

RC #2

We thank the reviewer for their comments, as they have substantially improved the draft. Below we present the original question, followed by our response.

1. Title: To my mind, the title poorly reflects the content of this paper. My first question was “Desaturation and Precipitation of what??”. Maybe desaturation and precipitation are well-understood terms in groundwater research, but I had to read the introduction to figure out the main research question, and even then, it was not fully explicit. Perhaps a more comprehensive title could be along the lines of “A multi-phase biogeochemical model for improving soil stability through microbially-induced water desaturation and calcium carbonate precipitation”, or similar. Line 20 in the abstract should make it clear that we are talking about N₂ gas here, since methane was the gas that first sprung to mind for me.
 - a. **We adjusted the title to be the following: “A Multi-phase Biogeochemical Model for Mitigating Earthquake-Induced Liquefaction via Microbially Induced Desaturation and Calcium Carbonate Precipitation”**
2. The introduction jumps straight into the topic with very little relevant background. Key facts are missing, such as: Why is liquefaction important? How much of a problem is it in today’s society? What methods are currently used to tackle it? Is MDIP treatment a one-off exercise, or does it involve continuous application. Is the basic idea of desaturation that production of N₂ gas reduces the partial pressure of H₂O(g), thus favoring subsurface evaporation? And so on. Without such essential background, I got a little lost here trying to find the rationale for this study and its wider implications.
 - a. **We augmented the intro paragraph to read: “Microbially induced desaturation and precipitation (MIDP) is a biogeotechnical technique that takes advantage of native subsurface denitrifying bacteria to mitigate earthquake-induced soil liquefaction (O’Donnell et al., 2017a, b; Pham et al., 2018). MIDP mitigates liquefaction in two ways: generation of nitrogen gas (N₂) and mineral precipitation (usually calcium carbonate, CaCO₃). The generated N₂ desaturates the soil, increasing its compressibility and reducing the increase in pore water pressure during cyclic loading, which is the root cause of earthquake-induced liquefaction. The carbonate precipitation increases the soil strength, thereby increasing the intensity of earthquake shaking necessary to trigger liquefaction. A primary benefit of MIDP for liquefaction mitigation is, being non-disruptive, it can be used underneath existing structures (O’Donnell et al., 2017a; Hall, 2021). Trillions of dollars of existing infrastructure are at risk due to the potential for liquefaction, and there is currently no cost-effective way to mitigate that risk. MIDP may be a**

potential solution and is currently being evaluated at different experimental scales (O'Donnell et al., 2017a, b; Moug et al., 2022).

b.

3. The basic set-up of the model is not described and instead refers to earlier studies (which I do not have time to read). Please supply some more basic information. Is this a batch model, or a reaction transport model (1-D. 2-D ?). After reading the results, I presume this is a batch model. If not, what are the model dimensions, physical set-up (e.g. solid/liquid/gas fractions of pore space, grid-spacing etc), boundary and initial conditions? I presume that the scenario tested is for an anaerobic environment? How realistic is this assumption in actuality?
 - a. **We added more text surrounding the model set-up; please see RC1, Comment 3.**
4. Why is a model that explicitly includes biomass growth and decay favorable over one where biomass is treated implicitly? Such a model comes at the burden of significant additional parameterization, with some parameters arguably poorly known such as microbial decay (see comment below). It would be instructive too see how sensitive the results are to the decay constant.
 - a. **We opted to consider decay explicitly to avoid over- and under-estimations from responses from environmental conditions (i.e., increased decay with increase in temperature), observed in implicit models. As such, we are better able to look at sensitivity and simplify the model. While this may be an oversimplification, we will include this sensitivity analysis in our supplementary information along with our analysis on initial biomass concentration.**
5. The model includes nitrate reduction to nitrite, and nitrite reduction to dinitrogen. It seems as though a key process is missing here, namely, anammox (anaerobic ammonium oxidation by nitrate, producing N₂). Ammonium is produced from the decaying microbial detritus, and anammox is widespread in anaerobic aquatic environments. Why was this not considered, and would its inclusion impact the treatment recipes? Some careful discussion is needed here.
 - a. **Due to the high concentration of added acetate and the lack of ammonium and nitrite accumulation, anammox is not relevant. Anammox is an autotrophic (i.e., not heterotrophic) process in which ammonium is the electron donor and nitrite is the electron acceptor. In our situation, acetate is the (heterotrophic) electron donor and nitrate or nitrite is the electron acceptor, while ammonium is present in small concentrations due only to decay of heterotrophic biomass. Thus, none of the features that make anammox important are present in our situation. .**

6. It is not clear to me whether, in the real world, the treatments would be continuously applied. The model results shown seem to imply that once NO₃ is exhausted, SO₄ gets depleted and H₂S accumulates. How does this relate back to a real-life scenario? Are we to expect that H₂S gas will be released through the pore space at some point?
 - a. **If desaturation is the desired outcome, a single treatment appears to be sufficient for liquefaction mitigation in granular soil (O'Donnell et al., 2017a, b). However, if the persistence of desaturation is uncertain and carbonate precipitation sufficient to mitigate liquefaction is also desired, several treatments may be required. Further, gas starts to migrate upward or spread laterally at a desaturation level of ~20% for poorly graded (i.e., uniform) fine sands (Pham, 2017). Its migration varies depending on the site's geology and stratigraphy (van Paassen et al., 2017). Since we never reach that high of desaturation for any of the gasses produced at the depths we model, we expect all gas to remain in the subsurface.**
7. The microbial growth parameters are derived on a quasi-first-principles basis in the supplement, which is nice to see. However, the derivation of the mortality rate constants is not part of this treatment. How were these values constrained, and how sensitive are model outputs to these values?
 - a. **We used the decay parameter to reflect the aggregate mortality of the biomass. This number was assumed to be 0.05 d⁻¹ based on reported values by Rittmann and McCarty (2020).**
8. Is there a typo in Table 4? Second column "sulfide". Sulfide cannot be reduced, or have I misunderstood this?
 - a. **Thank you for catching the typo. This has been changed to be "sulfate"**
9. L208: K_a is given in units of L/d. Ca²⁺ is in mol/L (Table 1). This seems to conflict with the rate units of mol/L/d (Table 1). Should K_a in fact be in /d?
 - a. **Thank you for catching this. Yes, k_a should be d⁻¹. We adjusted this throughout the manuscript.**
10. L229: Typo: consider
 - a. **This has been fixed thank you for catching the typo.**
11. L229: Only the treatment optimized for desaturation is tested. Is there a good reason why precipitation as a liquefaction-mitigation mechanism is ignored? This seems at odds with the main thrust of the manuscript since, up to this point, the focus is on

both mechanisms (including the title of the paper!). If calcite precipitation were the desired treatment, how does the model deal with the ensuing reduction in pore space?

- a. We are preparing a separate paper on precipitation because our conclusion from that analysis is that achieving precipitation is technically inconsistent with desaturation (too much gas, e.g.), and we want to make that point strongly in its own paper.
- b. The model is a batch and we have clarified this further in the manuscript.
- c. Our recent work shows that desaturation can be expected to persist for decades in most geologic environments where this technique is feasible. See our response to RC2, Comment 6.

12. Table 5: Seawater Ca^{2+} is ~ 10mM, but the results (Fig. 2) show that Ca^{2+} at the start of the simulation is in excess of 20 mM. Please explain. Maybe this all becomes clear with a clearer description of the model initial conditions etc. Is Table 5 the initial condition?

- a. **The higher concentration of available calcium is from the input treatment recipe. Both the electron donor and electron acceptor are introduced as calcium acetate and calcium nitrate. We have added the following to reduce confusion:**
- b. **“Table 5 does not reflect the added treatment recipes of varying concentrations of calcium acetate and calcium nitrate, which are described later.”**

13. Results plots: I don't know how to interpret these plots since it is not clear whether the data represent a one-off addition of a treatment or a continuous flow-through.

- a. **We added the following to the text:**
- b. **“We assumed that each treatment recipe was added in one application (i.e., not in a continuous flow-through manner).”**