

## ***Interactive comment on “Soil organic carbon dynamics following afforestation in the Loess Plateau of China” by N. Lu et al.***

**Anonymous Referee #2**

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General: The manuscript discussed a hot topic: soil organic carbon dynamics. The authors did an excellent job to use a chronosequence sampling method accompanied with a modeling approach to examine the temporal dynamics of SOC following afforestation of former arable land for 6 sites in the Loess Plateau. The results showed that SOC decreased in the first few years at the wetter sites but increased to levels higher than before afforestation after 25 to 30 yr. The accumulation rates of SOC were 1.58 to 6.22% yr<sup>-1</sup> in the upper 20 cm of soil. Plus, the simulations by the Yasso07 model reproduced the basic characteristics of measured SOC dynamics, suggesting that litter input and climatic factors were the major causes. The study got plenty of valuable data, including SOC by both direct measurement and modeling, the tree data and the biomasses of fine root and forest floor vegetation. The results are interesting and have been fully discussed in the manuscript. I think the study is innovative and

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worthy of publishing. However, the data were not shown in a proper way and there are still some aspects needing further discussion, so I think the manuscript need a major revision.

Comments: 1. In my opinion, SOC dynamic models should be validated by the data from at least one local long-term observation site (E.g. Lugato et al., 2010; Xu et al., 2011). I'm not sure about the modeling results since the absence of the details of the Yasso07 model in the present study. Considering SOC simulations by the Yasso07 model is a major part of the study, the model should be described in detail instead of just citing former researches. For instance, did the Yasso07 model need soil data as input? If so, soil data input to the model were obtained from the closest met-station or directly from soil sampling?

2. The descriptive information of soil and biomass sampling, such as plantation age, should be listed in an extra table for a direct view.

3. The author should check the data carefully. For instance, there is a plot locating between 10 and 20 years in Site 1 of Fig. 4, but missing in Fig. 5.

4. I am not sure that whether “the linear regressions are  $R^2 \geq 0.96$  at  $p < 0.05$ ” for all the sites in Fig. 5.

5. Litter input and climatic factors explained 88–96% of the variations in annual SOC changes at the soil depths of 0–20 cm. The ratio seems to be overestimated and the authors should make sure the data are reliable.

6. Explanations should be stated that Why “Figures are not shown for sites 5 and 6” in Fig. 5 and “Data is not available for sites 5 and 6” in Fig. 8.

7. Fig. 2, 4, 5, 6, 7 and 8 should be shown with legend instead of text in note.

8. Page 11193, line 11 “The trend of carbon accumulation in the soil was the result from” should be “The trend of carbon accumulation in the soil resulted from”

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Lugato, E., Zuliani, M., Alberti, G., Delle Vedove, G., Gioli, B., Miglietta, F., Peressotti, A., 2010. Application of DNDC biogeochemistry model to estimate greenhouse gas emissions from Italian agricultural areas at high spatial resolution. *Agriculture Ecosystems & Environment* 139(4), 546-556. Xu, S.X., Shi, X.Z., Zhao, Y.C., Yu, D.S., Wang, S.H., Zhang, L.M., Li, C.S., Tan, M.Z., 2011. Modeling Carbon Dynamics in Paddy Soils in Jiangsu Province of China with Soil Databases Differing in Spatial Resolution. *Pedosphere* 21(6), 696-705.

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Interactive comment on *Biogeosciences Discuss.*, 10, 11181, 2013.

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