

## ***Interactive comment on “Spatio-temporal analysis of nitrogen cycling in a mixed coniferous forest of the northern United States” by I. Howard and K. K. McLaughlan***

**Anonymous Referee #3**

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This manuscript presents an analysis of tree ring stable N isotopes from an unmanaged pine forest in Minnesota, adjacent to a lake where sediment isotope analyses have also been carried out. The major change in disturbance regime and biogeochemistry in this stand is the suppression of the natural fire regime beginning in the early 20th Century.

The data presented in this paper are certainly a valuable contribution to a literature that is still somewhat inconclusive about how to process or interpret N isotopes in tree rings. The length of the record presented is a major strength of this manuscript, as is the ability to compare with a sediment record. The exploratory work attempting to explain variation in N isotope patterns among individual trees is interesting, but did not add much in the end.

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Overall this study was quite well conducted, but the manuscript could be more carefully written to avoid overstating the conclusions. The largest problem with this manuscript is that in several places, it frames its conclusions as relating directly to N availability, indicating that the accumulation of biomass in the absence of fire disturbance has more than compensated for the global and regional increases in DIN deposition. This is a reasonable explanation for the observed pattern, but is not conclusively proven. These sections (e.g. the second paragraph of the results and first sentence of the discussion) could be rewritten to acknowledge that testing for changes in  $\delta^{15}\text{N}$  is not a very direct test of the hypothesis presented, though it is helpful evidence. The linkage between  $\delta^{15}\text{N}$  and N availability at the ecosystem level has a firm theoretical grounding but mixed empirical support in the literature, as there are many other factors affecting  $\delta^{15}\text{N}$  (which are described elsewhere in the manuscript).

I am curious what the authors think about the rather striking similarity in long-term trend and breakpoint between this site and Mirror Lake NH site, (McLaughlin et al. 2007). This is especially interesting given the very different natural disturbance and land-use histories of the two sites. Are there other sites in the central and eastern parts of North America that show similar patterns? If so, is there a more parsimonious explanation than the various changes in disturbance regime at each site?

Abstract: In the concluding sentence, “consequences of global-scale alterations . . .” is a bit ambiguous, and could be worded more precisely. Introduction:

The description of how fire and human disturbances affect the N cycling and N availability could be more thorough; the conclusions hinge on the reader understanding these links.

Study Site: Unpacking the fire history some more might help the reader better understand the site. Also, how are stand-initiating fires occurring every 10 years while the return interval is 22 years? Are these two metrics at different scales (stand vs. landscape?) This should be clarified.

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Section 2.1. As this is a global journal, a bit more information about the soils and climate at the study site might be helpful to readers who are not familiar with the region.

Section 2.2. – the writing is awkward and difficult to follow in several places

I would also like to see the authors briefly discuss their choice to analyze total N rather than conducting some sort of extraction or separation procedure, rather than simply citing one reference. There is no single standardized approach that is universally accepted, so this choice is reasonable, but should be discussed.

Section 2.3, line 22: Is this really a Z-score treatment? Were values expressed as # of standard deviations?

P3627, L20: what else might be different near the shoreline? Soil moisture, soil texture, soil organic matter? How would these relate to N cycling? What about fire frequency? Also, is the lake level steady over time or not?

P3631, L19: can the authors be more specific about the “low levels” of N deposition?

Figure 2: What is meant by “% change in slope”? Is this the second derivative of elevation? Or should it just say “slope (%)”? Also, the scale bar is a bit ambiguous – is the entire bar 100m, or each marked interval?

Figure 4: this does not appear to be the correct caption for this figure.

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Interactive comment on Biogeosciences Discuss., 12, 3617, 2015.