

Interactive comment on “Nitrogen inputs and losses in response to chronic CO₂ exposure in a sub-tropical oak woodland” by B. A. Hungate et al.

Anonymous Referee #2

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The paper “Nitrogen inputs and losses in response to chronic CO₂ exposure 1 in a sub-tropical oak woodland” is a synthesis on nitrogen inputs and losses in an oak woodland after 11 years of exposure to elevated atmospheric CO₂. Briefly, the authors found that while elevated CO₂ may have resulted in a positive response in nitrogen fixation, it was only transitory, and the greatest effect may be an increase in nitrogen leaching in this ecosystem. The paper is well written, if somewhat lengthy, and the authors did an excellent job at synthesizing the results from multiple studies over the course of the field experiment. This paper will be very useful to those working to build an integrated picture of the effects of climate change on the nitrogen cycle, and the length of the paper certainly demonstrates the complexity involved. I have only a few

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minor comments to be addressed listed below.

1. Section 3.1 (N₂ fixation by *G. elliotii*). In the last paragraph of this section, the authors state that as the dominant plants grew larger, N₂ fixation declined. While I don't disagree, this is somewhat confusing because the proportion of N from fixation increases over time (Table 1) and fixation estimated via ¹⁵N data (I'm assuming) shows no strong directional pattern over time (Figure 1). I feel some additional clarification would be very helpful here.

2. Section 3.5 (Nitrous oxide and nitric oxide fluxes). It is not clear to me if the number 1.4 g N m⁻² refers to the loss of N₂O-N over the 11 years, or the increase in the loss of total N (as the difference between elevated and ambient plots). Because of my confusion, it is also not clear if the 1.4 g N m⁻² is comparable to the NO_x loss of 0.2 g N m⁻². Given that the NO_x losses are so small, it is not crucial to the overall conclusions the authors draw, but it would help if the authors clarified this point.

3. Are the legume nodule mass values reported in Figure 2 directly comparable given the fact that the first set came from ingrowth cores while the second set came from intact cores? I'm only looking at the figure here, so I'm not sure if this is discussed or not in the manuscript. If they are indeed that different, then this figure might be very misleading if not read carefully.

Interactive comment on Biogeosciences Discuss., 11, 61, 2014.

BGD

11, C264–C265, 2014

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