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Biogeosciences Discussions

Interactive comment on "Free atmospheric CO_2 enrichment did not affect symbiotic N_2-fixation and soil carbon dynamics in a mixed deciduous stand in Wales" *by* M. R. Hoosbeek et al.

Anonymous Referee #1

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This paper presents C and N content data for bulk soil and SOM fractions, as well as the d15N of vegetation and d13C of bulk SOM, from samples obtained from an elevated CO2 experiment in Wales. The work is of obvious importance for biogeochemical questions about ecosystem C and N dynamics in a high CO2 world, and touches on important topics addressed in many elevated CO2 papers. I have several questions and comments that I think, if addressed, could make this a stronger contribution.

First, think the authors should address free-living N2-fixers in the soil as well as symbiotic fixers. This has been addressed at a couple of FACE sites (pine forest, Mojave, perhaps others?) and deserves mention here.

More importantly, the authors start to make some points that could be expanded on to make this a stronger paper. How do the declines in NH4+ with elevated CO2 compare to plant productivity and N content in those plots? What is the isotopic composition of the NH4+, and how does that relate to the d15N of the Betula? The data as presented prevents us from understanding these N dynamics and yet give tantalizing hints that something really interesting is going on. Do we know about rates of nitrification (i.e. are they increasing with elev Co2 to explain the declines in NH4+?)? I think the d15N of vegetation can be used to infer more than just the N2-fixation status. d15N w/ elevated CO2 can provide a valuable proxy of the pools of plant-available N. The lack of difference b/n Betula in and out of FACE treatment implies that FACE has not changed the isotopic signature of plant-available N, which in turn implies that sources have not changed. However, NH4+ content has declined. What are some possible mechanisms for such a collection of data?

This is a great paper in which to discuss the incorporation of the 13C label into the SOM fractions. I gather these investigators did not perform these analyses, but it seems relevant to address the concepts introduced in Lichter et al. 2008, who present the incorporation of the label into separate SOM fractions. It's important that 20% of the C incorporated into SOC at Duke FACE was apparently incorporated into the smallest size fraction! Can these investigators add this analysis to this paper? If that's possible, it would be a much stronger contribution. The d15N of those fractions would also be important; it would signal, qualitatively, the degree to which microbial transformations of organic matter had occurred before that OM had arrived in, or had occurred within, those fractions.

I think if the authors are going to address priming here, they need to address the motivating factors driving priming. The motivation of microbes performing this priming is a key point - if NPP increases w/ FACE, decomposition of SOM must occur to a greater degree if pools of inorganic N do not differ and if there is no additional source of nutrients that differs b/n FACE treatments. This might explain the mitigation in SOC accrual

with FACE - the enhanced NPP was "fueled" by N derived from SON mineralization, microbes were thus N-deprived, and turned to SON min for additional supplies.

It looks to me like the se increases for N total in the elvated CO2 plots - this is important, and implies that something has changed in N dynamics here in spite of the similar inorganic N contents. Can the authors expand on this? I am also confused about their discussion of a decline in N with FACE (p. 4166-4167) when discussing the C:N ratios. The decline I see in N is in NH4+, and the C:N ratios are referring to organic matter, which is the lion's share of N in these soils. This needs to be clarified.

In general, I like this paper, but I think it could be a much stronger contribution if the authors could expand on their data. I realize that I might be asking for the moon - if they cannot perform isotopic analyses, for example, on soil solution NH4+, or on separate SOM fractions - however, even in the absence of those data, the authors could develop some mechanisms beyond the NUE hypothesis to try to explain the decline in NH4+, the consistency of Betula d15N, and the changing C:N ratio over time.

Interactive comment on Biogeosciences Discuss., 7, 4153, 2010.