

## ***Interactive comment on “Comparison between eddy covariance and automatic chamber techniques for measuring net ecosystem exchange of carbon dioxide in cotton and wheat fields” by K. Wang et al.***

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The authors wish to thank the anonymous Referees for valuable comments to improve our manuscript. We have addressed below each of the comments point by point. Whenever the Referee's comments are cited, the text is written inside quotation remarks.

Anonymous Referee 1 “General comments: Although it is unfortunate that AC dataset has long-term data missing periods, and that the authors did not measure energy bal-

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ance closure at the study site for assessing a systematic uncertainty, comparison between EC and AC fluxes of CO<sub>2</sub> exchange on the seasonal (for wheat) and the annual scale (for cotton) is valuable and therefore worth publishing.”

“Specific comments: My only concern is that the discussion on changes in SOC seems to be out of the scope of the present paper, and I wonder this paragraph is unnecessary for this paper. It is generally interesting to compare the obtained annual fluxes with estimates by independent methods, but in the present paper, only insufficient data and information are provided for the comparison. For instance, it is not shown when and how the authors measured the amount of harvested and incorporated biomass (aboveground residue and the roots) of cotton and their carbon contents, and how much their uncertainty ranges. Definition of the conversion efficiency from incorporated residue to soil organic carbon and its uncertainty range are also not clear. Is the efficiency defined for the first year after incorporation? Do we need to multiply the amount of incorporated carbon by the conversion efficiency to compare with the annual fluxes? Further, only a little evidence or information is shown for the discussion in the latter half of the paragraph.”

The authors agree with the referee to delete the discussion on changes in SOC (the paragraph of P8489, L28 ~ P8491, L4). We also realized that some parameters for the comparison might have large uncertainty due to insufficient data and using empirical parameterization. For example, the conversion efficiency from incorporated residue to SOC (11%) was an averaged estimate derived from other field studies based on long-term measurements in the North China Plain region.

“Technical comments:” “1) P8476, L21-23. Further explanation is needed how to estimate the detection limits.” We will explain more in the revised manuscript about estimating the detection limit.

“2) P8477, L2-4. F should be Fac ” Revised.

“3) P8477, L6. Is rho really CO<sub>2</sub> density?” The chamber fluxes (Fac) had a unit of mg C

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$\text{m}^{-2} \text{s}^{-1}$ . Therefore,  $\rho$  was the  $\text{CO}_2$  density (in  $\text{g C m}^{-3}$ ) under standard atmospheric conditions. We have revised it. "4) P8478, L2-3. How big was the coefficient  $c$ ?"

The coefficient  $c$  ranged from 0.45 to 0.79, with mean value of 0.61. We will put these data in the revised manuscript.

"5) P8487, L21. "(Table 1)" should be removed." Revised.

Anonymous Referee 2 "Minor comments: P8490 – I would recommend the authors to include a description on how DSOC was estimated in the material and methods section rather than in the discussion section. In addition, how was the C content of the harvested measured? Can the authors provide an estimate of the uncertainty in the increase in soil organic C stock?"

Before the cotton residue was incorporated into soil at the end of growing season, the amount of the residue (both aboveground and belowground) was measured using the method described in P8480, L6~12. The C content of the residue, as well as the cotton fiber collected during the entire measurement period, were measured in the laboratory using the dry burning method. However in the revised manuscript we would like to remove the discussion part on changes in soil organic carbon. See also the comment of Referee 1 and our response.

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