

Figure A1: Probability density functions for the exponential PSDs analyzed in this study as a function of particle diameter. Panel a shows the distribution of particle number across diameters (a Rosin-Rammler distribution with shape 3 and scale given by the mean-volume diameter d) and panel b displays the distribution of particle mass (a generalized gamma distribution with shape parameters p=3 and d=6 and scale d). Vertical dotted lines display the mean volume diameters d of the three distributions.

The mean particle volume for this PSD is $V_d = \frac{\pi}{6}d^3$ for a given mean volume diameter d. As such, the PSD does not change the average particle volume and alkalinity content relative to uniform particles with the same mean volume diameter d, but adds dispersion around this average volume. As a function of diameter, the PSD is equivalent to a Rosin-Rammler (or identically Weibull) distribution with shape 3 and scale given by the mean-volume diameter d (shown in Appendix Figure A1a). The Rosin-Rammler distribution is a common model for mineral particle size distributions (e.g., Jillavenkatesa et al., 2001). The shape parameter of 3 indicates a light-tailed distribution. The distribution of particle mass for this PSD is a generalized gamma distribution with shape parameters p=3 and d=6 and scale d (Appendix Figure A1b), obtained by weighing the probability density for particle number by particle mass and then normalizing the result to 1. The P80 is the 80th percentile of this mass distribution."

L122-127 This section confused me a lot. You introduce an approximation on L123, assuming the particle doesn't shrink while traversing the mixed layer, given the approximate equation on L123, followed by the exact equation 11. So far everything is good.

L125 seems to justify using the approximation because Eq11 would require solving a third order polynomial. This doesn't seem particularly hard - why is is this prohibitive ?

But then you proceed to say that Eq 10 leads to the same third order polynomial ?

So in the end, is Eq 10 an approximation which assumes no shrinking or is it exact ?

Does it imply tMLD from L122 is the same as that from Eq 11 (I assume not) ?

Do you have to solve the third order polynomial or not ?

I'd recommend rewriting this section. If what you end up is an approximation, just say that and justify its reasonable accuracy. If you end up with an exact result anyways, just derive the exact result from Eq 10 and or Eq 11. If tMLD is just a stepping stone and all you care about is Chi and the Chi is exact, perhaps just derive that and skip the discussion of tMLD calculations entirely ? Either way, try and clarify the explanation/justification here.

Response: Thank you for raising this point. The purpose of the section L119-127 is to compare the fraction of alkalinity released within the mixed layer derived from our formulation (Eq. 10) with the approach used in previous studies (Köhler et al., 2013; Renforth and Kruger, 2013; Yang et al., 2025). In those studies, the fraction of alkalinity release is calculated from the reduction in particle size over time in the shrinking core model, $\chi(t_{\mathrm{MLD}}) = 1 - R^3(t_{\mathrm{MLD}})/R^3(0)$. This approach requires determining the residence time of a particle in the mixed layer, t_{MLD}. As the sinking velocity declines with particle size, calculating t_{MLD} requires to solve Eq. 11. As a simplification of the problem, previous studies assume a constant sinking velocity in the mixed layer, such that mixed layer residence time simplifies to $t_{\mathrm{MLD}} = MLD/v_{\mathrm{sink}}(R(0))$. In contrast, our expression for the fraction of alkalinity release in the mixed layer (Eq. 10) does not rely on this assumption. It is identical to the exact solution obtained by first solving equation 11 and then using the exact residence time in $\chi(t_{\mathrm{MLD}}) = 1 - R^3(t_{\mathrm{MLD}})/R^3(0)$.

We have revised the section to clarify this point: "Previous studies have calculated the fraction of alkalinity released in the mixed layer based on the decline in particle size during the mixed layer residence time t_{MLD} in the shrinking core model, $\chi(t_{\mathrm{MLD}}) = 1 - R^3(t_{\mathrm{MLD}})/R^3(0)$, assuming a constant sinking velocity within the mixed layer such that $t_{\mathrm{MLD}} = \frac{MLD}{v_{\mathrm{sink}}(R(0))}$ (Köhler et al., 2013; Renforth and Kruger, 2013; Yang et al., 2025). Our result (Eq. 10) does not require this approximation and is equivalent to computing $\chi(t_{\mathrm{MLD}})$ using the exact residence time obtained from $\int_0^{t_{\mathrm{MLD}}} v_{\mathrm{sink}}(R(t))\mathrm{d}t = MLD$ for t_{MLD}."

L164 landuse → land use

Response: Changed as suggested.

> L185 While true that reservoir feedbacks will cause the total CO2 removal from the atmosphere to be lower than the gross efficiency of 0.8-0.85, the same feedbacks also apply to emissions of CO2. But mCDR credits are generally measured as “negative emissions” (i.e. 1tCO2 of CDR offsets 1tCO2 of emissions and is credited as 1tCO2 “removed”). Previous work (https://iopscience.iop.org/article/10.1088/1748-9326/ad5a27 and https://egusphere.copernicus.org/preprints/2024/egusphere-2024-2150/) discuss this in more detail.
>
> I'd add a sentence explaining this relationship between gross and net efficiencies that the gross efficiencies are what applies to carbon credits.
>
> One could potentially focus on expressing the efficiencies of the sinking rock-based simulations relative to the surface-dissolving reference simulation (though I understand there is already a lot of noise in these curves (Fig 3)).

Response: We agree with the reviewer's perspective and add: ”This net ocean capture efficiency including the carbon cycle feedbacks is lower than the gross efficiency of 0.8-0.85 without the adjustments in natural carbon reservoirs (Grosselindemann et al., 2026). The net efficiency is relevant for studying the carbon, climate and ocean acidification responses to OAE, while gross efficiency characterizes the negative emissions due to OAE, thus important for emission budgets and carbon credits.”

While it is interesting and intuitive to express efficiencies relative to that for surface dissolution (we do that at several places in the text when making statements like efficiency is less than half of that for surface dissolution), we agree that the strong year-to-year variability prohibits calculating meaningful efficiency ratios from the yearly data.

> Figure 3. These figures are difficult to parse. I'd recommend the following:
>
> Panel a) Use dashed and dot-dashed lines to distinguish groups of related graphs. Perhaps Make the PSD graphs dashed and the surface dissolution dot-dashed ?
>
> Panel b) The annual average values are extremely noisy and impossible to parse. I dont see how they add any value plotted as such. I think showing the 31-year running means is sufficient to get your conclusions across.

Response: We have revised the figure as proposed, and have also added the averages over the first 10 years in panel b (that are discussed in the text):

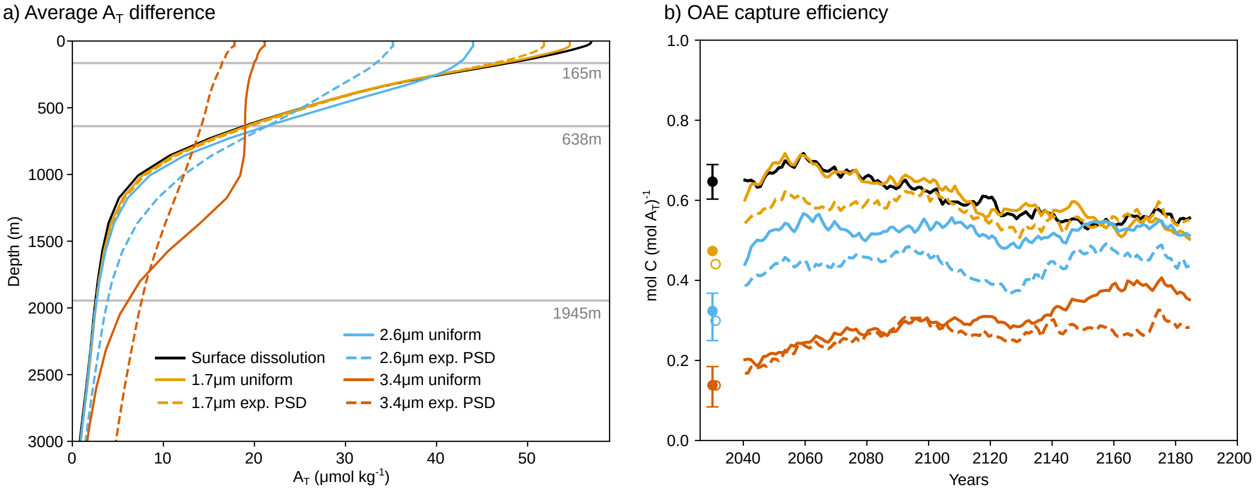

Figure 3: (a) Global mean difference in total alkalinity between the OAE simulations for surface dissolution and uniform as well as exponentially distributed particle size distributions and the baseline simulation without OAE, averaged over 2026-2200. The horizontal gray lines indicate the global mean dissolution depths z_0 for the three particle diameters 1.7 μm, 2.6 μm, and 3.4 μm. (b) The OAE efficiency for these simulations over time, defined as the number of moles of additional carbon uptake in the OAE simulations divided by the number of moles alkalinity added. The lines show 31-year running means. The dots indicate averages over the first 10 years of the experiment, with hollow dots for the PSD experiments. For surface dissolution and the 2.6 μm and 3.4 μm uniform cases, the five-member ensemble ranges and ensemble means shown.

> L260 This upwelling of deeper alkalinity is quite interesting. Is there a way to show a plot of vertical alkalinity evolution over time ? This would be cool to see for all the different runs shown in Fig 3. Figure A6 only shows pH difference. How about an analogous plot for ΔAlk ?
>
> However that might wash out the effect mentioned in L260, perhaps doing a vertical cross-section of just the tropical upwelling regions ?

Response: Following the reviewer's suggestion, we have added a new appendix figure showing the evolution of the alkalinity difference between different vertical alkalinity release scenarios and surface alkalinity addition along the watercolumn over time. We do not show the alkalinity difference for the uniform particle profile with $d = 1.7\,\mu$m, as differences are minor. Additionally, we also added a 6th panel showing the alkalinity difference in the equatorial Pacific (-8 °N to 8 °N, -190 °E to -85 °E) for the uniform case with particle diameter of 2.6 μm (also shown in Fig 4).

We now add a reference to this new appendix figure in L220f: "Each profile type (uniform and exponential PSD) and particle size results in a characteristic vertical distribution of additional alkalinity relative to the reference simulation without OAE (Figure 3a, Appendix Figure A4)."

Additionally we add in L270ff: "The increase in carbon uptake in these regions suggests that additional alkalinity released at subsurface in other regions is transported there, where it comes in contact with the atmosphere. In the eastern and central equatorial Pacific, for example, we find a distinct buildup of alkalinity in the upper 1000 m, likely due to an inflow of alkalinity from North and South of the equatorial Pacific region (Appendix Figure A4f)."

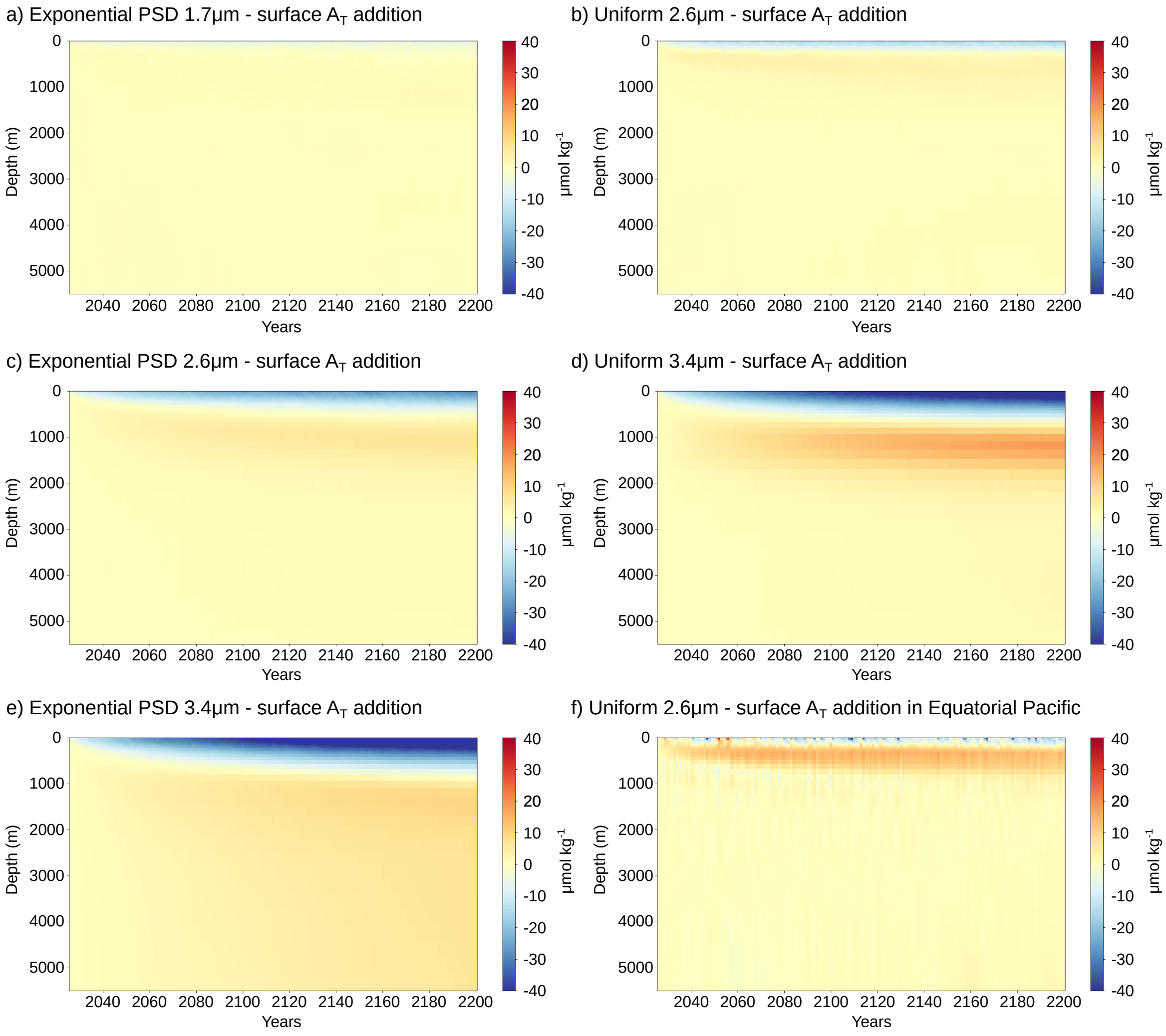

Figure A4: Global mean vertical distribution of additional alkalinity relative to that for surface alkalinity addition over time for the uniform and exponential PSDs with mean volume diameters of 1.7 μm, 2.6 μm, and 3.4 μm (panels a-e). We do not show the alkalinity difference for the uniform particle profile with $d = 1.7\,\mu$m, as differences are minor. Panel f shows the alkalinity difference in the eastern and central equatorial Pacific (-8 °N to 8 °N, -190 °E to -85 °E) for the uniform case with $d = 2.6\,\mu$m.

L301: …may further reduce the efficiency of olivine-based open-ocean OAE.

Response: Changed as suggested.

L361-363 This seems to confirm that mean particle size is not a good metric to describe the PSD of a a bunch of ground rock for purposes of OAE. P80 or mean mass size would be better.

Response: In our study, the PSDs are characterized by the mean volume size (equivalent to mean mass size), i.e. the diameter corresponding to the mean particle volume (see also response above). We now explicitly refer to the mean volume size in the manuscript.
We prefer characterizing the PSDs in terms of mean volume size instead of P80, because our primary objective is to compare OAE efficiency between the case of uniform particles of a certain volume and the case of a PSD with the same average volume.

L363: Can't simple sieving trivially remove the heavy tail of the distribution ?

Response: We can not judge the technical feasibility, but it would definitely help removing the too large particles from the PSD that would else sink deep without significant alkalinity release in the surface ocean. We have added a sentence on this idea in line 363 (see answer to comment above).

L364-368 One major conclusion of the paper is tucked away in L364-368: Olivine is found to be unsuitable for open ocean deployment, because it must be ground <1um which is energetically and financially incompatible with affordable negative emissions. I would recommend expanding this section and discussing these economics more explicitly, stating estimated costs per tCO2 absorbed, for different grain sizes, accounting for efficiency losses: Coarse grinding is cheap but sinks too far, fine grinding dissolves but is too expensive.

Response: We agree that a discussion of costs per tCO_2 captured for different grain sizes would be highly valuable. However, to our knowledge, no study currently provides cost estimates resolved by particle size.

In response, we have expanded the discussion of the energetic costs and associated life cycle emissions, focusing specifically on emissions from grinding. We write: "The strong dependence of carbon capture efficiency on the particle size distribution makes particle comminution to grain sizes near $1\,\mu$m for olivine-based open-ocean OAE imperative. However, grinding olivine to such fine grain sizes strongly increases energy demand and associated costs and emissions. Foteinis et al. (2023) estimated CO_2 emissions to increase from $0.04\,tCO_2\,(1.25\,t\ \text{olivine})^{-1}$ for $10\,\mu$m particles to about $0.21\,tCO_2\,(1.25\,t\ \text{olivine})^{-1}$ for $1\,\mu$m particles from comminution alone, translating to reductions in OAE capture efficiency by 0.03 and 0.13 mol C $(\text{mol}\ A_T)^{-1}$, respectively. Additionally, such fine olivine powders might also increase particle aggregation (Köhler et al., 2013), negative impacts on zooplankon (Fakhraee et al., 2023), and may be harmful to human health (Doelman et al., 1990)."

> Section 4.3: Another issue with very finely ground rock is that it causes substantial turbidity which stresses many sea organisms. It would be worth discussing this here as it is another reason why super-finely ground olivine is likely a no go, even if grinding costs could be overcome.
>
> Can the authors add some analysis of the expected steady state turbidity increase (Total suspended solids) if OAE was deployed, say, at 100Mt/yr or 1Gt/yr scale, using the particle sizes used here and using a uniform deployment (as modelled here, ignoring shipping costs for the moment) over the whole ocean ? It seems the shrinking core and vertical distribution model could easily calculate the steady state for such a deployment.
>
> How much would it change relative to typical ocean conditions and its variance ? Would it exceed federal EPA or EU limits for marine waters ?
>
> How does the turbidity situation change with increasing dissolution rate (going to faster dissolving minerals)

Response: Turbidity is an important topic. However, it is difficult to assess the impact of OAE-caused turbidity as literature is scarce. Guo et al., 2024 add olivine particles ($1.9\,\mathrm{g\,l^{-1}}$), more than 3 orders of magnitude larger than what we accumulate in a 50 m mixed layer over one year in our simulations ($0.3\,\mathrm{mg\,l^{-1}}$), finding no negative impacts on mesocosm plankton communities. However, they report particle sizes of 150-250 μm, which are about 2 orders of magnitude larger than ours, implying that we add about 2 orders of magnitudes more particles per volume compared to Guo et al., 2024. The number of particles per volume is further increased for the PSD reported by Renforth (2012) where a considerable fraction of particle mass is occupied by the finest size classes, such that the mean volume diameter is only about 0.086 μm (Appendix A). As such, adding $0.3\,\mathrm{g\,l^{-1}}$ with the Renforth PSD implies adding a million times the particle number in Guo et al. (assuming that the particles by Guo et al. have a mean volume diameter of 200 μm). However, turbidity does not necessarily increase with the amount of small particles (e.g., Yao et al., 2014; Figure 5 therein), rendering the influence of very fine olivine powders, as required in our experiments, uncertain.

We add a discussion on turbidity effects in L370: "The micrometer scale olivine grain size necessary for open-ocean OAE and associated high particle amounts may also impact marine ecosystems due to increases in turbidity. While no detrimental impacts were observed for coastal plankton communities with coarser olivine powder (Guo et al., 2024), turbidity-associated impacts on plankton should be investigated with micrometer-scale olivine powder, and ideally also for open-ocean ecosystems."

> L375-L378: Shaw et al 2025 (https://www.frontiersin.org/journals/climate/articles/10.3389/fclim.2025.1616362/full) showed that precipitated Brucite (using alkalinity exchange) dissolves orders of magnitudes faster than mined brucite due to poorly formed crystals and would thus be very suitable for open ocean OAE. This would be worth mentioning here as a viable path forward for open ocean OAE.

Response: Thank you for drawing our attention to this publication. We have now added a short discussion of this study to line L378. We write "Importantly, dissolution rates of brucite have been shown to vary by orders of magnitudes depending on mineral properties (Shaw et al., 2025), rendering particle size constraints for effective brucite-based OAE in the open ocean uncertain."

> L368: Please add a paragraph in the conclusions emphasizing that this research does not preclude the deployment of coarser Olivine in shallow seas which is a viable option and does not suffer from the deep dissolution issues discussed in this paper. This is currently only stated briefly in one phrase on L368 and would likely be missed by readers who may come away with the impression that Olivine-based OAE is fundamentally infeasible. Shallow sea deployment of coarsely ground olivine appears to be economically feasible approach that is being scaled currently. FWIW it would be valuable to examine the CO2 uptake kinetics of shallow-ocean floor release of alkalinity in future work.

Response: We have adapted the conclusions paragraph to highlight more strongly that it refers to open ocean OAE (see comment below). Picking up the proposed future research direction, we also expand the sentence on coastal alkalinity enhancement in L368 and now write ”These problems with olivine grain sizes near 1 μm call into question the feasibility of olivine-based OAE in the open ocean. However, olivine may remain suitable for coastal applications where the mixed layer extends to the seafloor, such that alkalinity released from coarser olivine grains in the sediments enhances carbon uptake from the atmosphere (Renforth, 2012; Montserrat et al., 2017; Foteinis et al., 2023). Future research should constrain the carbon uptake from OAE in shallow coastal seas, when alkalinity is released from sedimented olivine grains.”.

> Conclusions statement (section 5)
>
> This section should be rewritten to give more concrete, stronger final conclusions of the clear implications and insights from the work done, in summarizing sentences. Many readers, especially in adjacent fields, will read the abstract and conclusions only so it's worth reiterating every major insight in short form here. Some clear statements that should be included (non-exhaustive list):
>
> Open ocean mCDR using olivine requires < 1uM particle sizes, which are noneconomical and potentially run into turbidity limits. For these reasons olivine addition is likely limited to shallow coastal regions (where it is currently being operationalized). Given conclusion 1., this mode of OAE should be a major direction of future research. Open ocean mCDR can still work with rapidly dissolving materials such as precipitated Mg(OH)2, NaOH or preequilibrated/predissolved approaches (e.g https://www.science.org/doi/full/10.1126/sciadv.adr7250) which don't suffer from sinking problems or excessive, persistent turbidity.

Response: Thank you for the suggestion. We revised the conclusions paragraph:

”In summary, the efficiency of mineral-based alkalinity enhancement in the open ocean and associated CO_2 uptake is highly sensitive to feedstock particle-size characteristics. Insufficient comminution can substantially reduce carbon capture efficiency and delay CO_2 uptake by decades to centuries, often shifting it far from the deployment site. Such temporal and spatial lags complicate monitoring, reporting and verification and challenge carbon crediting schemes based on short-term removal. For olivine, required grain sizes near 1 μm likely render its application in the open ocean unfeasible. More rapidly dissolving minerals may prove to be suitable for efficient alkalinity enhancement, when mineral particle properties are closely controlled. Our findings emphasize the need for integrated process–energy–climate assessments to evaluate the feasibility of mineral-based alkalinity enhancement in the open ocean.”

L415: The term theta (θ) appears but I can't find a definition of it. Is this explained somewhere else?

Response: Thank you for the detailed check of the calculation. θ represents the Heavyside function $\theta(x)$ which is 0 for $x < 0$ and 1 else. We added the missing definition of the symbol in line 417: "with θ representing the Heavyside step function ($\theta(x) = 0$ for $x < 0$ and 1 else) and $V_{\min}(z)$ the minimum initial volume of a particle to still be present at depth z (Equation 8)."

Response to the Reviewer 2

General Comment

This paper describes how particles, that might be used for ocean alkalinity enhancements (OAE) in order to reduce atmospheric CO2 are dissolving in the water column of the ocean and where they eventually might add alkalinity to the ocean as function of particle size and time. It is with respect to the dissolution process an update to Köhler et al. (2013) with some more details and the use of more recent parameters. The results show, that doubling the size of particles will increase the depth at which the particles are fully dissolved by a factor of 12, consequently reducing the alkalinity release near the ocean surface where it might influence the marine CO2 uptake capacity.

This is very nice piece of work and I have hardly anything to comment, which might improve the study, since it is already in a very good shape.

My only comment is on the references to Yang & Timmermans 2024 and Yang et al 2025, (lines 311, 342) which commented on the results of Köhler et al (2013) and suggested that due to instabilities in the water column - caused by the added material - particles might sink faster and therefore dissolve in deeper waters having a smaller effect on the CO2 update capacity of the OAE method. In these studies the authors assume an input of olivine of 15g/l citing laboratory studies where such numbers have been applied. However, in the open ocean context and the applied OAE used here and in other studies the amount of added particles is much lower. These 15g/l, or 15 kg/m^3 would transfer to 750 kg/m^2 if we assume a distribution over the whole of the ocean surface mixed layer with roughly a mean depth of 50 m.

In Köhler et al. (2013) at maximum 10 Pg of olivine was added to the ocean per year . Here, 0.14 Pmol alkalinity/yr is added in the control run (line 177), which is about 5 Pg of olivine/yr. Distributed across the ocean surface of the order of 360 10^{12} m^2 (slightly smaller here when avoiding high latitudes) gives 28 g/m^2 or 0.028 kg/m^2. This is more than 4 orders of magnitudes smaller than the numbers used in Yang & Timmermans 2024. The authors here might therefore savely say, that the suggested instabilities which might increase the sinking speed of particles will probably only appear if particles are distributed in a very dense and localized setting.

Response: We thank the reviewer for the positive feedback and the comment on the potential for faster sinking due to water column instabilities. We have revised the discussion of the results by Yang & Timmermans 2024 and Yang et al 2025 in line 311ff according to:

"Additionally, when mineral particles are added over a small area such that high mineral particle concentrations near 15 g l^{-1} are reached, fluid instabilities may considerably enhance the sinking velocity (Yang and Timmermans, 2024; Yang et al., 2025). While such mineral particle concentrations in the mixed layer are not reached in our idealized global experiments, where 15 g olivine per square meter and year are added, instabilities may occur in more realistic localized application schemes."

References:

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