

Reply to RC2 by Authors

Interactive comment on “Plant controls on post-fire nitrogen availability in a pine savanna” by Cari D. Ficken and Justin P. Wright

Anonymous Referee #2

Received and published: 9 October 2016

General Comments: The main objective of this research was to assess potential mechanisms responsible for post-fire variations in nitrogen (N) availability and pools in a longleaf pine savanna. The manuscript provides weekly measurements of N over a 9- week period (several weeks pre- and post-fire) during the growing season at five sites at Ft Bragg, NC. This allowed detection of rapid but ephemeral pulses in N availability post-fire. The subject matter appears appropriate for Biogeosciences. The paper is relatively well-written and thought out. However, background information provided in Introduction needs additional citation (e.g., Pg 3 lines 17-18, 21-22; Pg 4 lines 1-2, 6, 7-9, 9-11), and there is a lack of detail in the Methodology (see specific comments below). As written, the sampling design appears a bit weak, and the choice of sites and inclusion of the wildfire site and exclusion an outlier control site need more justification. The use of the Bayesian model to account for pre-fire heterogeneity of N values appear to be a useful technique. The authors claim that decreased vegetation cover post-fire could be a major factor driving increases in NH₄⁺ availability, but no vegetation data are provided, although these data apparently exist (cited in Discussion as ‘unpublished data’). Inclusion of vegetation data (at least in some form) would substantially strengthen the paper.

Thank you for taking the time to review our manuscript. We address your individual comments below. Our responses to your suggestions are bolded and indented for visual clarity.

Specific comments. Pg 2, Line 24: This phrase is confusing and needs clarification: “localized studies with ecosystem-specific temporal data resolution”

This has been changed to “This suggests a need for localized studies with temporal data resolution appropriate for that ecosystem to evaluate the mechanism behind changes in soil N availability following fire.”

Pg 5, line 6: Please provide examples of the Ericaceous species

This has been amended to read “...dominated by Ericaceous species, including *Vaccinium formosum* Andrews, *V. fuscatum* Aiton, *V. tenellum* Aiton, *Lyonia lucida* (Lam.) K. Koch, *L. mariana* (L.) D. Don, and *Gaylussacia frondosa* (L.) Torr. and Gray ex. Torr. ...”

Pg 5, lines 10-15: It would be useful to know more about the burn regime of the study sites, especially since one of the fires was a wildfire. When are prescribed fires normally

set? At the same time as the wild-fire? Was the wildfire similar in severity and timing as the prescribed fires?

We did not measure burn temperature in the wildfire site (B2, see p10 lines 8-11), but observationally, the burn in B2 was comparable to prescribed burns in B1 and B3. See p10 lines 13-16.

On P5 lines 11 we added “Prescribed burns have occurred primarily during the growing seasons, when wildfires also occur. To keep the prescribed burns under control, they are performed as low-intensity backing fires (ignited along a road or other fire break and allowed to burn into the wind).”

Pg 5, lines 15-19: It’s unclear what is meant by “historical burn characteristics.” The information in parentheses makes it seem like this means that all the sites burned under similar conditions, but the last part of the sentence makes it seem like only the return interval was the same.

Prescribed fires of Sites on a 3-year burn rotation are staggered such that not every site burns on the same year (for example, some sites burn on years 1,4,7,10; other sites burn on years 2,5,8,11). We wanted to target sites that were on the same burn rotation and that burned in the same years. To clarify this, we added “the same” on page 5 lines 17-18 as follow: “...we limited the number of burned sites to those with similar burn histories (i.e. all on the same 3-year burn rotation). ...”

Pg 5, line 20: The grouping of the wildfire with other fire sites needs more justification. The authors go to great length to explain in previous sentences why the three ‘burned’ sites were chosen based on their similarities and then seem to gloss over the grouping of this wildfire with other fire sites despite the fact that it burned at a totally different burn interval and likely under very different conditions.

Indeed the wildfire burned out of rotation, however the climatic and weather conditions at the time of all burns was similar (ie all fires occurred in a 5 day period in July). 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. Additionally, 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 shortened fire return interval, and biomass is a strong determinant of fire intensity. We will amend the next draft of the manuscript to reflect this information (page 5 lines 21-27).

Pg 5, line 22-24: The removal of the third control site from further analyses is questionable. Were these differences among control sites unknown prior to sampling, or were they only

discovered after sampling? Is there nothing that can be gleaned from information on this site? How representative of the area is this site?

There was some indication prior to sampling that the third control site differed from the other sites. For example, it had cinnamon fern (*Osmunda cinnamomea* L.), a species that generally grows in wetter areas, growing near our sampling area, while neither the other control nor treatment sites had this species. However, it was not until we examined data on soil nitrogen content that we realized how different this site was from the others. For example, its nitrogen content was at least one order of magnitude greater than all other sites, and it had substantially greater soil moisture than the other sites. This site is not necessarily anomalous of longleaf pine forests- the area is underlain by an undulating water table which gives rise to drastic micro-topographical gradients and high floristic diversity. However, because it had such substantially different initial soil conditions than the remainder of our sites, we excluded it from analyses. We suspect differences in the depth of the water table (specifically, it may be closer to the surface) between the site we removed from analysis and our remaining sites may be a cause of such differences in vegetation cover.

Pg 5, line 23: Five sites is pretty small sample size, especially since the wildfire site might not represent the prescribed fire sites. If the sites are not considered replicates, and instead the cores within a site are the replicates, this should be clarified.

Sites are considered replicates.

Pg 5, line 26: How big was the sampling area?

The sampling area was 1 m². This information will be added to the next draft of the manuscript.

Pg 5, line 27-28: The description of sampling above the 'ecotone' needs clarification. Was this just to avoid being in the 'extremes' of either upland or lowland?

Indeed this was to avoid sampling either in the riparian wetland areas or the very dry upland areas. We state "This topographic location was chosen to minimize the effects of extremely well-drained, hydrologically disconnected (as found in the uplands) or saturated, anoxic (as found in the wetlands) soils on microbial processing. "

Pg 6, lines 1-2: The vegetation sampling needs to be described in more detail. Exactly how was this done? Given that the vegetation link to N availability is a big part of this study's conclusion, why aren't these data included or at least described in more detail?

We collected vegetation cover data at the onset of the experiment, prior to any fires, to help ensure that our sites were appropriate replicates. We clarify this as follows: "At the onset of the experiment, all vegetation within sampling plots was identified

to species and the percent cover was estimated.” However, we did not anticipate that vegetation regrowth could play a role in structuring post-fire nitrogen availability. Instead, this hypothesis arose as our data did not support other explanatory hypotheses. As a consequence, we did not track vegetation recovery following fire for this study.

Pg 6, line 3: This sentence should be moved to where the fire regimes are being described.

Because the complexities of the fire regime at each site necessitated substantial discussion in advance of actually listing the sites identifiers along with their burn dates, we found it difficult to relocate this sentence earlier in the manuscript. We chose not to move this information so that we could concisely provide the burn dates with the individual sites for all the burned sites we analyzed.

Pg 6, lines 4-7: What temperatures were recorded? Where in the burns were these pyrometers installed? How far apart from each other? How high off the ground?

Burn temperatures are reported in the results (page 11 lines 8-11). We amended the methods section to include to include information on the tag layout within the site.

Pg 6, lines 6-7: Are there other surrogates of fire intensity that could be used to assess this wildfire site? Canopy mortality? Depth of residual organic layer? Char cover?

These prescribed burns are low intensity and do not reach into the canopy. We described observational metrics of fire intensity in the results on page 10 lines 13-17.

Pg 6, line 13: How far apart were cores? “Adjacent to each other” is vague.

We collected cores within approximately 10 cm from each other, but we avoid sampling large roots. We will add this information to the manuscript.

Pg 6, line 19: “throughout the growing season” makes it seem like samples were collected over a much longer period. Longleaf probably grow for many weeks (much more than 9 weeks) in North Carolina.

We replaced “throughout” with “during”.

Pg 6, line 21: “until they were analyzed” is vague. How long were soils typically stored frozen before analysis?

Soil cores were stored on ice while in the field and were moved to 4C within the same day. Cores were never frozen. We analyzed soils for nitrogen content within 48 hours of collection, as stated on page 6 line 25. Other analyses which are not time sensitive were analyzed in the fall.

Pg 7, line 21: What is the date referring to?

Thank you for catching this. This was an error on the part of our citation managing software. The correct citation for the monthly precipitation values has been added.

Pg 8, line 24: What is SOM? Soil organic matter? From the description in previous sections, no organic layer develops in this system. Please clarify.

An organic litter layer (ie an O horizon) does not develop, but the soil still has some (albeit very little) organic matter. We added a definition for SOM (soil organic matter) at its first mention (after revisions, p9 line 8).

Pg 10, line 11-12: The vegetation description should be expanded to show how the communities varied across sites. What functional types are the three species listed? Grasses, forbs, shrubs?

We added the functional types of the listed species.

Pg 10, line 17-18: This sentence seems out of place since no description of fuel load or moisture across the sites is given in this manuscript.

This information will be omitted.

Pg 14, line 16-17: The link to plant communities would be strengthened if there were more detailed plant data included in the study. As written, there's no way to assess whether N availability co-varied with plant abundance. Apparently these data exist (Pg 15, lines 18 and 21), so why aren't they included?

Unfortunately, we do not have data on vegetation regrowth patterns following fire that can be incorporated in this manuscript. The data that are cited as unpublished corresponded to data collected for other ongoing studies. That plant uptake might help to explain post-fire patterns of N availability arose because two other explanatory mechanisms failed to account for observed changes. As such, we present this as a hypothesis that can and should be tested. We stress that this study was not designed as a test of the hypothesis. We hope that we have been appropriately cautious about the strength of our arguments and conclusions (for example, we acknowledge limitations in our ¹⁵N analyses on page 18 lines 11-15); our aim was not to claim that our data conclude that plant uptake is solely responsible for post-fire N patterns, but, based on our results, to hypothesize that plant uptake may be another factor that should be explicitly considered in future studies.

Pg 17, this paragraph on ¹⁵N is way too long and hard to follow. Please simplify and condense, or break into a couple of paragraphs.

We have broken this into 3 separate paragraphs to improve the flow of this section.

Pg 18, lines 25-28: Inclusion of the vegetation data would substantially strengthen this statement.

We are unable to provide vegetation regrowth data because this experiment was designed to test how microbial cycling and ash deposition contributed to N availability.

Figure 1 is a bit confusing. The text description could use more detail for clarification.

Comments we received on earlier drafts of this manuscript have found this figure both helpful and unhelpful. Reviewer 1 here commented that the figure was redundant to the information provided in the text and may not be necessary, but also indicated some uncertainty about illustrations in the figure. As a consequence, we are hesitant to remove the figure entirely and prefer instead to clarify it. We are attaching a revised version of Figure 1 along with this reply. We have substantially increased the text in the figure, and would appreciate your feedback on the revised version.

Technical Corrections: Throughout: “Southeastern” is sometimes one word and some- times two words.

We have replaced “south eastern” with “southeastern” in all cases. Thank you for catching this.