

Interactive comment on “Seasonality, drivers, and isotopic composition of soil CO₂ fluxes from tropical forests of the Congo Basin” by Simon Baumgartner et al.

Anonymous Referee #1

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The topic is very important because little is known about the carbon balance of the Congo basin forests. However, the manuscript fails at bringing this missing information because of an unsuitable measurement technique and a very poor number of replicates (3 chambers only were used). Overall, the sampling strategy is poorly described; the site also. In addition, the manuscript is poorly written, and the discussion related to the stable isotope is too much speculative.

Page 1 A scientific article is different to a competitive grant proposal: let's try to stay humble. Remove “enormous” (line 3), “for the first time” (line 4) Line 6: Respiration in montane forest soils => Soil respiration in a montane forest. To avoid confusion, use

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either soil respiration or soil CO₂ efflux but avoid mixing the two. Line 8-10: be more precise. What are the differences that lead to this suggestion? And this suggestion is quite speculative because you compare soil respiration and soil C, but soil respiration also includes root and rhizospheric respiration that are less connected with the isotope composition of the soil C. It was only a suggestion line 10 but it becomes a firm conclusion line 14. This is annoying.

Page 2 Line 4: Ruehr 2010 is not the correct citation. In addition, there are lots of much older papers to cite here line 5: "respiration of organic matter" has no meaning. Soil organic matter is not a living organism. Overall, the paragraph in lines 3 to 9 is poorly written and lacks logical structure line 12-13: What is the reason why a high flux of CO₂ and a high production would indicate a rapid turnover of C. Turnover (or mean residence time, the inverse) are flux divided by stock line 27: What do you mean by soil CO₂ consumption What is the link between an increase in air temperature (line 31) and the length of the dry season (line 34). To my knowledge, the dry season in the Congo is cooler than the rainy season. At least, it does not indicate change in temperature (line 35) Page 3 Isotopic signatures of leaf-litter, soil organic carbon, soil-respired CO₂ and dissolved stream water CO₂ are not enough to determine sources of soil respired CO₂. And what are the sinks? A bit wordy here. of soil respired CO₂. Information on the stand structure are missing (at least tree density and basal area), as well as dominant species. Fine root biomass would also bring valuable information for comparing the two sites Page 4 line 3-4: The duration of the measurement is therefore not three years in contrast to what is claimed in the abstract line 7: three chambers for one site! That is definitely not enough to cover spatial variability. If there is something that is well documented in tropical forests, this is the large spatial variability. line 10: Static chambers installed for 12 hours! Does the chamber remain in the same place for two years? Measurements last one hour. Were the chambers opened before and after? 1 hour is already very long. How much CO₂ accumulate during this time? Based on the keeling plots, concentration seems to vary from 500 to 2000ppm. It is far from the state of the art in terms of measurement of soil CO₂ efflux.

page 5 line 10: this equation was used by many before Imer 2013 Line 31: how many litter traps? Line 33: how many soil samples?

page 11, line 31: Universally???? Page 12, line 13-16: it's totally speculative. Root respiration may contribute differently in the type of forest. Nothing allows the authors to test their hypothesis that decomposition is faster and soil microbes carbon-limited.

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