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Comment

Interactive comment on “Weathering by tree root-associating fungi diminishes under simulated Cenozoic atmospheric CO₂ decline” by J. Quirk et al.

R. Finlay (Referee)

Roger.Finlay@slu.se

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This paper deals with the general theory that symbiotic mycorrhizal fungal hyphae, associated with different plant hosts, are able to use plant-derived carbon to weather mineral surfaces, mobilizing nutrients that become available to their plant hosts. The theory is not totally new in itself and has been put forward by a number of authors with varying degrees of experimental support. The specific advance made by this study is the use of vertical scanning interferometry imaging of nanoscale surface topography of muscovite and basalt to investigate physical interactions of these minerals with different mycorrhizal fungi growing from plant hosts exposed to different atmospheric concentra-

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tions of CO₂. The data presented show decreased amounts of surface modification at low (200 ppm) levels of CO₂, corresponding to the minimum Cenozoic level, compared with the surface alteration seen at maximum CO₂ levels of 1500 ppm. Surface/physical alteration of minerals by fungi has been shown before in previous publications (Gazze et al 2012, Saccone et al. 2011, Bonneville et al 2009) but the comparison of treatments of different types of mycorrhizal symbiosis at different CO₂ levels provides novel information. Integration of these results into process based biotic weathering models suggests there may be a feedback regulation between reduced levels of CO₂ and reduced flux of calcium and magnesium from silicates. The ultimate conclusion is that trees and fungi may have had a stabilizing influence in stabilising CO₂ levels during the past 24 million years. The data provided, methodology used and statistical analysis carried out appear satisfactory. The lack of "direct confirmatory evidence" in previous studies is mentioned by the authors on several occasions. Some of the results provided here are also not "direct" in the sense that no direct connections between the plants and the supposed mycorrhizal fungi are shown. Only superficial morphological features are discussed and no DNA-based analysis of the fungi is provided. There is also no "direct" confirmation of the mycorrhizal status of the experimental plants used in this study. This could easily have been remedied by microscopic visualization/quantification of internal/external fungal structures associated with the root systems. Such demonstrations would greatly improve the robustness of the conclusions made and might even have supported the differences in hyphal length. However the comparisons between different systems, including controls with no plants, support the conclusion that plant-associated mycorrhizal fungi may be involved. Although the "background" values of non-mycorrhizal fungal hyphae are subtracted it would be interesting see how large (or small) these are. The paper is largely well written, clear to follow and appropriate references are provided. The text should be carefully revised to take account of the comments above and special attention should be paid to terminology used in different places. For example -it would be good to explain what is meant by "hyphal strands" in the context of AM fungi. Are these linear hyphal aggregates as

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formed by ectomycorrhizal fungi?

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