Reply to RC1 by Authors

Interactive comment on **"Plant controls on post-fire nitrogen availability in a pine savanna"**

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Anonymous Referee #1

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General comments

The study presented in this paper investigates soil nitrogen availability in longleaf pine savanna after prescribed burning. This piece of work fits within the scope of Biogeosciences as it studies nitrogen (N) pool sizes and transformations in an open canopy savanna-like ecosystem. The paper presents a novel dataset of weekly data points spanning across a pre- and post-fire period of nine weeks. The description of the study site and scientific methods applied are clearly articulated.

> **Thank you for the time you have put into reviewing this manuscript. We appreciate your thoughtful comments and look forward to an active discussion. We address your individual comments and suggestions below. Our responses to your comments have been indented and bolded; text added to the manuscript has been italicized here for visual clarity.**

The authors could improve model description for pool sizes and cycling rates by including more details. The paper is well written with a logic structure and concisely summarised in the abstract.

> **The model description will be improved based on your specific comments. See below for details.**

However, in my opinion, the results do not sufficiently support the interpretations in the discussion, as the chosen setup of study sites does not seem adequate. Firstly, the aim of this study was to present data about the effects of prescribed fire on soil N dynamics; yet, one of the three treatment sites (B2) was affected by wildfire and had a shortened fire return interval compared to the other two sites. Secondly, the two sites affected by prescribed fire had very different responses to fire in terms of vegetation re- sprouting and different standing biomass stocks prior to fire. While the authors related the differences in magnitude of the mineral N pulse to these site differences the number of independent sites (N=2, with three replicate soil cores per site and week) seems too small to support the overall conclusion proposed in the paper - that plant uptake regulates post-fire N availability; especially given the high variance within site pre- and post-burn data and between sites.

We believe there are two important points to consider here. The first relates to the sample size and the effects of prescribed vs wildfire. In this system, prescribed fires are employed to mimic historically naturally occurring wildfires, which occurred on a 1-3 fire return interval (Frost 1998; mean fire return interval = 2.2 years; Stambaugh et al 2011). As such, the wildfire that unexpectedly occurred, did so within the range of both wild- and prescribed-fire intervals observed in this system. After 40 years of either 2- or 3-year fire intervals, there was no statistically significant difference reported in the % cover of any understory plant group (tree seedlings, shrubs, vines, graminoids, forbs, or ferns and mosses; Brockway & Lewis 1997). Consequently, we do not anticipate substantial differences in biomass accumulation after one year of a shorted fire return interval, and biomass is a strong determinant of fire intensity, and we will amend the next draft of the manuscript to reflect this information as follows (page 5 lines 21-27):

The site that burned prematurely due to a wildfire was grouped with other burned sites, despite its shortened fire return interval (one year) relative to the other burned sites (three years). Previous work has found no significant difference in vegetation cover after 40 years of management with either a 2- or 3-year burn interval (Brockway and Lewis, 1997); because biomass is a strong determinant of fire intensity, we did not anticipate that a site experiencing a shortened burn regime for one year would have substantial effects on fire dynamics.

Nevertheless, N=3 sites remains a small sample size and certainly limits our ability to draw conclusions across ecosystems. We will be cautious about the strength of our claims in the next draft of this manuscript. If there are particular sentences in which you feel we have stretched the applicability of these findings, we would appreciate your direction to them.

Secondly, we agree that it is premature to draw conclusions that plant uptake is solely responsible for post-fire N availability dynamics. Rather than claiming that our data conclude this, we believe our data disprove two alternative hypotheses- an increase in microbial processing and ash deposition- that could independently account for the observed patterns. Instead, we hypothesise that the role of plant N uptake is another factor that should be explicitly considered in future studies. We believe we have been appropriately cautious as to the strength of our arguments and conclusions. For example, we acknowledge limitations in our ^{15}N analyses on page 18

lines 11-15:

Given the uncertainties surrounding the redistribution of surface inputs down the soil profile, we cannot conclusively rule out the potential to surface additions to contribute to the observed NH_4^+ pulse. Nevertheless, considering the unrealistic mass of ash-N needed to be deposited onto surface soils to account for our measured shifts in $\delta^{15}N$, we conclude that ash inputs are unlikely to fully account for the increase in measured soil inorganic N availability.

And on page 18, lines 22-28, we acknowledge that it would be inappropriate to conclude that post-fire increases in NH_4^+ is solely driven by changes in plant sink strength:

If fire damage temporarily halted or slowed the plant uptake of inorganic N, we would expect to see an accumulation of soil N if microbial immobilization did not increase sufficiently to deplete the pool. However, N accumulating in excess of demand can only partly explain observed increases in inorganic N availability, since the pulse of N we detected following fire was many times greater than what was produced by net mineralization and net nitrification. Nevertheless, a change in plant sink strength may have contributed to post-fire NH_4^+ pulse.

Brockway, DG and CE Lewis. 1997. *Long-term effects of dormant-season prescribed fire on plant community diversity, structure and productivity in a longleaf pine wiregrass ecosystem*. Forest Ecology & Management (96): 167-183.

Frost, C.C. (1998). Presettlement fire frequency regimes of the United States: A first approximation. Proceedings 20th Tall Timbers Fire Ecology Conference: Fire in Ecosystem Management: Shifting the Paradigm from Suppression to Prescription, Boise, ID (eds T.L. Pruden & L.A. Brennan), pp. 70–81. Tall Timbers Research, Inc.,Tallahassee, FL.

Stambaugh, MC, RP Guyette, and JM Marschall. 2011. *Longleaf pine (*Pinus palustris *Mill.) fire scars reveal new details of frequent fire regime*. J. Veg. Science (22): 1094-1104.

Specific comments

The authors may consider revising Figure 1 as the schematic illustration of the paired- core sampling design is not readily understood. For example, it is unclear what the single circle below week 9 represents, is it the last sample for the measurement of pool size? It might be better to depict paired-soil cores for all nine weeks or omit the figure altogether as the sampling design is sufficiently explained in section 2.2.

The circle (core) below week 9 was meant to indicate that the incubating core from

the final week's set was collected one week 10, since it was installed on week 9 and incubated for a week. This is consistent with how the other core sets were treated (the dashed circle depicts the core collection timing). If the text explains the sampling protocol sufficiently, we will omit this figure from the next draft.

Authorities for plant species names should be included when species are mentioned for the first time.

Authorities for plant species have been added.

In the methods section, the description of the Bayesian hierarchical models would benefit from including more details, specifically: -

Site effects (intercepts for B1-B3) should be reported –

We have included significant site effects in the text (see below for example). For visual clarity (because site effects were substantial greater than environmental effects), we have omitted them from figures. This omission note is also added to figure legends.

Page 11 lines 22-26: ***Site effects (β_0) had the strongest overall effect on NH_4^+ pool sizes, although this effect was not significant at C1. In burned sites B1-3, β_0 was -26.9 (95% credible interval (CI) = -46.11– -6.57), -22.03 (95% CI = -38.91– -4.78), and -24.16 (95% CI = -41.78– -6.12), respectively. At C2, β_0 was -23.23 (95% CI = -40.38–-5.69).***

Page 12 lines 8-9: ***Site effects on NO_3^- pool sizes much weaker than for NH_4^+ and were only significant at B1 (β_0 = -4.81, 95% CI = -9.46 – -0.08).***

Did the authors standardise the coefficients? –

Coefficients were not standardised prior to analysis.

What is the underlying distribution for β0i,j? –

β 0i,j has a normal underlying distribution. This information has been added to the text on line 13-14 (page 10): ***All predictors, including random site site effects, were modelled with normally distributed, uninformative priors.***

Should the formula in 3b have a minus before β6iy0i,j as the initial concentration is subtracted from the incubated concentration? –

The effects of initial concentrations are reflected in the negative posterior estimates of S_0 (Fig 8).

Using the rjags package, how many chains and iterations were run?

Three chains were run with 200,000 iterations after a 100,000-iteration burn-in period. This information will be included in the Methods section (page 10, lines 14-15).

How was convergence tested? –

Convergence was tested by examining chain density and trace plots to ensure proper chain mixing, and by calculating the Gelman-Rubin diagnostics using the gelman.diag() function in the coda package to ensure the scale reduction factors for each predictor was <1.05. This information will be added to the methods section.

Does σ ~ unif(0,100) relate to both models or just the cycling rates model?

This relates to both models and will be included in the set of equations describing the pool size model as well.

On page 15 (line 19), please state how soon following fire vegetation re-sprouted in sites B1 and B2.

The information was amended as follow: ***Page 15 (line 24) "While B1 and B2 exhibited rapid vegetation resprouting following fire, regrowth in B3 was patchy. Vegetation began resprouting in B1 and B2 six days after fire, but not until 18 days after fire in B3."***

On page 18 (line 4), should it read "...and sharp increases in soil temperature with depth. . ." instead of decline?

Yes, it should read "increases", and we changed the word. Thank you for catching this.

In the discussion on page 18 (line 27) authors refer to the preference of plants in pine savanna for uptake of ammonium. It would be good to include a reference confirming this statement about uptake preference in this ecosystem as the authors argue that plant preference for ammonium uptake could explain the relatively large nitrate pool sizes relative to ammonium.

As far as we know, there are no studies explicitly documenting the nitrate vs

ammonium preference for species inhabiting pine savannas, but preference for one N form is likely the result of multiple drivers, including enhanced uptake of the dominant N source (e.g. Kronzucker et al 1997; Houlton et al 2007; Wang and Mack 2011). Because nitrification rates are low at low pH, acidic soils often have greater ammonium availability than nitrate. If the availability of each nitrogen form is one component of preference, we expected that plants inhabiting the acidic soils of our study system to take up relatively more ammonium than nitrate. Here, we draw an analogy between seasonal patterns of N availability in longleaf pine savanna (high soil ammonium and low nitrate during the winter, but low ammonium and high nitrate during the growing season; Christensen 1977), and seasonal patterns of N concentrations in northeastern US streams (winter maxima when terrestrial plant N uptake is low; Vitousek 1977). To clarify this, we added references explaining the drivers of plant N uptake patterns and resulting environmental availability, and amended the manuscript as follows.

Preference for NH_4^+ by plants inhabiting acidic soils, where nitrification is limited by low pH and NO_3^- availability is consequently low (Ste-Marie and Paré, 1999;Houlton et al., 2007;Wang and Macko, 2011;Kronzucker et al., 1997), could help to explain the relatively large pool sizes of NO_3^- relative to NH_4^+ during the growing season (Vitousek, 1977), and this pattern is consistent with previous seasonal trends in a longleaf pine savannas (Christensen, 1977).

Temperature is an important influential factor on N transformation processes (MIN, NIT) and soil temperatures might change after fire due to the blackened surface promoting increased heat absorption. The authors could discuss whether they consider soil temperature to have an effect on N transformation processes in the context of their study.

Indeed, temperature is an influential factor for N transformations. To reflect this, we added the following sentence on page 16 (line 13-14), and included two citations for readers interested in learning more. We limited the discussion of this driver, however, since we did not observe increases in cycling rates following fire.

Soil surface blackening after fire may increase soil temperature and stimulate immediate and prolonged N transformations after fire (Booth et al., 2005;Ojima et al., 1994).

Technical corrections Page 5, line 17: delete 'in'. Page 9, line 1: correct the word 'through'. Page 10, line 23: correct reference to figures to '3-4' instead of '2-3'. Page 11, line 7: correct figure number in brackets to Figure 5. Page 17, line 9: delete first 'ash' in sentence.

These technical corrections have been made, and we appreciate your careful review of our manuscript.