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Interactive comment on "Nitrogen inputs and losses in response to chronic CO_2 exposure in a sub-tropical oak woodland" *by* B. A. Hungate et al.

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The reviewer raises important issues and has made some excellent suggestions. Below, we respond to each and describe how we have revised the manuscript.

Reviewer 1

1. Section 3.1 (N2 fixation by G. elliottii). 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.

Authors' Response: This is a good suggestion, because changes in N2 fixation could be driven by changes in the proportional reliance on atmospheric N2. In this case, while the proportional reliance on atmospheric N2 goes up over time, though only slightly, total N2 fixation goes down over time. This is possible because productivity of the legume is the major driver of the response of N2 fixation. This is now clarified in the text by adding two sentences to section 3.1: "Given the small range of variation in reliance on atmospheric N2, temporal changes in N2 fixation (Figure 1) were driven by effects of time and treatment on the productivity of G. elliottii. Nevertheless, the proportion of N derived from the atmosphere by G. elliottii was sensitive to temporal variation and to the CO2 treatment."

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 NOx loss of 0.2 g N m-2. Given that the NOx 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.

Authors' Response: We state more directly that the 1.4 g N m-2 figure describes the effect of elevated CO2 on N2O losses: "our best estimate is that elevated CO2 increased losses of N2O-N by 1.4 g N m-2, though this difference (elevated-ambient) is not significant (5% and 95% confidence limits, -2.9 to 5.3)."

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.

Authors' Response: As estimates of nodule biomass or productivity, they are not di-

rectly comparable as the reviewer suggests. The ingrowth cores are closer to nodule production, whereas the direct soil cores measure the mass of nodules. We now note this explicitly by adding the following sentences:

“The earlier assay using ingrowth cores captures new nodule growth, whereas the cores at the final harvest measure the standing crop of nodules. Though not directly comparable, both assess responses of N2-fixing nodules to the elevated CO2 treatment and thus are presented together here.”

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