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Interactive comment on "Impact of the addition of different plant residues on carbon–nitrogen content and nitrogen mineralization–immobilization turnover in a soil incubated under laboratory conditions" *by* M. K. Abbasi et al.

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Sr # Referee's Query Reply Anonymous Referee #1 01 The aim of paper is clear, but authors should address following i) ii)..., their objectives. The aims of the study have been re-organized as suggested on Page 4 Lines 5-9. 02 Number of soil samples. This data should be clearly indicate in the methodology. Also, more especifications about soil sampling and treatment are needed. Section 2.1 of Materials and methods

(Soil sampling) has been re-written as suggested. Description of site and sampling methodology is explained properly. Please see page 4 Lines 19-23. Treatments section is re-written in 2.3. Laboratory incubation Page 5 Lines 23-25. 03 Statistical analysis. Did author contrast the homoscedasticity (e.g, using Levene's test) of data prior ANOVA? If so, authors should address it in the text. The homoscedasticity e.g, using Levene's test was not done. The statistical analysis performed were in accordance to our previous incubation studies. Please see Ecological Engineering 39 (2012) 123–132; Chemosphere 82 (2011) 1629–1635; Soil Sci. Soc. Am. J. 77:558–567 (2013). 04 The authors use several abbreviation for the data. Sometimes it is difficult to follow the text fluency and to remember what is the meaning of each of them The abbreviations in most of the cases have been omitted as suggested. 05 –Conclusions Please, avoid abbreviations in this section; it makes it hard to achieve what is really relevant in the study. The authors should rewrite the conclusions more concisely, for instance, following the scheme i), ii), iii),... The abbreviation have been replaced by full names. The conclusion in its present form looks fine and numbering will not be appropriate in my openion. Anonymous Referee #2 General comments 01 The experimental design could had been improved, since all plant residues where added to the same soil complex sample and previous crop history of the soil had not been taken into account Previous crop history has been added on page 4 Lines 15-20. The statistical analysis performed here is in accordance to our previous incubation studies with different added materials. Please see Ecological Engineering 39 (2012) 123– 132; Chemosphere 82 (2011) 1629–1635; Soil Sci. Soc. Am. J. 77:558–567 (2013). 02 Plant residues quantity and quality can condition soil microbial community composition. Soil microbial functional capacity to degrade or consume more efficiently plant material which had been previously present in this soil should be considered in the interpretation of the results and conclusions. Soil microbial community composition could be highly different between Zea mays and Trifolium repens soil samples, so the origin of soil samples is important The original soil characteristics have been added by incorporating Table 1. The soil used in the study was barren at the time of sampling and previously maize

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and wheat had been grown. This has been added in the revised draft. Uniform soil is used so there will be no difference in microbial composition in the original soil. It will be changed after adding plant residues that had affected the C and N contents and N mineralization. If the reviewer is interested to study microbial community composition, it is not possible at this stge. Specific comments 01 Material and methods Pag 3055, lines 23-24: Data of soil analyses should be on result section. Soil analysis data have now shown separately in Table 1. 02 Pag 3056. Lines 1-5: the plant species names should be named the first time in the material and methods with the full name, that means: Zea mays L. instead Z.mays. I suggest making a diagram summarizing different experimental treatments and abbreviations that will be use for each one. Referee 1 also suggested to avoid Abbreviation. Abbreviations have been replaced by full names in most of the cases. The treatments description has been re-arranged on Page 5 Line 23-25 . Hopefully it is fine and understandable now. 03 Result and Discussion Pag 3058, lines 1-20: I think that authors should summarize this part, since data are exposed in the table and focus more thoroughly this section in the origin of the differences in plant residues quality This part has been shortened from 415 words to 315 words as suggested. 04 Pag 3059, section 3.2 Nitrogen mineralization. Similar to previous paragraph, the enumeration of the results is hard to follow, due to the lack of a clear standpoint of the results. Considering mineralization data are showed in table 2, I think that result should enumerate or comment following always the same order and perspective. For example, compare mineralization result depending on plant component (root, leaves, etc) or the type of plant species (leguminous/non-leguminous). The section is re-written accordingly 05 Pag 3059-3060: Section 3.3. Net cumulative mineralization. I'm a bit worried about this estimation. I've consult Sistani et al.,2008 manuscript where "Cumulative litter-derived inorganic N for each soil was calculated by subtracting the inorganic N of the-un-amended control and initial litter inorganic N content from amended soils at each sampling time" and I think that this estimation could result too simple and could lead to get confused conclusions. The author should take into account N-Biomass which reflects would reflects part of N from plant residues

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degradation and is probably related to NCMN negative data. In addition, the fact to consider that inorganic-N from soil-organic N will be the same in the control sample that in the samples amended with litter is in my opinion uncertain, since microbial activity (C and N mineralization) will be different when fresh organic matter is added to the soil, even soil microbial population stimulated could be different at each treatment. I understand that this estimation gives us an idea of N-dynamic, but in my opinion authors should explain better the calculation and the meaning of this data In the absence of 15N analysis, this is the only way to estimate N release from added materials. The most relevant paper described this method is written by T.S. Griffin, Z. He & C.W. Honeycutt in Plant and Soil (2005) 273: 29–38. On the basis of these calculation we have already published number of manuscripts in high quality Journals e.g. Ecological Engineering 39 (2012) 123– 132; Communications in Soil Science and Plant Analysis, 38: 1691–1711, 2007. 06 Conclusions Pag 3064-3065: Please simplify the conclusions focusing on the most relevant information related to the objective of the work, trying to avoid the repetition of the result enumeration Conclusion is re-written as suggested 07 Figures and tables Figure 3. Caption: check caption of 3.a This is “Mineralization trend of added plant residues across timings (3a)”. It is fine

We believe that the manuscript after series of evaluation is modified according to the suggestion of the Referees and it is now in improved shape. We expect that the submitted manuscript will now be accepted for Publication in “Solid Erath”. The title of the manuscript is modified according to the contents of the Manuscript.

Thanking in anticipation

Regards

Prof Dr M Klaeem ABBASI

Please also note the supplement to this comment:
http://www.solid-earth-discuss.net/6/C1343/2014/sed-6-C1343-2014-supplement.pdf

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