Biogeosciences Discussions

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Comment on bg-2021-277

Anonymous Referee #2

Referee comment on "Fire in lichen-rich subarctic tundra changes carbon and nitrogen cycling between ecosystem compartments but has minor effects on stocks" by Ramona J. Heim et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-277-RC2, 2022

The manuscript by Heim et al. reports on C and N stocks and their stable isotope composition in various ecosystem compartments (soil organic matter, aboveground vegetation including lichen and vascular plants) in three large fire scars (between 12 and 44 years post-fire) and adjacent control sites; in Western Siberia. Only d15N data on soil samples are missing (due to low soil N concentrations, although I would think this is quite feasible from a technical point of view).

The authors' main conclusions are that

(i) total ecosystem C and N stocks were not significantly affected by fire.

(ii) soil C and N stocks were not affected. Soil make up the majority of ecosystem C and N stocks.

(iii) vegetation was affected, mainly due to a reduction in the lichen layer, which takes a long time to recover.

(iv) a few trends in d13C and d15N – but those are not well explained, see further.

Personally, I would stick to the first 3 (which are related, obviously). Given that the soils in these systems are relatively poor in organic C (from less than 1 to slighty over 2% on DW basis), these conclusions are in line with expectations. The interpretation of the isotope data is not conclusive and too speculative.

Overall, I would mostly encourage the authors to take out the more speculative sections, keep the manuscript and conclusions to what can be unambiguously demonstrated, i.e. fire affects aboveground biomass, and not belowground OC stocks in a system with rather mineral soils, and as the biomass presents a small fraction of ecosystem OC stocks, those are not strongly affected. The isotope data are intriguing, but I do not feel much can be drawn from them at this stage.

Carbon stable isotope data

-abstract L 11-13: "This could be related to …": this is not conclusive, and your data do not really allow you to draw anything firm from this.

-Figure 2: use small delta symbol, not capital delta on y axis label

-The mechanism behind the decrease in d13C in plants and lichen is not well understood. I am not very convinced on the suggestion that lower d13C in local atmospheric CO2 due to the fire would be the cause. This would imply a higher CO2 concentration, from increased mineralization – but the soil OC stocks suggest there is no enhanced mineralization (at least not observed as a decrease in C stocks). Such local variations in CO2 and d13C-CO2 are typically observed in closed canopy systems. Without actual data demonstrating local gradients in d13C-CO2, I would not make this suggestion too explicit. The authors refer to Dawson et al. (2002) and Lakatos et al. (2007) in this context – but neither of these papers mention anything about fire and its effect on local d13C-CO2 years after the actual burning.

Alternative explanations for minor shifts in plant d13C (water availability or sources, interactions with nutrients, ..) are not considered (although with the data at hand, one would not be able to make a strong case for a precise mechanism).

-Hence, I also find that conclusions such as "they [=lichen] strongly reflect environmental changes, such as increased soil respiration, after a fire", since the data presented do not show direct evidence for increased soil respiration.

-The explanation offered for the soil d13C data is also too speculative in my opinion. Linking such a small difference to increased temperatures (no data are presented to back up an increase in temperature), and I feel that Ehleringer et al. (2000) is not adequately

interpreted here: microbial communities do not have an inherent preference for 13C-depleted organic matter- that's not what Ehleringer et al. conclude. Microbial biomass appears to be slightly enriched in 13C, yes – but again, given that the soil C stocks do not appear to change, why invoke higher mineralization (and thus, a higher contribution of microbial biomass) ?

Nitrogen stocks

The scenarios described in Section 4.1.1. are quite speculative – possibilities, but nothing conclusive.

Statistical analysis: The approach used by the authors is outside my comfort zone, but I do not understand why they follow this road (posterior distributions). It would be good to shed some light on why this is helpful, and why not simply use the actual measured data. I would personally prefer to see the actual data presented in the ms, and the predicted values in the supplement.

Minor suggestions

-species names should be in italics throughout

-soil d15N data: given the N concentrations (0.05 % DW approximately), I don't see why this is not possible – 20 mg of dried sample should provide 10 µg N for analysis, which is largely sufficient for good d15N data.