

Interactive comment on “Turning sunlight into stone: the oxalate-carbonate pathway in a tropical tree ecosystem” by G. Cailleau et al.

Anonymous Referee #2

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General Comments:

We usually associate carbonate pedogenesis with dry alkaline soils in arid and semi-arid environments. However, this very interesting paper shows that the oxalogenic iroko tree (*Milicia excelsa*, family Moraceae) has the potential to favor and promote carbonate precipitation in osixols in African tropical forests, an unexpected process in soils generally recognized as acidic. This surprising process is linked to the oxalate-carbonate pathway, which increases soil pH through oxalate oxidation.

In brief, the authors report that the iroko tree forms oxalate within its tissues, and this oxalate-rich plant tissue is delivered to the soil environment via litterfall and root turnover processes. Then, termites, bacteria, and fungi decompose the organic matrix of the litter and release oxalate into the soil environment, where it is subsequently

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mineralized by oxalotrophic bacteria. Bacterial oxidation of calcium oxalate permits formation of calcium bicarbonates and/or carbonates, resulting in increased soil pH.

Specific Comments:

The overall length of this paper is not justified by the amount of new data being presented. The authors present a few $\delta^{13}\text{C}$ values, two x-ray diffraction patterns, and some photographs, all of which are very good. However, it seems like the text that accompanies this relatively small amount of new data is excessive, and the paper could be made much briefer and more concise with some thoughtful editing. Just for example, Lines 2-17 on Page 1095 do not add much to the paper and could easily be deleted without compromising any of the key points in this paper. Similarly, Lines 21-25 on Page 1083 and Lines 1-19 on Page 1084 could also be deleted without compromising the paper at all. Many other passages throughout the manuscript could be deleted.

Having said that, I would like to see the authors provide some additional context regarding their study site in the Methods section. There is no information presented about the ecological context (e.g., biome, vegetation type, dominant species), the climate at the study sites (e.g., rainfall, mean annual temperature), or soil properties (e.g., taxonomy, texture, pH, organic C, total N). Without this information, the work has almost no context.

In addition, there is no clear indication of the sampling regime that was employed. How many plant and soil samples were taken? What was the size of the area that was sampled and how intensively was that area sampled? Is the sampling regime adequate to justify the length of the paper and the scope of the conclusions? In the final paragraph on Page 1086 and on all of Page 1087, it sounds like the isotopic measurements were made on samples collected from only a few different trees. While the results are admittedly interesting, it is difficult to generalize about this phenomenon based on data from what appears to be a rather limited data set.

This paper would also be strengthened if the authors could provide us with some in-

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dication of how important this oxalate-carbonate pathway might be in other locations around the world. Is this unique to forestlands? Does it only occur in forestlands in the Ivory Coast, or does this phenomenon likely have a broader geographic significance? Some indication of how common these processes might be in other ecosystems around the world would be helpful. Alternatively, if we currently have no idea how geographically important this process might be, that would be good to know, too.

In the Abstract (P. 1079, Line 2) and Conclusions (P. 1095, Line 26), the authors suggest that ecosystems dominated by iroko trees are carbon sinks because carbonate formation is favored there. However, I think this is much too strong of a conclusion based on the very limited information available. Yes, they have identified a process that appears to convert organic carbon to calcium carbonate; however, they do not provide any data regarding the rate at which this process might be occurring, or the magnitude of the stores in the soil derived from this process. Rates of conversion of oxalate to carbonate and the pool sizes of carbonate formed by this process may both be exceedingly low compared to other carbon cycle processes in this ecosystem. Furthermore, there are also many other carbon cycling processes occurring simultaneously in this ecosystem that remain unquantified, and which may actually result in this ecosystem being a net source of carbon to the atmosphere. So, without a more complete picture of the magnitude of the carbon fluxes and pools associated with this process, and the carbon budget of this ecosystem, it seems premature to suggest that this ecosystem dominated by iroko trees is a carbon sink simply because plant-derived oxalate carbon is converted to carbonate carbon in the soil.

There is a paper by L.A.J. Garvie (*American Mineralogist* 88: 1879-1888, 2003) looking at very similar phenomena occurring beneath saguaro cactus in the Sonoran Desert of western North America. This paper also employed stable carbon isotope ratios to trace some of the processes and support their reasoning. The authors should have a look at this paper as another point of comparison that might help to support and substantiate their own findings.

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In the Methods section (P. 1083, Line 18), the authors indicate that their $\delta^{13}\text{C}$ values are expressed relative to the PDB standard. However, many years ago the international stable isotope community agreed to express all $\delta^{13}\text{C}$ values relative to V-PDB. I encourage the authors to conform to this standard convention which is outlined briefly by Coplen et al. (*Analytical Chemistry* 2006, 78, 2439-2441).

Also in the Methods section (P. 1083, Line 14), the authors mention that ^{14}C measurements were made, but I did not notice any radiocarbon data in the tables or subsequent text. If no radiocarbon data is presented, then this reference to radiocarbon measurements should be removed from the Methods section.

In summary, this paper contains relatively little quantitative information about the processes involved in the oxalate-carbonate pathway in their study area. While their data are unique and very interesting, I do not think that they are substantive enough to justify the extent to which they elaborate and generalize about the significance of their findings. This is a great preliminary study which forms a nice basis and justification for now going out and acquiring the data required to evaluate their proposed model (Fig. 7) of the oxalate-carbonate pathway in this and other ecosystems.

Technical Comments:

P. 1078. Lines 19 and 21. The authors should replace the word “gents” with “agents”

P. 1081, Line 19. The authors should replace the word “binoculars” with “binocular microscopes” here and elsewhere throughout the manuscript.

P. 1082, Line 13. Change the word “centrifugated” to “centrifuged.”

P. 1082-1083. The verb “have been” should be replaced with the verb “were” throughout these two pages to place everything in the past tense.

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