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Interactive comment on "Flux measurements in the near surface layer over a non-uniform crop surface in China" *by* Z. Gao et al.

Z. Gao et al.

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The authors greatly thank the referee for the positive comments on the article and for the recognition of the importance of the topic. The comments made by the referee, highlighting the manuscript weakness and our carelessness in preparing our article, are now discussed. The original comments in lines begin with a dash.

– The presentation and analysis could be improved to support the conclusions better. Some of the author's claims are not very clearly supported by the presented data and some of the results might be flawed due to the use of a constant heat capacity (same value for wet and dry soil).

We have clarified our presentation and corrected our analysis.

– The alleged strength of the study, i.e., that is performed over a "non-uniform crop surface" could be its weakness at the same time. By lumping together different crop types, their respective responses to climate can not be separated and information is lost in this way. Therefore the results are possibly unique to the particular site (its particular composition of rice, corn, bean and grass, and their arrangement in space together with the prevalent wind direction). It is obvious what is lost by measuring over a mixed crop, but the authors did not make evident what is gained by this measurement setup. It would be helpful if the authors would show the arrangement of the different crops in relation to the measurement tower, the footprint of the measurements, and how the footprint changes in time (if it does).

Because non-uniform cropland surfaces are very common in China now, the main goal of our experiments was to capture the CO_2 flux and energy exchange over such surface rather than to distinctly separate the flux sources. Obviously it is a big challenge because of the surface cover heterogeneities, crop growths and mutative weather conditions. We have added a figure (Figure 1) to show the arrangement of the difference crops in relation to the measurements mast. The footprint should change in time, but because it is difficult to determine the time series of the zero plane displacement, we fail to give the time series of the footprint.

– Addressing some of my points may require a major revision of the manuscript, but the potential outcomes are significant enough to make this undertaking worthwhile in my opinion.

Yes, we have addressed these comments and significantly improved our article. These comments are important and appreciated.

– Points 1 and 4 suggest that crop height did not significantly influence the energy partitioning of the system. This is an important outcome, as one would intuitively expect that the Bowen ratio would decrease as Leaf area index (LAI) Increases. However, it is difficult to confirm this finding from figures alone, so it would be helpful to derive some meaningful energy balance ratios (like the Bowen ratio) from the data and plot them against LAI or time if LAI values are not available.

We greatly appreciate for this comment. We added a figure (Figure 9) of Bowen ratio and other flux ratios to confirm our finding.

– Points 2 and 5 are not obvious from the presented data. Fig. 4 shows a major increase in CO_2 uptake between DOY 175 and 182, but after DOY 182 there does not seem to be a further increasing tendency in CO_2 uptake. Between DOY 175 and 180 the most significant rainfall events occur, so that the increase could support statement 2, but statement 5 could only be confirmed if a significant upwards trend after DOY 182 was observed. However even the finding that CO_2 uptake increases after rainfall events is an important outcome, as 60 of the area was covered by a rice field, which was flooded throughout the measurement. It would be interesting to find out whether the increase of CO_2 uptake after rainfall could have been sustained by the non-flooded crops alone or if the rice field had to contribute to this increase as well. Therefore it would be important to know the footprints of the measurements and the location of the different crops within these footprints. In the fetch analysis (chapter 3.1), it is only mentioned that "approximately 90 of the measured flux at the measurement height was expected to come from within the nearest 600m of upwind area for neutral stability during the entire period", and in chapter 2.1 the authors mention that "the predominant wind direction was south-east during the period of the experiment". In the same chapter they also mention that "the site surface was non-uniform, and consisted of grass (10), bean (15), corn (15), and rice (60)", but they do not comment on the spatial distribution of these crops relative to the footprint of the measurement, so that it can not necessarily be assumed that the above distribution reflects the distribution of crops contributing to the measurements. Furthermore, from the given information alone it can not be excluded that a short-term change of wind direction after rainfalls led to a different footprint and potentially different composition of crops contributing to the measurement, which would make the conclusion that CO_2 uptake increases after rainfall unjustified.

We have accepted these comments in our revision process. Figure 1 was added to illustrate the spatial distribution of surface cover. The conclusion has been clarified, and the presentation and analysis have been improved to support our conclusion.

– Point 3 could be a very important finding, but needs further elaboration. The authors explain the energy imbalance with “warm or cold rainwater infiltrating into soil". If heat transport due to rainfall falling on the soil is significant, the common formulation for the energy balance would have to be re-formulated to account for this effect. However, the authors mentioned that no measurements could be performed during rainfall, so the energy balance is calculated after the rainfall only, when infiltration has already happened. Thus the 'missing energy' can only be due to water fluxes within the soil, which are not accounted for by equation (4), or due to a change of the soil heat capacity. In fact, the authors assume a constant volumetric heat capacity of soil (2.42*106 J m-3 K-1). This will result in the largest error when soil moisture changes the most, e.g. just after rainfalls. The change of heat capacity due to a change in soil moisture could be easily accounted for, as the heat capacity of water is known, so the authors should try to remove this error and test whether the energy balance gets improved.

We accepted these comments and corrected our article corresponding. The description of method to calculate soil heat capacity was corrected, and we re-calculated the soil heat flux also. Our presentation about calculation of soil heat flux is wrong, but soil heat capacity was correctly calculated in our original analysis. In this way, the results were not changed much. Now we have clarified our presentation.

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