Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2019-46-RC2, 2019

Biogeosciences
Discussions
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Interactive comment on "Biological enhancement of mineral weathering by *Pinus sylvestris* seedlings – effects of plants, ectomycorrhizal fungi, and elevated CO_2" *by* Nicholas P. Rosenstock et al.

Anonymous Referee #2

Received and published: 24 April 2019

The ms describes an experiment to assess the relative influence of plants and two associated ectomycorrhizal fungi on weathering budgets. By executing the experiment under both ambient and elevated CO2 the authors also wanted to address the issue whether elevated CO2 would affect weathering rates. In the experiment appropriate controls without plants were included. It is a pity that the supposedly non-mycorrhizal control was (partly) mycorrhizal. While molecular methods could have been used to identify that fungus, I would not think this is a major problem, as the paper does not make any claim about ectomycorrhizal fungal enhancement of weathering rates. How-

ever, it may be preferable to refer to the treatment as the control or non-inoculated treatment rather than to the non-mycorrhizal treatment. While in my view the design of the experiment is OK, I found interpretation of the data more complicated. Part of the data certainly are in support of (or are at least consistent with) a biotic mechanism that enhances weathering, for which reduced transport limitation is proposed as the driving factor. However, in order to focus on the weathering story some inconvenient facts do not receive the attention that they deserve in the view of this reviewer. Negative weathering losses (Table 5) for Ca (246 μmol) and Mg (175 μmol) in the unplanted controls do not receive much attention. The authors refer to this negative value as a missing sink and suggest that the cause may be sought in what happened in the three-week flushing phase before planting. They also suggest that, were this explanation correct, the effect would be similar for both planted and unplanted treatments, and hence would not affect the calculations. While that may be true, that explanation fails to provide any suggestion why that missing sink is so different for K (no missing sink at all) and Mg. This reviewer would like to know better how likely the flushing effect was. If that effect was major, one would expect also relatively large leaching losses in the first leachates compared to the leachates that were collected towards the end of the experiment (as the missing sink implies leaching losses in the period that there were no measurements undertaken). It may then be interesting to connect these to the observed leaching losses (Table 2) for Ca and Mg that differ almost an order of magnitude. As the ms states that the eleven leachates were all analysed separately (p. 4, l. 20-22) I think that the temporal pattern for leaching losses would allow a better evaluation of the arguments for the missing sink. In my view the crucial table 3 (with ΔEC) demands more reflection; and providing data on the time course of ΔEC would be very helpful. Whereas many previous studies have shown a large role for ectomycorrhizal fungi (certainly members of the Boletales like Suillus, Paxillus and Rhizopogon) in mineral weathering and a small role for non-mycorrhizal seedlings in weathering, this study does not find no evidence for an ectomycorrhizal fungal role (despite the title), nor does it find evidence for the production of di- and tricarboxylic acid production by ectomycorrhizal fungi. The discussion on that discrepancy is (too) short in my view. Also the lack of effect of elevated CO2 on the weathering budget (even though it increased allocation belowground and production of LMWOA) is somewhat curious in view of earlier (presumed) knowledge on the role of ectomycorrhizal fungi in weathering. Based on these results the authors of this ms conclude that production of organic ligands (the anions of these LMWOAs) are not the main mechanism for weathering. As they also did not find lowering of pH, they also state that that hypothesis (acidification) can be refuted as a main mechanism for weathering. The ms lists two further mechanisms, but while physical disruption is mentioned, the data are not discussed in relation to this theory. The authors then suggest that alleviation of transport limitation is the driving mechanism. I am not sure whether I understand this hypothesis correctly. It seems that the concentration in the soil solution is higher than plant demand (as leaching losses are substantial compared to plant uptake), so why (to put it in anthropomorphic terms) would plants increase weathering rates way beyond their demand? What I found somewhat surprising that no attention is given to the possibility of (some) weathering as a consequence of autotrophic respiration (by roots and ectomycorrhizal fungi). Root respiration has been proposed as a major weathering agent; and while the authors may disagree with that point of view, I think it is fair that they discuss this possibility. Considering the likely large difference in contribution by heterotrophic respiration (based on low fungal biomass in Fig. 1) and autotrophic respiration, I think the issue merits more attention. While the causes for the high pH of the leachates remains unknown, one could well imagine that increased CO2 production would have lowered leachate pH (Figure 4).

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