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6, S999–S1007, 2009

Interactive Comment

# *Interactive comment on* "Nitrogen fertilization did not affect decay of old lignin and SOC in a <sup>13</sup>C-labeled arable soil over 36 years" *by* A. Hofmann et al.

## A. Hofmann et al.

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We thank the referees for their constructive comments and overall positive reception of the manuscript. Below we give a list of the referees comments and our responses.

### **Referee 1**

## 1. Introduction

The authors need to develop the context of the research more.

We will revise the introduction of the manuscript according to comments 1.1 to 1.5.

1.1 P1658 L24: The initial jump here from increased plant production to increased



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Interactive Discussion

decay of SOM is too terse. A few more sentences joining these two statements would be helpful.

This introductory section will be amended. An increase in the amount of plant biomass returned to the soil because of nitrogen fertilization was already discussed in the 1950s as a potential means to raise soil organic matter concentrations in arable soils (Allison, 1955). We will include this information in the revised manuscript. Also comment 1.4. will be included at this point. It states that net storage of SOC is a balance between biomass input and decay. With this statement we can then lead over to the decay of SOM.

**1.2** P1659 L10: I would remove dependency on microflora. Tangential for the paper.

This will be removed in the revised manuscript.

**1.3** P1660 L 1 The introduction could use more development here. Can you describe this a bit more? Maybe give a hypothesis?

The last part of the introduction will be improved by adding a concise aim/ hypothesis: The objective of our study was to test if nitrogen fertilization might reduce lignin decay over a long time period in a field experiment.

**1.4** For SOC storage, it would be good to acknowledge that net storage is a balance between production and decay.

We will incorporate this information in the introductory section of the revised manuscript, as stated in 1.1.

**1.5** The authors focus on N, while also P was added. A short literature review of P addition and SOM dynamics is important so as not to over-interpret the N results.

We agree with the referee. The fertilization treatment of the long-term field experiment was nitrogen and phosphorous (300 kg N ha<sup>-1</sup> a<sup>-1</sup>, 150 kg P ha<sup>-1</sup> a<sup>-1</sup>) in combination, as stated in the methods section. All effects (negative, positive, neutral) of the

6, S999–S1007, 2009

Interactive Comment

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Interactive Discussion



fertilization would thus be due to both, nitrogen and phosphorous. The focus of our study is on the effect of nitrogen fertilization on lignin decay because nitrogen was suggested to reduce enzymatic lignin decay by Keyser et al. (1978), Fog et al. (1988), Carreiro et al. (2000), Hagedorn et al. (2003), Foereid et al. (2004). Phosphorous was not suggested in this context. We found only few studies on the effect of phosphorous on SOM decay. Ostertag and Hobbie (1999) show that fertilization with N or P had neutral or positive effects on decomposition of fine roots. Han et al. (2006) show that the rate of SOC decrease was reduced when N and P were fertilized, instead of N alone. The field experiment we used archived samples of, was originally designed as a long-term maize fertilization experiment with a conventional agricultural fertilization treatment. We accepted the compromise that not only nitrogen but also phosphorous was fertilized (two factors changed instead of only one) because we were interested in this experiment for the unique long-term natural <sup>13</sup>C-labeling in combination with the fertilization treatment. Nitrogen fertilization alone in a long-term experiment might have induced phosphorous limitation both for maize plants as well as for microbial communities. We would like to address this referee comment on phosphorous in the discussion, without expanding the manuscript too much.

#### 2. Methods

**2.1** In the site description, can you include mean annual temperature and precipitation?

This information had already been included in the methods section (P1660 L1-18). It will be moved up directly to the site description in the revised manuscript.

#### 3. Results

**3.1** The calculations of lignin production are dubious and not an overall asset to the paper. This could be mentioned in the discussion, but it dilutes out the results section. In the second part of section 3.4 (P1665, L21 to P1666, L5) we tried to give an approxi-

6, S999–S1007, 2009

Interactive Comment

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Interactive Discussion



mation of how much organic carbon and lignin carbon entered the soil via belowground biomass. The results are best estimates for this long-term field experiment, where no belowground biomass data were available. We would like to keep the paragraph on belowground lignin carbon input but agree that it should be moved from the results to the discussion section where it is needed for comparison of biomass inputs with decay.

#### 4. Discussion/ conclusion

**4.1** Not all lignin is of similar chemical recalcitrance. The authors focus mostly on quantity in the discussion, but N fertilization could be decreasing the recalcitrance of lignin the plants are producing.

The quality of lignin can be assessed by acid to aldehyde ratios and syringyl to vanillyl ratios, which indicate the state of lignin degradation (e.g. Kögel, 1986; Guggenberger et al., 1994; Sollins et al., 2006). We will provide these quality indicators for lignin from aboveground plant material in the revised manuscript.

**4.2** I like the last paragraph contrasting stabilized and non-stabilized SOC. N addition might have no effect on bound C, but affect non-stabilized SOC.

We will add this comment on stabilized versus non-stabilized SOC to the second conclusion.

**4.3** Point 3 of the conclusions seems a bit harsh in this light. For example there is a lot of excellent work on soil excenzymes that would provide insight into recalcitrant C dynamics without measuring lignin.

We will omit this conclusion, indeed it sounds very harsh. A recent paper on the topic of exoenzymes is by Keeler et al. (2009). The authors did not find an effect of nitrogen fertilization on lignin degrading enzyme activity. We will cite this paper in the discussion of the revised manuscript.

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6, S999–S1007, 2009

Interactive Comment

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Interactive Discussion



extremely valuable for the interpretation of the results, especially those of the nonfertilized treatment. However, unfortunately these data are not available because atmospheric nitrogen deposition was not analyzed at the site of the agricultural field experiment as this had not been an objective of the experiment when it was started in 1966. Therefore we had to rely on data on the approximate atmospheric deposition (10-25 kg N ha<sup>-1</sup> a<sup>-1</sup>) obtained from a study by Holland et al. (2005), who provide maps of atmospheric nitrogen deposition for Europe and the U.S. The time period of these measurements spanned 1978-1994. These are the best available data and they are given in the manuscript.

#### 1. Introduction

**1.1** The authors are right in pointing out that N deposition might generally affect the amount of available N in the system. However, most of the available N entering soil is soon incorporated into microbial biomass.

We agree that added nitrogen cycles through the microbial biomass. However, it was also plant biomass that was affected by nitrogen fertilization, as can be seen in the significant fertilization effect on grain yield and stover yield (Table 3). The fact that soil microorganisms use the available N does not contradict the suggestions by Fog (1988), Keyser et al. (1978) and Carreiro et al (2000) of how this nitrogen might affect the soil microorganisms, e.g. selection of certain decomposer microorganisms or blocking of enzyme production (P1659 L10-19).

#### 2. Discussion

**2.1** To interpret the data obtained I suggest to provide much more data on the amount of N fertilization over time as well as the changes that might have occurred in the amount of N atmospheric input.

We agree that more data on the atmospheric input (temporal resolution) would be

BGD

6, S999–S1007, 2009

Interactive Comment

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Interactive Discussion



**2.2** Most probably the SOC contents will have changed during the experiment. This will most probably also change the amount of available N in the soil, leaching of N etc. Thus much more information on the N are necessary to better interpret the data.

As we stated on P1664, L 2-3 total SOC concentrations decreased slightly in both treatments over the course of the experiment (Table 2). At the same time we could show that C to N ratios in soils did not change significantly over time (Table 1). We are not sure if we understand the comment correctly. But as we had only archived (dry) soil samples available for our study, we could neither infer data on nitrogen mineralization rates nor on soil nitrogen pools. Nitrogen mineralization dynamics would have to be measured on an intra-annual time scale while the focus of our study was on the long-term (inter-annual) SOC and lignin concentration measurement.

**2.3** The discussion switches between decomposition of total SOC and lignin. This should be clearly differentiated.

In the revised manuscript we will discuss the effect of nitrogen fertilization on lignin decay separately from SOC in a new section of the discussion.

**2.4** The effect of N on decomposition of lignin may be adverse to the effect on total SOC.

Indeed, mineral fertilization accelerated decomposition of relatively new (younger than 36 years), maize-derived lignin. On the other hand, new maize derived total SOC was not affected by fertilization (section 4.2). We will amend the second conclusion to state this point more explicitly. This adverse effect of fertilization on lignin and total SOC was however not found for the old (> 36 years)  $C_3$ -derived carbon, pointing out a difference for non-stabilized and already stabilized SOC as stated in comment 4.2 (referee 1).

2.5 Lignin is quantitatively only a minor component of the plant residues.

The aboveground maize plant material of the experiment contained 20 to 40 mg lignin carbon ( $C_{VSC}$ ) g-1 plant dry weight (Table 3, measured after CuO oxidation, Hedges

6, S999–S1007, 2009

Interactive Comment



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and Ertel, 1982). This corresponded to 50 to 95 mg C<sub>VSC</sub> g-1 plant organic carbon (OC). Dignac et al. (2005) give similar concentrations for maize plant material, also measured after CuO oxidation: 55 mg C<sub>VSC</sub> g-1 plant OC in the leaves, 107 C<sub>VSC</sub> g-1 plant OC in the stems and 103 C<sub>VSC</sub> g-1 plant OC in the roots. Although C<sub>VSC</sub> represents only 5 to 10

**2.6** Did the authors consider priming effects, which might be different in the different N fertilization treatments.

Yes, we did consider priming as one of the underlying concepts. In the introduction of the revised manuscript we will use the term negative priming effect (Kuzyakov et al., 2000) so as to better provide the reader with the link to the concept. However, we avoided putting too much emphasis on the concept because priming effects are defined as short-term changes in the turnover of SOM, caused by comparatively moderate treatments of the soil (Kuzyakov et al., 2000). If we had observed an effect of the fertilization on the decay of old,  $C_3$ -derived lignin carbon or SOC, this would then have been a long-term priming effect. We cannot provide data about priming as a short-term effect because the effect occurs within time scales that need a higher temporal resolution than analyzed.

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6, S999–S1007, 2009

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6, S999–S1007, 2009

Interactive Comment

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6, S999–S1007, 2009

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