

***Interactive comment on* “Increase in soil organic carbon by agricultural intensification in northern China” by Y. Liao et al.**

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(1) Soil carbon concentrations were measured using two different methods (pp. 16501-16502) that have shown to differ from each other. Most of the samples were analyzed by the potassium dichromate method and these samples were concentrated near the beginning and middle of the 20 year study period. However, soil samples collected towards the end of the 30 year study period were measured by dry combustion. Dry combustion usually yields higher carbon concentrations than wet chemical digestions, and this is documented in perhaps 20-30 different reports in the literature. For example, see Tivet et al. (2012) Soil Sci. Soc. Am. J. 76: 1048-1059; and, Islam (2006) Encyclopedia of Soil Science (R. Lal, ed.), pp. 1164-1167. Does this important methodological difference have any bearing on the rise in SOC concentration and

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density towards the end of the 30 year study period as illustrated in Figure 1 on page 16523 of the manuscript?

[AUTHORs]: We did a pre-experiment to compare the two different methods on the soil used in our study. Our result indicated that SOC contents determined by dry combustion and potassium dichromate titrimetric method are comparable and not significant different within the SOC level 7-12 g kg⁻¹. This meant that the SOC differences due to analytical methods are negligible for the soil in our study (Wang Pan-lei, Qin Feng-qin, Cai Pei, Meng Fan-qiao, Zhang Min, Comparison of Acidification and Soil Organic Carbon Determination for Semihumid Soils in North China, Chinese Journal of Soil Science, 2014, 45(4): 880-887) and will not lead to significant increase of SOC due to different method. However, we agreed with the reviewer that for SOC at high level (<12 g kg⁻¹), this difference should be taken into account.

(2) The authors indicate that bulk density was "interpolated" for apparently all of the soil samples taken during the 1982-2011 study period (page 16503, lines 8-9). I assume this means that there were either none or very few direct measurement of soil bulk density to accompany the soils that were collected for measurement of carbon concentration. Since bulk density has a very large and important impact on the calculation of soil carbon stocks, this strikes me as an important limitation to the value of this data set. Furthermore, bulk density can vary substantially across a landscape in response to soil physical characteristics, organic matter production and decay, land management practices, and variation in these factors through time. So, trying to simply interpolate this very important number could give rise to large and unknowable errors in the estimate of soil carbon stocks (mass per unit area).

[AUTHORs]: Our study aimed to analyze the impacts of agricultural intensification on soil carbon, and the relationship between SOC content and driving factors (climate and farming managements). The SOC content and its relationship with driving factors are not influenced by soil bulk density. The soil bulk density was included in calculation of the change of SOC storage within the past three decades and the minor error of soil

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bulk density will not change the general conclusion of this study. However, we revised the text of Discussion section at line 347-349 to highlight this issue.

Minor comments Page 16499, Line 7: I'm not sure why there should be a tilde () in between the two numbers 1.3 and 21.2. This is also done on Page 16501 Line 14. Should this actually be a dash, or some other symbol?

[AUTHORs]: Tilde has been replaced by dash at Line 50 and 112, and modification has also been done at other relevant lines of the MS.

Page 16499, Line 7: The units “million T C annually” are used in the middle of this line. It might be better to transform this into Teragrams (Tg) of C since you use Tg throughout the remainder of the paper.

[AUTHORs]: This has been revised at Line 50.

Page 16502, Line 4: The term “SOC content” is used here and in many subsequent locations throughout the manuscript. This is a vague term and should be replaced with “SOC concentration”.

[AUTHORs]: We have consulted several scientists on this issue and the responses are different. Some scientist thought that concentration is more appropriate for liquid. The term of “SOC content” or “carbon content” has also been in many papers, for example: Guohan Song, Lianqing Li, Genxing Pan, et al. Topsoil organic carbon storage of China and its loss by cultivation. *Biogeochemistry*, 2005, 74: 47-62; Pat H. Bellamy, Peter J. Loveland, R. Ian Bradley, et al. Carbon losses from all soils across England and Wales 1978–2003. *Nature*, 2005, 437(8): 245-248; Catherine E. Stewart, Keith Paustian, Richard T. Conant, et al. Soil carbon saturation: concept, evidence and evaluation. *Biogeochemistry*, 2007, 86:19-31. So in our MS we kept the expression of “SOC content”.

Interactive comment on Biogeosciences Discuss., 11, 16497, 2014.

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