Reviewer 1
The manuscript reports the results of experimental burns over a rainfall transect of savanna where C4 grass represented from 35% to 99% of the standing biomass. The article reports and discuss results about the influence of standing biomass on fire residence time, production of HyPyC, and effect on the isotope disequilibrium induced by the combustion processes (SIDE). The authors found a negative correlation with the production of recalcitrant PyC (HyPyC) and a 13C in PyC compared to parent material. The hypotheses tested are extremely relevant both for the modeling of the impact of fires on C cycle and for reconstruction of past fire regimes. The experimental design is very innovative. The hypotheses tested were clearly stated in the introduction. There- fore I suggest the publication of this article in biogeoscience after some minor revision.

…Specific comments

Our comments are in bold
Line 8-9: please specify the meaning of distal and proximate fluxes in this context (is clarified in the text, but it may worth a clarification also in the abstract).

We have now introduced this clarification in the abstract. The text now reads: '…with each of these fluxes also partitioned into proximal (>125μm) likely to remain close to the site of burning, and distal (<125μm) likely to be transported from the site of burning.'

Line 12: 17: I completely with the authors on the wide range of residence time estimates. Nonetheless the authors report that: "some components of PyC appear to be susceptible to degradation on comparatively short timescales (Bird et al., 1999; Zimmermann et al., 2012) while some are resistant to degradation, remaining in soils and sediments for thousands to millions of years (Cope and Chaloner, 1980; Lehmann et al., 2008; Masiello and Druffel, 1998)." I am not sure whether this difference is due to the presence of components having different decomposition rates or rather can be attributed to the different methodologies adopted to estimate PyC decomposition (incubation vs observation).

While the use of different techniques does indeed lead to different results, it has become quite apparent over the years that the PyC continuum is also associated to a PyC degradation continuum (e.g. Kanaly and Harayama, 2000; McBeath and Smernick, 2009; Zimmermann et al., 2012).
In the text we include the statement above and make reference to our latest work, which is a compilation of the Pyrogenic Carbon Cycle addressing these very questions (Bird et al., 2015).

Bird, M. I., Wynn, J. G., Saiz G., Wurster C. M., and McBeath A.: The Pyrogenic Carbon Cycle, Annual Review of Earth and Planetary Sciences, 43, 2015.
Kanaly, R. A., & Harayama, S.: Biodegradation of high-molecular-weight polycyclic aromatic hydrocarbons by bacteria. Journal of bacteriology, 182, 2059-2067, 2000.
McBeath, A. V. and Smernik, R. J.: Variation in the degree of aromatic condensation of chars. Organic Geochemistry, 40, 1161-1168 2009.

Zimmermann, M., Bird, M. I., Wurster, C. M., Saiz, G., Goodrick, I., Barta, J., Capek, P., Santruckova, H., and Smernik, R.: Rapid degradation of pyrogenic carbon, Global Change Biology 18, 3306-3316, 2012

Line 16-23: are these Kuhlbush (1996) and Masiello (1998) the most up to date articles on this topic?

We feel that the article by Kuhlbusch et al. (1996) needs to be cited in this context, as it is a seminal field-based experiment that has directly determined the initial allocation of PyC produced immediately after savanna burning. Moreover, and as indicated above, we also make reference to the most updated work covering these topics (Bird et al. 2015).

Page 15556: Line 10: In general were also reported the effect of being in a Tree (T) location or near a Grass (G location)?

The purpose of covering Tree (T) and Grass (G) locations was to encompass the widest possible range of spatial biomass (fuel) heterogeneity, which, as explained in another comment below, would minimise autocorrelation issues. Beyond the expected grass/woody biomass ratios (expressed in % grass biomass in newly named Table 2) it is unfortunately not possible to draw any sensible conclusions about their influence on PyC production, as they would just be based on two single observations per site.

Page 15159: The soot retrieved after cleaning the structure was it added to the >125 pool or < 125 or was it not measured? As it is specified later this is classified as distal, I suggest the authors report this also here for clarity. Also I would appreciate if they could discuss at which distance it is likely that fine particles are transported.

We have added the expression: 'This fraction was subsequently added to the distal (<125 μm) pool'. In addition, section 4.2 now contains a statement about the transport of fine PyC particles.

Page 15160: Line 16: Was all the non-HyPyC-C remaining after fire considered PyC? Was the hypotheses that part of the TEC was not altered by combustion discarded? I think that this is just a terminology issue, but it could worth to stress it in the text.

In the last paragraph of the introduction we acknowledge that there could still be some material non-thermally affected after the burns. The text reads: 'Here, we use the term PyC to describe all post-fire carbon, which in our experiments might also include non-thermally altered material'.
Coarse woody debris (CWD) was not considered due the small-scale experimental set-up. However, all the chosen locations and quadrats harvested for biomass quantification were studied as originally found, with

every residue left after the fire being accounted for (regardless of their degree of combustion).

Page 15161: Line 12-16: I did not find very clear why two different methods were used. Basically you estimated first the real bulk 13C content by measuring it (and I assume was somewhere in between -13 and -27) and then you measured the theoretical bulk 13C based on a two pool mixing model? Did the two measures agreed?

This is correct—the reviewer has understood the two methods that were used. We added a statement as to why this was done, and noted that the methods generally agreed.

Line 24-26: It is not clear to me against what the SIDE was regressed, i.e. what was the explanatory variable of the SIDE? From figure 4 it looks like they were regressed against grass biomass. I suggest the authors report it in the text as well.

The explanatory variable is now described in the text.

Line 27: of an F test, I would substitute with: "with an F test". Did the authors also tested the normality and constance of errors using the power model?

The text is now replaced as suggested by the reviewer. The normality of residuals was tested with Jarque-Bera test, and stats are reported in the figure.

Page 15162: This measure of fire residence time, is very interesting, it could worth to describe it in more in depth in the material section if it was created ad-hoc by the authors or cite the works if previously adopted, why was 100◦C chosen?

We have incorporated a brief description of this measure of fire residence time in M&M. We chose this temperature threshold on the basis of anecdotal evidence gathered during previous experimental fires conducted at these very ecosystems (Saiz et al, 2014). We observed a close parallelism between temperature and smoke emissions, which virtually ceased when temperature dropped round about 100 degrees.

Line 22: as it is reported in the figure the decrease is significant, I suggest the authors report it also in the text.

We have also added the significance of this relationship in the text.

Line 15: Why was the median used as a measure of central tendency instead of the mean? Were there many outliers?

With such a small sample size (n=16), it is difficult to say whether the distribution is normal. To be conservative, we used the median.

Figure 4: Since ”The 13C of the CO2 was calculated by mass balance using the amount and isotopic composition of initial biomass and and the residual product of combustion”, I was surprised that the uncertainty on the 13C of the CO2 was so little in fact this results from the sum of other measures (each having its own uncertainty usually higher than the one of CO2), therefore I would expect that the uncertainty would increase.

This is indeed a bit surprising. We note however, that the uncertainty was not calculated by propagating errors on the other estimates, but rather by statistics of the distribution of d13C of CO2.

Pag 15164: So if I understood correctly in nature high TCE correspond to short residence time fires, while you observed the opposite. Could this be an artifact of the chamber you installed?

We actually think that, under comparable environmental conditions, large biomass fires are likely to result in longer fire residence times compared to low (but continuous) biomass sites, if only because there is more material to be burnt. Moreover, in our experiment sites with the larger TCE contained a larger presence of woody biomass (Table 1), which would normally combust slower than the characteristically finer grassy biomass.

Pag 15165: Allocation of HyPyC produced during savanna fires: could the author discuss a bit deeper the mechanisms that regulate the relation between transport and residence of PyC in soil?

The discussion section (4.2) contains now a statement about the potential causes that may influence the transport and residence of newly produced PyC particles.

Page 15180: Figure 3: Was it taken into account that the part of the points where spatially correlated ? I mean that the points coming from the same area are likely to be spatially correlated, and could therefore be considered pseudoreplicates. This inconvenient could be solved by either using multiple variance anova, or mixed effect models. I think that r squared are not ideal to describe the fit of non-linear model (http://www.ncbi.nlm.nih.gov/pmc/articles/PMC2892436/)

We took the effort of spreading the location of the experimental fires across the natural ecosystems to minimise this effect. Furthermore, we point out in the text that, in tropical savannas, the abundance and $\delta^{13}C$ values of litter and standing biomass are quite heterogeneous at a local scale. This heterogeneity is largely controlled by the distribution of trees, and consequently we conducted duplicate burns at locations at half crown distance from trees ('Tree'; T locations), and at two additional locations remote from trees ('Grass'; G locations). Taking these factors into account, we were reasonably confident that we had likely encompassed the widest possible range of spatial heterogeneity, thus minimising autocorrelation issues.

We thank the reviewer for pointing out this common bias. We have now supplemented the degree of fitness (r^2) of the calculated equations with a Bayesian Information Criterion (BIC) as recommended by the suggested publication, so the reader can have more balanced information about how much these models could compare with others.