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Biogeosciences Discussions

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

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Referee 2

The overall length of this paper is not justified by the amount of new data being presented. The authors present a few δ13C values, two x-ray diffraction patterns, and some photographs, all of which are very good. However, it seems like the text that ac- companies 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.

Authors' answer : We do not agree with this statement. In our opinion, and until now,

the carbonate accumulations, in the context of the oxalate carbonate pathway, were supposed to occur in a soil thanks to an oxalate provider (the oxalogenic iroko tree) and oxalotrophic bacteria present in the surrounding soil (Braissant et al 2004). The paragraph cited by the referee aims at emphasizing the fact that the system is more complex than previously thought, with other guilds of organisms (saprophytic fungi, termites, etc.) being heavily involved.

Similarly, Lines 21-25 on Page 1083

Authors' answer : we agree; P1083 Lines 21-25: deleted

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.

Authors' answer : We do not agree with this statement. Field observations are often disregarded in place of the state of the art and techniques, which often exclude the understanding of more general concepts such as ecosystems. The study of natural macroscopic systems should not be considered without a naturalistic approach such as the one presented here.

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.

Authors' answer : We agree.

To be added in P 1081 in section 3.1 At Biga the site, the mean annual temperature (MAT) is 24.5°C the mean annual rainfall (MAR) is 1500 mm/yr. 5 dry months occur in this semi-deciduous forest belonging to the Guinean domain. At the Kani site, the MAT is estimated as equal to the Biga site, MAR is 1300mm/yr and there is a 6-month dry period occurring in this gallery forest belonging to the Sudanian domain. In the Massangam area (including the Massangam, Machatoum, and Mankaré sites), the MAT is 23°C and the MAR is 1800 mm/yr. These sites take place in the gallery forest belonging to the Guinean domain. The studied soils mainly belong to the reference soil group Ferralsol (WRB 2007).

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 indication 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.

Authors' answer : Regarding the quantification and calculation of the C sink, the tree

density has been provided by a logging company (Société d'Exploitation Forestière du Noun, Cameroon), which works over large areas. 11 trees of interest (big enough) were found during our exploration phase. 3 other trees were studied in Ivory Coast. Statistically, soils under Iroko trees were found to be more alkaline than the surrounding soils. The presence of carbonate associated with the iroko has been documented in several African countries, such as in the former Belgian Congo i.e. the Democratic Republic of the Congo (Adriaens 1934, Plancquaert 1946), Ivory Coast (Carozzi 1967, our work), Uganda (Campbell and Fisher 1932, Harris 1932) and Cameroon (our work). Other trees in Africa seem to trigger the oxalate-carbonate pathway as well, but with a lower efficiency (unpublished data). On the other hand, recent field trips in South America as well as in India have revealed the presence of the oxalate-carbonate pathway around native trees (unpublished data). These occurrences appear sufficient to generalize our present conclusions.

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.

Authors' answer : As stated P1079 L19-20, this part of our work has already been published. For instance, 960 kg of pure C as calcium carbonate (c.a. 8 tons) were quantified around a 170+/-30yr old iroko tree (14C dating allowed the age approximation of 80 yr given in Cailleau et al., 2004 to be revised). In other words, on average (the system obviously does not work at the same rate throughout the tree's lifetime) around 5.76 kg of pure carbon are sequestered each year by the tree (the biomass is not taken into account). Calculation using the iroko distribution leads to the expectation of a potential lack of C sequestration into carbonate due to deforestation equal to 1.2x10-4 to 2.6x10-3 Pg/Cyr (see Cailleau's PhD thesis manuscript available at http://doc.rero.ch/record/5512?ln=en for more information). Moreover, if by "other carbon cycle processes" the referee refers to organic matter sequestration, the authors would like to emphasize the fact that carbon residence time for organic matter is far lower than carbonate in soil, and as a consequence less efficient. Moreover, forest soils in a climax environment cannot be considered as a carbon sink as the soil C pool is in equilibrium with sequestration (soil-litter admixture) and release (soil organic matter oxidation).

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.

Authors' answer : As the C is sequestrated using a Ca originating from a carbonate-free source each atom of C sequestrated by a Ca constitutes a C sink (Elbersen et al. 1999); it must be emphasized that the concept of C sink is, by definition, different when considering organic or mineral matter. This is due to the transient nature of the organic matter in soils. Except for particular systems (i.e. peat), it is unanimously accepted that the average C residence time will not exceed 100yr. This is not the case for pedogenic carbonate for which the concept of a C sink is defined by its Ca source (see for example Capo and Chadwick 1999; Chiquet et al., 1999; Dart et al., 2007). Obviously, the only limitation is the residence time for these carbonate accumulations.

This point remaining under debate, the authors refer to the available literature (see Retallack's numerous publications, e.g. Retallack 1990 Soils of the past : an introduction to paleopedology), which suggests a soil carbonate residence time up to 100'000 times longer compared to organic matter. As the calculation using the iroko distribution emphasizes the importance of the process described in our paper, this sink should be taken into account in the C cycle in African terrestrial environments. To conclude, as a NT (Near Threatened) conservation status is attributed to the iroko tree, this C sink could disappear within the range of the calculated sequestration of C given above.

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.

Authors' answer : We would like to define some major aspects missing in this paper and in Garvie et al. (2006) concerning the comparison to our work. We are presenting some aspects of the oxalate-carbonate pathway. In order to do so, we first needed to test for the presence of oxalotrophic bacteria. This aspect of the work was performed and published in Braissant et al. (2004). This point was not addressed in Garvie's papers. Concerning the C sequestration aspect, there is no mention about the Ca origin (there is a lot of carbonate in the studied desert soils so is the Ca present in the secondary calcite originating from the soil caliche? As the author speaks about C sequestration, this point is crucial. If the Ca that bonds to a C as secondary $CaCO_3$ originates from an inherited calcium carbonate, there is a concomitant C release, and so the balance is null concerning C sequestration. This is not our case (Cailleau et al. 2004) as there is no calcium carbonate input in our system. Moreover, concerning the C isotope data provided in Garvie (2003), the author clearly states that the carbonate (monohydrocalcite) precipitated in a closed environment (the decaying Saguaro cactus) with no contact to the soil gas atmosphere, which is totally different from the case presented here. Nevertheless, the authors agree that the saguaro system should present the same oxalotrophic process, as we were able to detect it around Opuntia in the Tabernas Desert (see Braissant and Cailleau's PhD thesis, manuscripts are available at http://doc.rero.ch/record/5513?ln=en and http://doc.rero.ch/record/5512?ln=en respectively).

In the Methods section (P. 1083, Line 18), the authors indicate that their δ13C values are expressed relative to the PDB standard. However, many years ago the international stable isotope community agreed to express all δ13C 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).

Authors' answer : our mistake.

L18: V-PDB replaces PDB

Also in the Methods section (P. 1083, Line 14), the authors mention that 14C 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.

Authors' answer :We propose to add the following sentences

P1083 L13 before Organic carbon. At the Machatoum site, it was possible, using a chainsaw, to sample a 49 cm long-blade of iroko wood representing a ray of the stem, which was later dated using 14C.

P1086 L20 before Six samples 14C dating for the Machatoum tree gives an age of

170 $\pm$ 30 yrs old. As the Machatoum and Biga trees have similar trunk diameters, we assume that their ages are in the same range.

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.

Authors' answer : We have working on the oxalate-carbonate pathway associated with iroko trees for the last 10 years. This study is the most recent of many in which can be found abundant data. Most of the referee's questions are answered in previous work that we cited abundantly in order for the reader to be able to put these new results in context, which now allow us to generalize their significance. .

Authors' answer : all the "technical comments" were positively answered in the answers to the first referee.

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