SOIL Discuss.,
https://doi.org/10.5194/soil-2019-86-RC1, 2020

Interactive comment on "Switch of fungal to bacterial degradation in natural, drained and rewetted oligotrophic peatlands reflected in δ^{15}N and fatty acid composition" *by* Miriam Groß-Schmölders et al.

Anonymous Referee #1

Received and published: 1 February 2020

Groß-Schmölders and co-authors studied stable isotope profiles in intact, drained, and rewetted peatlands. They report finding a distinct maximum of d15N values present in drained but absent in natural peatlands. This maximum coincided with other factors of maximum decomposition (C:N, bulk density), as well as with the transition from fungal to bacterial dominance.

The authors present compelling evidence in support of an interesting and so far undescribed phenome unique to degraded peatlands, which surely is of great interest to the SOIL readership. The mansucript is clearly written and easy to read. In my view,

Printer-friendly version

Discussion paper

however, the manuscript fails to proof many of the authors central conclusions. I think this can be addressed by the authors by slightly changing the 'angle' of the paper. I also I also have concerns with regards to the authors methodology (PLFA analysis), but I don't think these are critical to the papers key finding. Overall, I think that this paper requires a major revision before publication.

1. The authors' central hypothesis ('maximum 15N enrichment at maximum microbial diversity) seems to come out of nowhere, and it is unclear to me how the authors came up with this hypothesis except as a post-hoc justifying their results. I do not see why greater microbial diversity should necessarily imply greater nutrient limitation as it matters little to the nutrient whether it is taken up by fungi or bacteria (fungi compete with themselves as much for nutrients as with bacteria and vice versa, abundance does not equal activity, etc.). It is also unclear to me how this conclusion is supported by the presented data, which shows that the 15N maximum occurs at the same depth of the change fungal to bacterial dominance, but does not provide evidence that one is related to the other. I don't see why these changes in microbial community composition would provide evidence for greater nitrogen limitation at the depth of the 15N maximum. However, I'm not sure if this rather speculative interpretation of 15N being driven by microbial community composition is actually needed in this paper – I think describing the differences between drained and undrained peatlands provides valuable information by itself.

2. I think the manuscript could be improved by presenting/discussing the results in a different way. In the present version, the author very much focus on changes in d15N and other parameters with depth. I would recommend to start by comparing drained and undrained soils – I think it would be helpful if the authors first identify how drainage has changed the parameters measured in the soil profiles (e.g., drainage increase 15N values in the mesotelm). The authors can then discuss which processes led to an increase in 15N in drained peatlands (relative to intact neighbour sites), and why these processes were strongest in the center of the mesotelm and less pronounced towards

the surface and the catotelm edge.

3. The authors could also improve the mansucript by providing a more detailed view on the processes that cause the N isotope fractionation in these soils. In particular, they do not propose a fate for the 14N-depleted nitrogen fraction. How does this carbon get lost from the soil profile (in drained relative to intact peatlands)? It does not simply get transported downwards in the soil profile as no large difference in d15N was observed in the catotelm (Fig. 2). Mineralization is a likely mechanism, does that mean that more depleted 15N is leached out of the soil profile and exported from the peatland? Or are there stronger gaseous losses (N2O, denitrification) in drained peatlands? What is the role of plant and microbial uptake of 15N in this process?

4. PLFA analysis: The authors use a non-standard method to extract/purify/derivatize PLFAs for analysis. While this is not a problem in itself, this method looks like a total fatty acid extraction to me. At least, it extracts and recovers free fatty acids (as shown by the use of the internal standard nonadecanoic acid). Please provide information how phospholipids were separated from glycolipids and neutral lipids in this method.

Some language issues: - I would prefer the more descriptive term 'maximum' rather than 'turning point', which implies some change in direction in processes. I think this would also improves the clarity in a central point of the manuscript.

L16: 'stable isotope signatures': See this advice on 'Isotope terminology' from Z. Sharp's 'Isotope Geochemistry' book (https://digitalrepository.unm.edu/unm_oer/1/): Mistake: "The isotopic signature of the rock was d18O = 5.7‰;" Recommended Expression "The d18O value of the rock was 5.7‰Ṫhus this rock has the oxygen isotope signature of the mantle." Explanation: "The word signature should be used to describe the isotopic composition of a significant reservoir like the mantle, the ocean, or a major part of the system being studied, not to the isotopic composition of ordinary sample" - L311: 'equilibrium' between fungi and bacteria – I don't think equilibrium is the correct concept here. Maybe change from fungal to bacterial dominance, but even that is not

necessarily true given that fungi have more biomass per unit PLFA than bacteria, and you're only looking at a very small fraction of the total PLFA pool.

Minor comments: L12-15: The first three sentences have very little to do with the content of this manuscript. L304-306: highly speculative and not well referenced. Figure 2: check axis ticks for BD and C/N, start these axis at 0. Tables 3-6 could be places in a supplement.

Interactive comment on SOIL Discuss., https://doi.org/10.5194/soil-2019-86, 2020.