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Biogeosciences Discussions

Interactive comment on "Variability of projected terrestrial biosphere responses to elevated levels of atmospheric CO_2 due to uncertainty in biological nitrogen fixation" *by* J. Meyerholt et al.

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Response to Joshua Fisher

> We thank Joshua Fisher for his interest and his helpful comments on the manuscript.

BNF in FUN was constructed *relative* to the costs of the other N uptake pathways. BNF would not occur if the other pathways are cheaper. So, if this is left un-checked in O-CN (this wasn't clear), then you may get BNF occurring when it otherwise should not be. As such, the cost of BNF in FUN was specified *high* relative to the other costs, so you wouldn't actually get it much except under very high N limitation. So, I

wouldn't necessarily say that "NDT" has "low N fixation costs". I guess everything is relative. As Reviewer 1 insightfully pointed out, NDT should also be constrained by NPP and demand relative to C:N ratios. Even in strong N limitation (e.g., those boreal forests where BNF increased "beyond plausible rates"), if there isn't much NPP, then BNF cannot occur. The NPP used in FUN is after respiratory costs. Perhaps the NPP is too high in O-CN in these regions? The eCO2 case would provide more C to pay for BNF in NDT, as you correctly pointed out; but, payment is still limited by available water and C:N ratios. FUN wouldn't just pay for more BNF ad infinitum just because it could.

> We apologize if this was not made clear enough in the manuscript, but NDT was not meant as our representation of FUN. FUN was cited mainly as a source for its reasoning to include the temperature sensitivity of Nitrogenase activity sensu Houlton et al. (2008). The revised version will clarify this issue. Some of the characteristics of FUN, such as the carbon cost evaluation between root uptake and BNF, can be found in the OPT approach. In our manuscript, we have adopted the scheme described by Rastetter et al. (2001), on which also FUN is based. (Fisher, J. B., S. Sitch, Y. Malhi, R. A. Fisher, C. Huntingford, and S. Y. Tan (2010), Carbon cost of plant nitrogen acquisition: A mechanistic, globally applicable model of plant nitrogen uptake, retranslocation, and fixation, Global Biogeochemical Cycles, 24(1).)

> None of the models in our study would indefinitely pay for more BNF. In both NDT and OPT, the occurrence of BNF does not directly feed back on root uptake. However, the root N uptake capacity affects the choice to do BNF in both models (in NDT, NDS as this controls the available C and N for growth, in OPT, because the costs of fixation are evaluated compared to the current root N uptake capacity and costs), limiting the investment into BNF in the long-term.

> The C used for BNF costs in NDT is a fraction of the plants' labile C pool, which reflects the N limitation on C assimilation on the one hand, and N limitation on growth and turnover on the other hand. Different to the FUN approach, which directly relies on NPP as C source for BNF, the labile C pool for fixation is not only affected by moisture

control on photosynthesis, but also by N limitation on growth. While little labile C is available if N is sufficient in supply (much of the assimilated C is used for growth), labile C accumulates under N deficit. In NDT therefore, the N fixation capacity increases with N stress, and decreases with increasing root supply of N. In other words, if root uptake provides enough N in NDT, no BNF occurs.

> Therefore, high boreal BNF under eCO2 in NDT is not necessarily a result of high NPP, but rather high N demand; It might be that the NPP constraint (that does seem to work reasonably without perturbation, Fig. B1 (d)) is simply offset under high CO2 concentrations at the end of the experiments.

> We still agree that we should not refer to the BNF costs in NDT as "low" (P19440 L 19-20). Instead, high tropical BNF in NDT should be explained as a result of high temperatures, high N losses, high C availability, combined with comparably low cost, relative to other biomes. Note, however, that the C costs for N fixation in the tropics are not much lower than the average values implied by NDS and OPT.

Finally, you (and I've heard this from others, so I'm in the minority here) remarked that the sensitivity of NDT to "instantaneous" changes is a bad thing. I still wonder about that. For instance, we find many observations of N fixing plants that have stopped fixing in relative instantaneous time scales because N has become more readily (i.e., cheaper) available from the soil, or N demand/NPP has come down; this can change if conditions change. Again, timing is all relative.

> We agree that such a statement should be made with caution, especially when experimental evidence is ambivalent. Our reasoning to see strong short-term responses as implausible is based on the following: 1. Even if field studies find such responses, scaling from individual plants/small stands to entire ecosystems somewhat contains the assumption of ubiquity of BNF on large scales, which is not really the case. 2. In multi-year FACE experiments, N limitation on eCO2 responses was not consistently lifted by BNF. We would still propose to revise the corresponding discussion paragraph

(P19442 L 1-10) to address this issue a bit more sensitively.

Interactive comment on Biogeosciences Discuss., 12, 19423, 2015.