

## ***Interactive comment on “Pyrogenic carbon from tropical savanna burning: production and stable isotope composition” by G. Saiz et al.***

**Anonymous Referee #2**

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The manuscript by Saiz et al. describes a series of experimental burns in Australia in which they measured the isotopic composition of pyrogenic carbon emitted and deposited after the fire. The authors show that relative to the isotopic composition of the carbon in the original fuels, the composition of pyrogenic carbon is more depleted, which is consistent with products from trees and shrubs ending up preferentially in this recalcitrant carbon pool. It is also more consistent with C4 grass biomass combusting more efficiently. Pyrogenic carbon emissions from fires (including savanna fires) are highly uncertain and poorly constrained by measurements. Hence, this study presents an exciting opportunity to look directly at the production and isotopic composition of pyrogenic carbon from savanna fires. Measuring fire-derived PyC is particularly challenging and this study presents an innovative approach, which is an important contribution to our capabilities of directly measuring PyC from savanna fires. While the

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study address issues that are of great importance for the field, hence contributing to our knowledge and understanding of PyC production and composition during savanna fires, there are a couple of aspects of the methods and results that are not very clearly explained in the current version of the manuscript. Overall the introduction is comprehensive, well balanced and provides a clear motivation for the study in terms of implications for the carbon cycle, interpretation of the origin of soil carbon pools using isotopes, and aerosols. The methods look to be state-of-the-art and are described in a robust way. The methods and results are logically presented, although as described below, some additional information would be helpful to the reader. In the discussion, the size of the impact of the isotopic disequilibrium presented by the authors here is likely to be an upper bound because some of the depleted pyrogenic carbon would be expected to decompose in subsequent years, lowering the instantaneous effect measured immediately after fire. Over all the paper presents new observations collected in a careful and quantitative manner that are relevant for fields of paleoecology and studies of atmospheric composition. In this context, the paper is likely to be relevant and of interest to readers of Biogeosciences. In the opinion of this reviewer, the paper may be suited for publication after consideration of the comments below.

Specific comments:

The authors may wish to provide more motivation for isolating a HyPyC component of the pyrogenic carbon and description of their approach for measuring HyPyC. While the difference between the HyPyC and PyC pools is based on the chemical definition, the implications and meaning of this distinction are not very clear. In the results section 3.1. (Production of PyC during savanna burning) – PyC is only mentioned once, whereas the rest of the text is focused on HyPyC. When the SIDE effects are discussed though, the authors note that the significance of SIDE during burning is more pronounced when considering the PyC produced by the fires than when considering the HyPyC component alone. What mechanisms contributing to this difference? Another aspect discussed in the paper is the partitioning of PyC and HyPyC into distal and

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proximal fluxes, based on particles size separation of greater (distal flux) and smaller (proximal flux) than 125 micrometers. More information on why 125-micrometer threshold is used as the separation point between the two size fractions is needed. The authors conclude the manuscripts by discussing the implications of this study for the  $^{13}\text{C}$  composition and interpretation of soil organic carbon (SOC). However, the concept and importance of SOC and its isotopic composition are barely mentioned in the rest of the paper – one sentence in the abstract and one in the introduction. The SOC topic needs further emphasis, and it is more suitable to move this section to the discussion rather than the conclusions.

Figure 1 is complicated and includes 5 panels and a table. It would be easier for the reader if the lat/lon information and site information were contained in a separate stand-alone table. This would also give the authors an opportunity to write out the full site names for all the sites, some of which are not provided in the text. It would also allow the reader to more easily see the pictures of the individual sites, which are now relatively small. It would also be helpful if the map of the study area is bigger with more clearly defined vegetation cover (maybe overlay vegetation cover layer on top of the image of Northern Australia). In the calculations and modeling, why assume mean isotope values from the literature for  $\text{C}_4$  grass biomass? Why not use  $\text{C}_4$  grass values measured from the individual field sites? How robust is the SIDE effect computed in Figure 5d as a function of grass biomass with respect to these assumed values? In the fire carbon cycle literature, combustion completeness (the amount of fuels consumed relative to their starting abundance) is used as a measure to analyze emission factors and other processes. The authors may wish to add a sentence or two about these results to the first paragraph of the results, and perhaps add a column for it to Table 1. The hypothesis that biomass from  $\text{C}_3$  vegetation is preferentially integrated into the pyrogenic fraction, contributing the observed disequilibria is an interesting one. Is it contradicted by the individual site results from MIT grassland sites that also show a strong disequilibrium effect, yet from the site pictures, no trees or shrubs appear to be influencing the composition for the grassland sites at this location? For the mass

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balance  $\delta^{13}\text{C}$  estimate, please clarify it is really the isotopic composition of  $\text{CO}_2$  and other trace gases ( $\text{CO}$ ,  $\text{CH}_4$ , etc) which may add up to 3-5% or more of the total combusted loss. Page 15166. The estimate of the global disequilibrium from pyrogenic fractionation (0.75 Pg C per mil) is really an upper bound, in the opinion of this reviewer, because it does not take into decomposition of some or most of the depleted PyC material in the months and years after fire. This has to be a lot, because the pre-fire HyPyC is less than half the post-fire HyPyC on average from Table 1, the FRT is  $\sim 2$  years, and it's difficult to imagine erosion is a dominant pathway at these relatively flat and dry sites.

Minor comments:

Page 15158: Line 15 – Define TOC – acronym is used for the first time. Page 15159: Line 3-4 – The authors mentioned they weighted samples pre- and post-drying to determine fuel moisture. It would be interesting to know the results, whether or not the moisture was high/low/variable, and if it could have any possible effects on the PyC production. There is a typo or mis-formed sentence on page 15159 between lines 20 and 25 “evaporator remove the liquid” Page 15160 : Line 7 – Write out Mo – molybdenum is used for the first time. Section 4.3. Page 15165 Line 18-19: “Values consistent with relative  $^{13}\text{C}$  depletion of PyC in savanna fires” – citations? General comment - please consider including SIDE values in Table 2

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