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Biogeosciences Discussions

Interactive comment on "Modeling impacts of management alternatives on soil carbon storage of farmland in Northwest China" *by* F. Zhang et al.

Anonymous Referee #3

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General comments: This paper focuses on the validation of DNDC model on the Soil Organic Carbon (SOC) storage in agricultural soils in Shaanxi Province, China, and the impacts of management alternatives are discussed on the sequestration of SOC at regional scale.

It definitely discussed SOC, one of the most important issues for global warming in agricultural fields, as IPCC pointed out. And it also discussed how to sequester the atmospheric carbon in agricultural soils, to propose the possible alternatives in agricultural management. In conclusion, the quantitative estimate for sequestration of SOC was made, by the incorporation of crop residues and farmyard manure for Shaanxi province, China. Hence, the application of the DNDC model could be a useful tool, for the future estimate of the storage of SOC in any other region. And this manuscript will

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be very attractive and useful for researchers and policy makers in the world.

Individual comment/Question: 1. In page 414, the authors clearly described that "DNDC simulates SOC dynamics by tracking the turnover of four SOC pools, namely plant residue (or litter), microbial biomass, humads (or active humus), and passive humus". In addition, farmyard manure and crop residues are used as the important sources of SOC in this manuscript. Thus, it is very important to understand how these four organic carbons are independently decomposed during 10 or 15 years, for baseline scenario or for the alternative scenarios (by the input of crop residues and/or farmyard manure to agro-ecosystems). The authors are expected to show a graph of the time-trends of decomposition of these different types of SOC for a typical case, including added organic carbons as farmyard manure and crop residues in upland crops and rice paddy fields. Then, we can understand clearly what happens in soils for decomposition and storage of SOC.

2. As one of the key points, the detailed information is needed, on the amount of crop residue (or litter) and/or farmyard manure which was incorporated to the upland and/or rice paddy fields, especially in the section of DNDC model validation.

3. Decomposition rate of organic carbon and production rate of CO2 and CH4 in rice paddy fields depends on not only climate and soil conditions but also water management, whether water is continually flooded or irrigated. More detailed explanation is needed on water management for rice paddy fields. And it is also better to describe how many amounts of rice straws were incorporated after harvest.

4. We read this manuscript with a little bit confusion, possibly due to the order of the content. My understanding is that the section of 2.1 (DNDC model validation) and of 2.2 (Sensitivity tests) are considered to be included in the part of 3. (Results), because the subject matter of these two parts are part of the results for this manuscript.

5. This manuscript does not estimate the total emission of greenhouse gases (GHGs), while the other GHGs, such as methane (CH4) and nitrous oxide (N2O), are very im-

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portant in agro-ecosystems. It is well known that CH4 is emitted from rice paddy fields, and crop residues and farmyard manure are the important sources of N2O emitted from agricultural soils to the atmosphere. The final target to determine the possible mitigation options is, to reduce the total emission of all the GHGs from agricultural fields. The incorporation of rice straws, one of the crop residues, to rice fields after harvest is well known to increase the CH4 emission in the following rice-growing season. In addition, the incorporation of organic manure and/or crop residues could increase the N2O emission. The authors are expected to discuss on this point even shortly in this manuscript, because the DNDC model can easily calculate CH4 and N2O emissions. Or in the second paper following this manuscript, the authors are requested to discuss in the near future, if the total emission of GHGs increases or decreases, in case of input of crop residues or farmyard manure, even their input results in the increase of SOC in soils as the DNDC model can estimate, and in increase in soil fertility.

6. Case 1: There is no description how many amounts of litter/crop residue incorporated into the soil, although it is an important source controlling the storage of SOC, as well as manure application.

7. Case 2: There is also no description how many amounts of litter/crop residue incorporated into the soil, while a large amount of crop litter/residues is produced from corn production, and they are included in the calculation by the DNDC model. How is the contribution of crop litter/residues to the SOC dynamics, compared to that of farmyard manure.

8. Case 3: There is no description how is water management in rice paddy fields?

9. Fig. 4: After 40 years, SOC content was constant. The authors are expected to explain clearly the reason why it reached an equilibrium between decomposition/consumption and input/accumulation of SOC.

10. Case 4: There is also no description on water management and an application amount of rice straws in local farming management, as well as in Case 3.

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11. In contrast to case 3, case 4 showed an increase in SOC content in double-rice field through all the period, according to the description in the text. The authors are expected to explain the quantitative difference between case 3 and case 4. The authors are also requested to explain any reason why the observational SOC in Fig. 6 has been decreasing since 1998, while the DNDC model could not simulate this decrease.

12. The authors are expected to describe any reason why sensitivity tests were made only for upland crop fields, because rice paddy fields are significantly different in soil condition and decomposition rate of SOC (anaerobic condition) from upland fields (aerobic condition).

13. In page 421, the authors described that DNDC requires four soil properties (1.e., bulk density, SOC content, texture, and pH). How are the four SOC pools, namely plant residue (or litter), microbial biomass, humads (or active humus), and passive humus, estimated for each soil type? And how was the validation of this estimate made, in comparison with any field data?

14. In "Results", the authors are expected to clearly describe how the annual SOC changes for 2000 were calculated, while in the section of 2.3 (Data base construction) and Fig. 9, all the data were shown only for one year of 2000.

15. It is not reasonable that, in the base line scenario, the annual SOC loss (Fig. 10) is higher in southern region where the rice paddy fields are located, than in northern region, because the rice paddy fields sustain much more SOC than upland crop fields, as described in page 425. More quantitative explanation is needed in the section of 3.2 (Impacts of management alternatives).

16. In Table1, average annual temperature at five sites is not correct, and the authors are requested to check and revise those data.

17. Which is the unit of SOC in Table 1 correct, "g kg-1" or "gC kg-1"? And is the SOC data of 6.34 in Jinqu, Zhejiang correct, because a constant SOC in Fig. 4 is 0.02 kgC

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kg-1?

Interactive comment on Biogeosciences Discuss., 3, 409, 2006.