**Reviewer 1:**

There should be a comment on the litter decomposition in the course of the experiments. In Calamagrostis, it seems that it was very low in the beginning, than fast between weeks 4 and 15 and later none or very low. Also with Lotus, the decomposition between week 15 and 30 seems to be close to zero. What is the reason for that?
We can assume two major reasons. One is linked to the environmental conditions present at the sites (mainly low water availability). More than in well developed soils, processes and turn overrates in the soil material of the “Chicken Creek catchment” are obviously linked to the direct climatic conditions present at the site due to the low capacity to store water. Thus the low precipitation during the autumn period might have a stronger influence on the degradation of plant litter compared to other sites with more developed soil ecosystems
Furthermore of course during the degradation of litter the ratio of easily degradable compounds to more complex compounds decreases, thus degradation rates slow down significantly. Again the specific properties of “young” soils may influence degradation in this phase more than well developed soils do, as degradation of lignin etc requires well developed microbial network structures as well as a high nutrient status of the soil, both properties which do exist only to a low degree in developing soils. These aspects have been included in the revised version in the discussion

I am not sure what is the source of the observation of high levels of the FA 18:3 and 18:2w6,9 in the initial phases of decomposition. Both of these FA are common in many litter types and the authors should clearly show what is their content in their litters. At best, the PLFA signature of the litter should be added. Although the authors claim that there is little litter material mixing into soil, the FA can perhaps leach from the litter. The best would be to demonstrate if this can occur.
As we measured phospholipid fatty acids (PLFA) in our study and not pure neutral lipids, a high content of “free” PLFA is quite unlikely. Zelles et al. (1999) calculated the average half time of free PLFA in soil to less than 1 day due to their high energy status and the subsequent fast degradation. Thus we assume that the measurement of an individual PLFA is strongly connected to the respective organisms, they are indicative for. This point has been included in the discussion section

Minor comments:
Abstract L12: delete "bulk"
done

Abstract L13: delete "process"
done

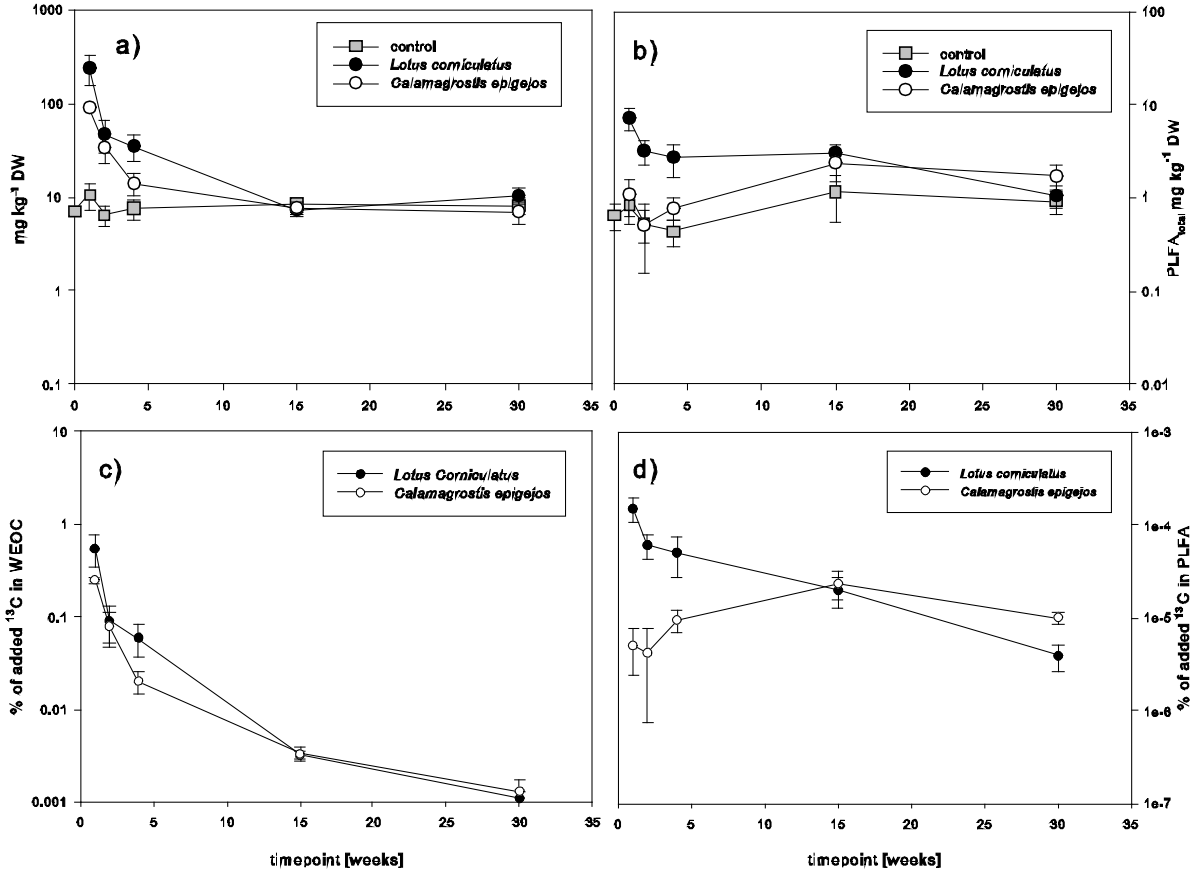
14985 L5: change "closed" to "close"
done

14988 L6: "the soil moisture (0-5 cm)" - rephrase, the meaning is unclear
rephrased to “the soil moisture in the top soil”

14992 L6: in Fig. 2, the interpolation of the litter decomposition data fits poorly, in fact, there is almost zero decomposition between weeks 15 and 30; please connect means with straight lines
We used the same fitting like in a previous microcosm-experiment (Esperschütz et al., 2011); a connection of single points would make the graph hard to interprete; thus we did not change the graph in the revised version

14992 L9: I can not see the data from week 10, so you can not speak about rates before / after week 10.
10 weeks has been changed to four weeks in the revised version

14992 L18: Include zero line in Fig. 3 or consider its transformation into log-scale
The graphs have been changed according to the suggestion of the reviewer (zero line included and transformation into log-scale)




14993 L9: There is high microbial biomass but low litter mass loss; can it be distinguished if the microbial biomass comes mainly on expense of the litter leachate or is there some contribution of the priming effect of the leachate?
as original carbon contents are very low in the original soil samples, we do not think that priming plays a major role in biomass formation in this study (which is indeed in contrast to what might be observed at well developed ecosystems with higher carbon contents in soil)

14994 L19: rephrase, the amount of applied litter can not change
has been changed to “During the experimental period of 30 weeks, a significant portion of the applied plant litter of *L. corniculatus* as well as *C. epigejos* had been degraded”

14995 L12-L13: unclear, rephrase 1
has been rephrased to “Consequently during the first four weeks of incubation the fast degradation rates of *L. corniculatus* plant litter might be linked to large amounts of water soluble plant litter components, rich in nitrogen content. Those compounds could be used by

microbes colonising the litter material to increase their activity and biomass (Aneja et al., 2006; Hopkins et al., 2007; Poll et al., 2008)"

4995 L25-L26: how much water soluble material was there in your litter
We did not measure the amount of WEOC in our initial plant material. However WOEC development is a highly dynamic process and is strongly linked to microbial activities, thus to answer this question not a simple value but a in depth time series on WEOC in litter material during degradation would have been needed.

14997 L25: explain what does "sustainable" mean here Fig 3. Caption: delete "were" after "are
deleted

**Reviewer 2:**

The only remark I have are the potential effects of the two different pioneering plants on the composition of microbial community in the initial (nutrients poor) soil ecosystems which should be discussed. From that aspect the two plants have been well chosen (Fabaceae vs. Poaceae)
Thankx for the praise related to our experimental design, which has been indeed developed in a way to compare plants with two contrasting strategies for nutrient uptake and consequently different C/N ratios in the litter. We made this a little bit clearer now in the introduction. We also pointed out that both plant species typically occur at postmining sites. If and how the obtained data can be transferred to other plants of the same plant family was beyond the scope of this study, which was more focused on the response of "young" soils to different substrates, than to a generalization of response pattern to other plant species.
"As these two plant species belong to two different plant families (Fabaceae respectively Poaceae) with different acquiring strategies for nutrients, differences in litter quality have been well described. Furthermore both plant species have been detected as dominant members of the plant communities in post-mining areas (Pawlowska et al., 1997; Süß et al., 2004; Gerwin et al., 2009)".

Specific comments:
Additional information on soil substrate characteristics would be beneficial. Soil nutrient status (e.g. available N, P, K), more detailed soil texture (% of sand, silt, clay), carbonates (as soil pH is relatively high), soil water status during experiment (range, constant or fluctuating conditions?).
The related data has been included in the revised version: Soil texture was characterised as sands to loamy sands (sand 85 % silt 9 % clay 6 %). Soil nutrient contents (e.g. for available nitrogen and phosphorous) were below or close to the detection limit (< 0.01 µg/g soil).

Soil carbonates were measured in another sampling campaign and were in the range of 0.2 %. As the data is not directly linked to our study, we did not include this value.

The changing water conditions in soil are indeed of prime importance for the interpretation of the data. Thus figure S1 has been already included in the original submission. We linked the humidity levels in soil now to the % of the max. water holding capacity (which was around 20 %), which makes more clear that in autumn the conditions in soil were quite unfavorable for microbial activities. "Mainly at the end of the incubation period (starting 21 weeks after addition of the plant litter material) temperature dropped significantly and soil moisture contents were lower than 30 % of the maximum water holding capacity. These …"

Typing errors:
14983 / 24: of highly important
has been changed to “of high importance”

14984 / 26 considerd
corrected

- 14986 / 18 Soil texture
“texture” has been added

14992 / 5 significant degradation rates (improve sentence)
Both plant litter types were degraded during the experimental period of 30-week (Fig. 2); faster degradation rates were observed for litter material of *L. corniculatus* over the whole experimental period

14992 / 8 undegraded degraded
„degraded“ has been deleted

14998 / 12 incoperation
corrected

**Editor**

Both reviews clearly indicate that the study is original with interesting results and use of appropriate methodologies. Referee 1 requires the authors’ to explain variable de-composition rates of Calamagrostis and Lotus.
has been added (see response to reviewer 1)

Furthermore, the author should demonstrate if leaching of FA from litter may occur that could explain high levels of FA 18:3 and 18.2w6,2.
has been added (see respone to reviewer 1)

The English needs improvement. I suggest that the author consult a native speaker to fix the English.
The English has been rechecked by our team assistant (Tanja Smith) who is a native English speaker and works in addition to her job at our center as a professional translator. Minor changes were made wherever needed

Furthermore, there are very few references on litter decomposition and biogeochemical cycles drawn from restoration ecology. The issue of how microbial activity commences in an “abiotic substrate” is a central question to restoration ecology and could aid in formulating a more precise hypothesis.
We like your idea linking the issue more close to restoration ecology. Therefore the issue of how microbial activity commences in an “abiotic substrate” is a central question to restoration ecology” as it links the study also more to practical questions. Therefore we included this issue in the last paragraph of the introduction.

Some methods need further information to ensure reproducibility by either adding references with accurate and detailed description or by adding supplemental materials/appendices.
Has been added (see below)

Detailed comments:
Page 14986 Lines 10-12: Statement of expectation in the introduction is ambiguous: for example "the amount of N derived from plant litter highly influences the performance of the litter degrading microbial biomass" Can you be more specific how the influence will be and what you mean by performance?
has been changed to: "We postulated that due to the initial nutrient-poor substrate that was associated with a low abundance litter decomposers, the amount of N derived from plant litter highly influences the abundance and activity of the microbes involved in litter degradation resulting in a much faster colonization and degradation of the litter derived from *L. corniculatus"*.

Lines 18-19: Texture information of the substrate used would be very useful to make the experiment reproducible. Also, indication of what type of clay would help.
Texture data has been added to the Material and Methods section; data on the clay type is unfortunately not available for the particular side

Lines 22-25: Refer to a publication in which technical details of the tent method are outlined.
Gschwendtner et al. (2011) has been added

Page 14988 Lines 7-9: Add reference concerning light fraction analysis.
Esperschütz et al. (2011) has been added.

Pages 14990-14991 Shouldn't you add a reference concerning the calculations – this is not the first time that for example delta 13 C is being defined.
Esperschütz et al. (2011) has been added

Page 14998 Lines 10-15: The presentation of the conclusion is not compelling. It starts with a statement confirming what other studies have already shown followed by indicating that further research is needed. I suggest you go through the points you make and start with a positive message of what you have achieved and ending with a suggestion of what should be studied (and how) next.
We thank the editor for this comment, as indeed an improvement and a focus on the major outcomes was helpful. We therefore deleted the link to former studies and the role of fungal communities during litter degradation. We also added a new idea of experiments needed for the future. "If this lack of nitrogen and probably also other nutrients, which obviously highly impacts transformation rates of litter material and therefore the generation of stable carbon pools in soil during ecosystem development, can be simply substituted e.g. by fertilization of the plots or by active planting of plants with a very low C/N ratio (e.g. legumes) remains an open question, which needs to be addressed in future research, mainly to transfer this knowledge to the development of practical application for soil restoration".

I have attached a pdf file with an annotated introduction, in which I tried to improve the presentation.
The respective sentence has been changed to "Therefore it has been postulated that mainly the second phase of litter degradation is more depending on the structure and activity of soil microbes" in the revised version