

## ***Interactive comment on “Increased carbon capture by a silicate-treated forested watershed affected by acid deposition” by Lyla L. Taylor et al.***

**Lyla L. Taylor et al.**

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Reviewer 2: Overall, I thought this paper was excellent. The manuscript is polished, thorough, and well structured in way that presents a data-heavy study in a concise manner. Testing ways to remove carbon dioxide from the atmosphere is critical for mitigating the response to anthropogenic climate change, which makes this paper of particular significance. I only have one query which I would like some comment on in the discussion, with a couple of minor comments. I recommend that the paper is accepted after these are addressed.

Response: Thank you!

Reviewer 2: Discussion: The long term efficacy of carbon capture and storage, both in

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geological and modern examples, seems to hinge on whether organic (via biomass) or inorganic (via carbonate) carbon are the dominant sinks for increases in atmospheric CO<sub>2</sub>. Given that this study concludes that uptake into biomass is an important factor for carbon storage in the catchments, what does this mean for the ability of experiments such as this to function on longer time scales (i.e. >100 years). Is this a one-off procedure that can be implemented on a catchment, or can it be repeated with a minimum repose time? Will the draw down via organic and inorganic pathways change with repeated treatments perhaps? I know this is going to be speculative, but I think it would be beneficial for the authors to share their thoughts on how this may be able to be integrated into long term catchment management strategies.

Response: Yes, this would be speculative but we can discuss how this type of treatment integrates with long-term management and organic/inorganic pathways of carbon sequestration. Such a discussion would come at the very end of the discussion and would be a good way to end the paper given that Reviewer 1 recommended removing the Conclusions. Forestry practices such as harvests are known to produce changes in biogeochemical cycling and ion export in streams, potentially affecting decisions re timing of treatments and need for repeat treatments. This may be particularly pertinent for evergreen forests which may be difficult to treat from the air, but also for deciduous forests with high productivity.

Reviewer 2: Minor comments: Line 30: 71 degrees west, rather than -71 degrees east

Response: We are happy to change this (but will comply with journal guidelines if they prefer degrees East).

Reviewer 2: Line 120: Repetition of "Mohseni and Stefan"

Response: Yes, Endnote added the authors' names again. We can remove "following Mohseni and Sefan".

Reviewer 2: Line 127: What does "mm/time" mean?

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Response: This means millimeters (cubic meters of water per square meter of land) per unit time. We calculated rolling annual fluxes at the sampling interval of the input chemistry data (approximately one month). We can change this to mm per year.

Reviewer 2: Line 192: Repetition of "Battles et al."

Response: Endnote can probably put in a reference without repeating the author names; alternatively we can reword the sentence to avoid the repetition.

Reviewer 2: Line 350: Replace "3.4 4" with "3.44"

Response: Thanks for catching this error, we are happy to fix it.

Reviewer 2: Line 418: A possibility for a low cost alternative to wol-lastonite could be volcanic ash (see e.g. Longman et al., 2020; <https://doi.org/10.1016/j.ancene.2020.100264>), particularly in catchments with volcanic deposits nearby

Response: We are happy to cite Longman et al in the paragraph starting on line 418, as an alternative lower-cost treatment along with mine waste. Soils located very close to volcanoes probably already contain considerable volcanic material but transport by ship may be viable.

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