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Interactive comment on "Wildfire effects on ecosystem nitrogen cycling in a Chinese boreal larch forest, revealed by ^{15}N natural abundance" *by* Weili Liu et al.

Anonymous Referee #2

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This manuscript seeks to determine the effects of fire, 4-5 years after burning, on soil and plant N cycling using a combination of soil and litter available inorganic N pools and fluxes and soil and plant 15N. This manuscript has several major issues that would need to be addressed before it could be published in Biogeosciences. Some of these concerns could, in theory, be addressed with major revision, but it would require another review as the manuscript would be completely changed. I hope my suggestions below are useful to the authors.

This is a largely observational study that also attempts to identify mechanisms. Though the two concepts are not exclusive, they can be challenging to reconcile, and the challenge is apparent here. The observation is that, 4-5 years after fire, there are higher

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N fluxes in organic soil, higher N pools in mineral soil, and higher 15N values in plants and soil in burned forest than unburned forest. The "mechanisms" that explain these observations are derived from patterns within the observations, but there are two problems associated with this extrapolation:

1) I was concerned by the fact that the mineralization results are compared to the TIN results, despite the fact that they were taken in different seasons. specifically, the TIN samples were collected before (June) the wettest part of the year (June to August; Line 107), whereas the N mineralization samples were collected after the wet season (Autumn, but not specified). Though it is well known that the size and direction of N pools and fluxes can change seasonally, these data are compared to each other as though they represent the same N. For example, the authors posit that the "high soil NH4+ pools did not lead to an elevated net nitrification rate" (Line 274), but the NH4+ pool and the net nitrification rate were collected months apart and likely had little bearing on each other. These two time points should not be compared this way.

2) As a result of the previous comment, the "mechanisms" that could explain these patterns of N cycling are difficult to establish, at best. The authors state that "a large amount of NH4+ was lost through volatilization" (Lines 276-277). However, there is no empirical evidence for this, and no way in which to rule out other mechanisms that explain differences in 15N isotopic signatures between the sites, such as denitrification or combustion (the authors addressed these, but emphasized volatilization as the proximate mechanism controlling 15N).

Throughout, from the title through the discussion, there is a strong emphasis placed on the revelation that 15N represented an "open" N cycle. For example, in the introduction, it is stated that "d15N could provide us a promising and comprehensive tool to detect the effect of wildfire on N cycling" (Lines 57-58). This makes it sound like establishing this is an objective of the paper, but this is a well-established use of these measurements (see Martinelli et al. 1999). By contrast, the important result related to the stated objectives would seem to be that fire leaves an open N cycle for several

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years. Likely, this problem could be addressed by changing verbiage.

There was a paucity of information about the fire: it was described as severe (line 118), but little other information was provided than unpublished results, not explained in the methods, that there was a tenfold loss of aboveground biomass (Lines 275-276). I wonder why the authors would expect to see a recovery of the N cycle given such substantial fire-related effects persist? This was not clearly articulated in the manuscript- what was the underlying rationale for this work, other than the lack of data on N cycling from Chinese larch forest?

I was also surprised that there was no mention of the role of cation exchange capacity in N cycling. While fluxes of N were affected by fire in the litter, N pools were affected in the soil. This likely represents the simple fact that a changed physical environment, combined with a reduction in plant uptake, means greater microbial processing of N in the organic layer and greater sorption of N in the mineral soil. This wasn't clearly articulated despite the fact that several lines were dedicated to changes in temperature (Lines 239-244).

Some specific comments: There were a number of typos and awkward phrases that would need to be cleared up before publication (for example: "winder" instead of winter (Line 106), "sever" instead of severe (Line 118), "colorimely" instead of colorimetrically (Line 146), "filtrated" instead of filtered (Line 131), "burned polts" instead of burned plots (Table 2) – not a complete list).

There are better citations for studies suggesting N limits high latitude terrestrial ecosystems than Popova et al. 2013 and Stark & Hart 1997 (Lines 34-35). Consider Vitousek & Howarth 1991, Lebauer and Treseder 2008, Elser et al. 2207, and Harpole et al. 2011).

How is Figure 3 different from Figure 4? Similarly, how are the values provided in the paragraph beginning on Line 202 different from the values described in the paragraph beginning on Line 211? Both involve foliar d15N, for example. Not clear.

How was the branch, moss and fine root 15N relevant to the objectives? Seems like these data, especially the moss, are ancillary and it was not clearly stated that there was a goal to observe vertical profiles of d15N signatures from tree top to 30 cm mineral soil.

Figure 1 didn't come out very well, and wasn't very helpful. The caption makes it sound like the unburned area is in the burned area: "Unburned area is chosen in the nearby burned area as control."

References: Martinelli, L. A., M. C. Piccolo, A. R. Townsend, P. M. Vitousek, E. Cuevas, W. McDowell, G. P. Robertson, O. C. Santos, and K. Treseder (1999) Nitrogen stable isotopic composition of leaves and soil: Tropical versus temperate forests. Biogeochemistry, 46, 45–65.

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