Dear reviewer Seth Spawn,

We thank you for providing the insightful and constructive comments. We carefully edited the paper according to these comments and suggestions. We hope the revised version of the manuscript is to your satisfaction, and of course, we are more than happy to improve the manuscript if new comments and suggestions might arise.

Reviewer: 2

The authors present a 10-year time-series of eddy covariance-derived NEE from a representative (wheat-maize double-cropped annual rotation) cropping system in the North China Plain. They find their system to be a net CO2 sink (negative NEE) but also that the strength of this sink has progressively declined throughout their observational record. Disproportionate increases in ecosystem respiration relative to gross primary production appear to be responsible for this trend and, interestingly, the authors assert that – at least during the maize season – changes in water table depth and shortwave radiation (not air temperature) are the proximate drivers of change. In addition, the authors embark to further partition ecosystem respiration into its autotrophic and both above- and below-ground heterotrophic components by coupling eddy covariance measurements to a concurrent year's worth of daily, in situ soil respiration measurements. While the authors demonstrate that such partitioning can be done successfully, results are presented in site specific manner and it's unclear to me whether they reveal anything that can be generalized to other sites. Finally, the authors compare their eddy covariance derived C balance to biometric proxies and concurrent changes in soil organic carbon concentrations.

Response

We appreciate the reviewer for the constructive comments. The research is indeed carried out at site level. Given the site we selected is representative over the North China Plain in terms of cropping style and tillage management etc, we are of the opinion that the site-specific research of this study can represent the general carbon characteristics over the winter wheat/summer maize cropping system over the North China Plain. We added the representativeness of this study by incorporating the reviewer's other comments, please also see our response to the detailed comments.

I feel that either story could be a valuable contribution to the literature but, at present, the potential of neither is fully realized. The former narrative (weakening sink) aligns with hypothesized metabolic responses to climate warming, though interestingly the authors suggest that temperature may not be the proximate driver – I'd like to see a more thorough assessment of these patterns/drivers and a richer discussion if the authors choose to pursue this narative. Likewise, the latter story – as the authors explain well in their introduction – has great potential to unveil ecological mechanisms that could inform process-based predictions of agroecosystem responses to change. Unfortunately, the study does not seem to dig into this much and thereby does not reveal generalities to that end. This narrative dichotomy is manifest in the manuscript's current structure. The introduction suggests that the focus will be on flux partitioning. By the end, though, the decadal trends – that have no apparent connection to the partitioning exercise – emerge as the dominate discussion. I encourage the authors to choose one narrative, focus all manuscript sections accordingly, and substantially expand the associated discussion. I've provided specific comments and suggestions below that are largely agnostic towards which ever story is ultimately emphasized.

Response

We appreciate the reviewer for the comments.

The current manuscript is a resubmission to BG. Our previous manuscript focused on the second narrative of flux partitioning, but a previous reviewer suggested one year measurement was not sufficient for a paper for BG, so we added the decadal variation of CO_2 flux to the research. That is the reason why we reported 10 years' flux record. The reviewer is right regarding the structure concern, and we realized the structure of the manuscript can be improved. By incorporating your advice, we revised the introduction and rewrote the result section to make the story more consistent. In particular, we balanced the contents between decadal variation and the detailed budget component across the manuscript. Now the manuscript reports the CO_2 fluxes at the inter-annual timescale, then the CO_2 budget components are described for a representative year. We believe the revised manuscript is coherent by incorporating the reviewer's comments.

The revised introduction is pasted here for your convenience:

"**Introduction**

The widely used eddy covariance technique (Aubinet et al., 2000; Baldocchi et al., 2001; Falge et al., 2002b) has enabled us to better understand the terrestrial CO2 exchange with the atmosphere, thereby forested our understanding of the mechanisms on how the terrestrial ecosystems contribute to mitigate the climate change (Falkowski et al., 2000; Gray et al., 2014; Poulter et al., 2014; Forkel et al., 2016). Agro-ecosystems play an important role in regulating the global carbon balance (Lal, 2001; Bondeau et al., 2007; Özdoğan, 2011; Taylor et al., 2013; Gray et al., 2014) and have great potentials to mitigate global carbon emissions through cropland management (Sauerbeck, 2001; Freibauer et al., 2004; Smith, 2004; Hutchinson et al., 2007; van Wesemael et al., 2010; Ciais et al., 2011; Schmidt et al., 2012; Torres et al., 2015), some studies further proposed the agroe-cosystems as the "natural climate solutions" to mitigate global carbon emission (e.g., Griscom et al., 2017; Fargione et al., 2018). The field management practices (e.g., irrigation, fertilization and residue removal, etc.) impact the cropland CO2 budget (Baker and Griffis, 2005; Béziat et al., 2009; Ceschia et al., 2010; Eugster et al., 2010; Soni et al., 2013; Drewniak et al., 2015; de la Motte et al., 2016; Hunt et al., 2016; Vick et al., 2016), but their relative importance in determining the cropland CO2 budget remain unclear because of limited field observations (Kutsch et al., 2010), prompting the interest on comprehensive CO2 budget assessments across different cropland management styles.

Over the past two decades, CO2 evaluations of agro-ecosystems have mainly focused on the variations in the integrated ecosystem exchange with the atmosphere (i.e., NEE) or its two derived components (i.e., GPP and ER) using the eddy covariance. To date, these evaluations have been conducted for wheat (Gilmanov et al., 2003; Anthoni et al., 2004a; Moureaux et al., 2008; Béziat et al., 2009; Vick et al., 2016), maize (Verma et al., 2005), sugar beet (Aubinet et al., 2000; Moureaux et al., 2006), potato (Anthoni et al., 2004b; Fleisher et al., 2008), soybean-maize rotation cropland (Gilmanov et al., 2003; Hollinger et al., 2005; Suyker et al., 2005; Verma et al., 2005; Grant et al., 2007), and winter wheat-summer maize cropland (Zhang et al., 2008; Lei and Yang, 2010). The long-term variations of the cropland CO2 fluxes remain limited, leaving our knowledge of the future potential of cropland as the climate mitigation tool incomplete.

The eddy covariance-derived CO2 fluxes of NEE, GPP and ER only report the integrated fluxes, but cannot provide the detailed CO2 budget components, which consist of carbon assimilation (i.e., GPP), soil heterotrophic respiration (RH), above-ground autotrophic respiration (RAA), below-ground autotrophic respiration (RAB), lateral carbon export at harvest and import at sowing or through organic fertilization (Ceschia et al., 2010). These different CO2 components result from different biological and biophysical processes (Moureaux et al., 2008) that may respond differently to climatic conditions, environmental factors and management strategies (Ekblad et al., 2005; Zhang et al., 2013). Differentiating among these components is a prerequisite for understanding the response of terrestrial ecosystems to changing environment (Heimann and Reichstein, 2008), so the carbon budget evaluations have been reported for a few croplands (e.g., Moureaux et al., 2008; Ceschia et al., 2010; Wang et al., 2015; Demyan et al., 2016; Gao et al., 2017). In particular, to account for the literal carbon export, the Net Biome Productivity (NBP) is often obtained by combining the eddy covariance technique and field carbon measurements associated with harvest and residue treatment (Ceschia et al., 2010; Kutsch et al., 2010). As detailed CO2 budget might facilitate better prediction of agro-ecosystems' responses to climate change, the CO2 budget evaluations in different croplands remain necessary.

The North China Plain (NCP) is one of the most important food production regions in China, and it guarantees the national food security by providing more than 50% and 33% of the nation's wheat and maize, respectively (Kendy et al., 2003). Irrigation is a common to alleviate water stress during sprint drought in the NCP. Diverting water from the Yellow River for irrigation results in a shallow groundwater depth (range from 2 to 4 m) along the Yellow River (Cao et al., 2016) (Fig. 1). Wang et al. (2015) suggested that a groundwater-fed croplands in the piedmont plain of Mount Taihang (Luancheng site in Fig. 1) were losing carbon, and other studies also reported that the cropland in this region was carbon sources (Li et al., 2006; Luo et al., 2008). However, the long-term variations (e.g., >10 years) of the CO2 fluxes over the NCP remains lacking, leaving the trend of carbon uptake capacity of this region unknow.

To this end, this study is designed to assess the long-term variation of CO2 fluxes and its budget of the representative wheat-maize rotation cropland in the NCP. The eddy covariance system was used to measure the CO2 exchange from 2005 through 2016. For the full 2010-2011 agricultural cycle, we measured soil respiration and sampled crops to quantify the detailed CO2 budget components. These measurements (1) investigate the long-term CO2 flux (NEE, GPP, and ER) trend over this cropland; (2) provide the detailed CO2 budget components; and (3) estimate the Net Primary Productivity (NPP), Net Ecosystem Productivity (NEP), and Net Biome Productivity (NBP)."

Specific Comments:

Line 17: Here and throughout (e.g. lines 110, 112, 122, etc.), it's not clear what "typical" means. I'd suggestion changing to something like "representative" and defining in a sentence or too (the definition can be provided in the main text and doesn't need to occupy space in the abstract).

Response

The reviewer is right, we modified it to representative.

Line 27: Here and throughout, "cultural" should be changed to "agricultural".

Response

Revised.

Lines 36-38: There is no discussion in the body of the text about the management implications of a more detailed understanding of the CO2 budget. I recommend that this concluding sentence be changed to better reflect what is actually discussed in the manuscript.

Response

We modified the concluding sentence to:

"The investigations of the temporal variation of CO_2 fluxes and its budget components of this study reveal the importance of temperature and groundwater depth in controlling the CO_2 fluxes."

Lines 41-54: I recommend framing these opening sentences less as though interest in terrestrial C-cycle's role in the climate system is new but instead that the advent of the eddy covariance method has changed the way we study it. People have long recognized and studied land-atmosphere C fluxes (Houghton et al (1983) is an early example but by no means the first or only one). Reframing in this way would then smoothly transition to your accurate assertion that the growing number of eddy flux studies further necessitate a mechanistic understanding of the processes that underly the integrative fluxes measured by the eddy system.

Response

We appreciate the reviewer for the comment. We revised it accordingly.

We open the paragraph now by “The widely used eddy covariance technique (Aubinet et al., 2000; Baldocchi et al., 2001; Falge et al., 2002b) has enabled us to better understand the terrestrial CO2 exchange with the atmosphere, thereby forested our understanding of the mechanisms on how the terrestrial ecosystems contribute to mitigate the climate change (Falkowski et al., 2000; Gray et al., 2014; Poulter et al., 2014; Forkel et al., 2016).

Line 42: Gray et al (2014) may also be a good reference here as it directly addresses the role of agriculture as a diver of variation in the global C cycle.

Response

We appreciate the reviewer for the paper recommendation. It is incorporated.

Line 67: Gray et al (2014) may also be a good reference here.

Response

We appreciate the reviewer for the paper recommendation. It is incorporated.

Lines 68-70: You might consider mentioning some of the emerging “natural climate solutions” literature. Griscom et al (2017) show that agroecosystems have a large potential to mitigate C emissions, globally. Fargione et al (2018) further show that agroecosystems can be a particularly cost-effective means of mitigating C emissions.

Response

We appreciate the reviewer for the paper recommendation. It is incorporated.

We added the text to mention such effort as “some studies further proposed the agro-ecosystems as the “natural climate solutions” to mitigate global carbon emission (e.g., Griscom et al., 2017; Fargione et al., 2018).”

Lines 73-74: Please change “the key factors” to “their relative importance in”.

Response

Revised.

Lines 92-95: These sentences seem to imply that agroecosystems are monolithic and might collectively be generalized as source or sink with the help of a few more CO2 budgets. Diversity in source/sink behavior among studies is almost surely an artifact of differences in management and edaphics. Instead, I'd suggest emphasizing that detailed budgets might facilitate better prediction of systems' responses to change.

Response

We appreciate the reviewer's constructive comment. It is revised accordingly. The updated text is pasted here for your convenience:

"As detailed CO_2 budget might facilitate better prediction of agro-ecosystems' responses to climate change, the CO_2 budget evaluations in different croplands remain necessary."

Lines 107-111: This study's central question needs to be clarified. Here the question seems to be something like 'how does variation in microclimate and management influence the source/sink status of croplands. This is a question that doesn't seem to necessitate the detailed C budget that distinguishes your study and could instead be inferred from [spatial] patterns of NEE. But back in lines 73-74 the question seems more about the proximate drivers influencing individual C cycle fluxes. Which is it?

Response

We revised the introduction to better clarify the question.

Our aim is to: report the temporal trend of the carbon fluxes over a representative cropland over the North China Plain, and investigate the detailed CO_2 budget components.

In fact, both are the reasons that drive this study, but now we revised the introduction thoroughly to better clarify our question. Please refer to our revised introduction in response to previous comments.

Line 182: Is this a standard gap filling procedure? I'm not familiar. Perhaps add a sentence to the text?

Response

This is a machine learning algorithm, which has been shown to have the capability to fill gaps of eddy covariance data (see Kang et al., 2019).

We modified in to “the machine learning Support Vector Regression (SVR) algorithm (Cristianini and Shave-Taylor, 2000) was used to calculate GPP and ER directly for this period (Kang et al., 2019)”

Line 194: Please change “groundwater table” to “groundwater table depth”.

Response

We modified it to “groundwater depth”, which is also suggested by the first reviewer.

Line 210: Here and throughout the text, “samplings” should be changed to “samples”.

Response

Revised.

Line 224: “guaranteed” is too strong of a term here. It ignores inevitable underlying heterogeneity in soil characteristics.

Response

The reviewer is right. We modified it to “The uniform field condition helps reduce the measurement uncertainty associated with the spatial variability (see Zhang et al., 2013)”.

Line 230: How were parameters “inferred”?

Response

The parameters were inferred by using the least square method. We modified it to “The parameters were inferred by fitting the R_H and T_S measurements by using the least square method (see Zhang et al., 2013)”. See Fig. R1 (figure from Zhang et al. 2013) which is pasted below.

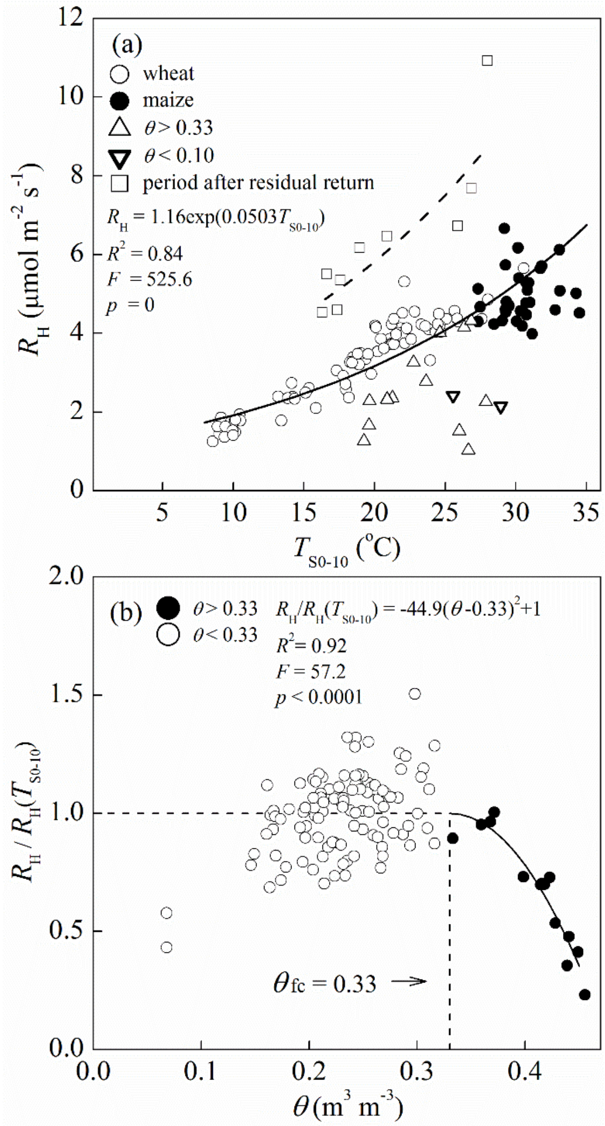

Fig. R1 (a) Relation between heterotrophic respiration (R_H) and soil temperature of the upper 10 cm ($T_{S0\text{–}10}$) (b) relation between temperature-standardized heterotrophic respiration ($R_H/R_H(T_{S0\text{–}10})$) and mean soil water content of the upper 5 cm (θ), vertex of the fitting quadratic curve was set to 1.0 at θ_{fc} . Dashed line in (a) was the fitting temperature dependence curve for the period of 3 weeks following the crop residual return.

Line 233: Please define the “contribution ratio”.

Response

The contribution ratio of R_{AB} to R_S is the ratio R_{AB}/R_S, we revised this to make it clearer as "while R_{AB} of other periods was estimated based on the R_H record and the ratio of the R_{AB} to R_S estimated previously (Zhang et al., 2013), the continuous R_{AB}/R_S ratio was interpolated from the daily records (Fig. 2)"

Lines 237-242: This is a remarkably narrow time period (1-year) within which to measure SOC changes in response to management. I'm highly skeptical a signal will emerge through the inevitable noise of heterogenous soil. Were samples taken at the same location every time? The regression technique used to calculate the rate of change needs to be reported and must account for the variance among samples on each sampling date. Was bulk density measured with each sample? If not, please clarify that these are measurements SOC concentration, not SOC stocks.

Response

This analysis was removed by incorporating the reviewer's other comment. Nevertheless, to respond to all the reviewer's concerns, we sampled soil from 10 fixed locations each time and pooled them before SOC analysis. The soil bulk density is the average value of the soil measurements in this cropland, so we did not measure it for each soil sample. We analyzed the SOC concentration to calculate the SOC stock.

Line 244: You might consider adding a conceptual figure showing the C-cycle as inferred in this study and highlighting the fluxes/drivers of interest.

Response

We appreciate this advice. We actually have the CO_2-cycle in figure 11, a new conceptual figure might be repetitive. We are inclined to reduce the figure count to use figure 11 alone for this purpose.

Lines 277-279: This could be moved to the site description at the beginning of the methods section.

Response

We revised accordingly.

Lines 307-309: Consider reporting these as percentages.

Response

This part is removed when we thoroughly revised the discussion.

Lines 327-328: As I see Figure 8, WT increased wheat NEE (positive coefficient) and decreased GPP (negative coefficient). But you say "decreased GPP, thereby reduce NEE". What am I missing? Also, please elaborate on the maize trends. Currently you say, "WT had a pronounced contribution to both GPP and ER, as well as to NEE." Please provide a more detailed description that includes the directions of changes.

Response

We revised this part.

The confusion is due to the sign of NEE. We adopted the commonly used sign system to use negative NEE as carbon uptake. For wheat, groundwater depth (WD) has positive correlation with NEE, implying the decrease of carbon uptake along with increasing groundwater depth, and we can further find that this result from the decrease of GPP under high WD (Fig. 8a). We modified the expression to "higher WD correlated negatively with GPP, thereby reduced net carbon uptake." to avoid the confusion. We also provided a more detailed description of maize. The updated texts are pasted here for your convenience:

"The NEE, GPP and ER for both wheat and maize were correlated with the three main environmental variables of Rsi, Ta and WD using the multiple regression (see Appendix B for details). In the wheat season, Ta showed its relatively greater importance to all the three CO2 fluxes with a higher Ta increasing both GPP and ER, and also enhancing NEE (more negative) (Fig. 8a), but Rsi showed negligible effect to all the three CO2 fluxes; higher WD correlated negatively with GPP, thereby reduced net carbon uptake. In the maize season, WD had good correlations with all the three fluxes of GPP, ER, and NEE, but Ta showed negligible effect to all the three CO2 fluxes; WD showed relatively greater importance to both GPP and ER, and a deeper WD drove higher net carbon uptake (more negative NEE); Rsi had a good correlation with ER, but a bad correlation with GPP (Fig. 8b), ultimately, higher Rsi in maize season lowered the net carbon uptake (more positive NEE). Overall, Rsi and WD showed its relatively greater importance in influencing the inter-annual variation of maize (Fig. 8b)."

Line 373: There is very little discussion of the flux partitioning work that was so heavily emphasized in the introduction. Why?

Response

By incorporating the reviewer's advice, we thoroughly revised the introduction. In particular, we expanded the introduction to the general CO_2 researches and measurements of cropland. Please also see our introduction pasted in response to previous comments.

Lines 383-384: Similar to my critique of lines 92-95, I don't get the impression that the scientific community is seeking a consensus on whether or not croplands are C sources or sinks. I would remove this assertion. It's well accepted (and demonstrated in the literature) that, like so many ecosystem processes, source/sink status is contingent upon management and landscape heterogeneity across scales and domains.

Response

We agree with the reviewer and removed this expression. We also thoroughly revised the whole introduction.

Line 390: As with all C-cycle assessments, results depend on the system boundaries. Since your results suggest that the sink status of your focal croplands is contingent upon irrigation water, I'd suggest including a brief discussion of the implications that emissions from irrigation pumping might have for the source/sink status of your croplands. Such a discussion may be more appropriately situated in the "Effects of ground water on carbon fluxes" section.

Response

We appreciate the constructive comment.

Our cropland is irrigated by diverting water from the Yellow River, we compared with a nearby cropland with similar cropping system but irrigated by pumping the groundwater (Wang et al., 2015). The updated texts were pasted below for your convenience:

"The groundwater depth at our site is considerably shallow because of the irrigation by water diverted water from the Yellow River, in contrast, the nearby Luancheng site (Wang et al., 2015) is groundwater-fed with a very deep groundwater depth (approximately 42 m) (Shen et al. 2013), and their CO_2 budget components had some difference with our study. Comparing the net CO_2 exchange, the GPP at our site is a little higher than the Luanchen site, implying the irrigation at our site may better sustain the photosynthesis rate for wheat. However, ER at our site is also a little higher than Luancheng site. For maize, both GPP and ER at our site were comparable to Luancheng site, implying that the irrigation method had no discernible effect on the integrated CO_2 fluxes for maize. However, the three components of ER in our study showed pronounced difference from the Luancheng site, where they reported the R_{AA} was 411 gC m^{-2} for wheat and 428 gC m^{-2} for maize, three times the results of our study (128 gC m^{-2} for wheat and 133 gC m^{-2} for maize). However, their R_{AB} for wheat (36 gC m^{-2}) and maize (16 gC m^{-2}) were less than a quarter of our results (136 gC m^{-2} for wheat and 115 gC m^{-2} for maize). Their R_H of wheat (245 gC m^{-2}) was less than our estimate (377 gC m^{-2}), but R_H of maize (397 gC m^{-2}) was greater than our result (292 gC m^{-2}). In general, the crop above-ground parts in our site respired carbon more than the Luancheng site, possibly because the shallow groundwater depth at our site increased the above-ground biomass allocation but lowered the root biomass allocation (Poorter et al., 2012). These independent cross-site comparisons demonstrate that carbon budget components may be subject to the specific cropland management strategies, and even the same crop under similar climatic conditions can behave differently in carbon uptake."

Line 398: These numbers are remarkably precise. Is that true to the precision of your instruments? What is the uncertainty associated with your numbers?

Response

We followed reviewer1's advice and round the data to the nearest whole number. Our NPPs of both wheat and maize were estimated based on two independent methods, and they gave very close estimations. The NPP was 783 (SD$\pm$46) gC m^{-2} for wheat and 562 (SD$\pm$43) gC m^{-2} for maize, which has already been described in the text.

Line 403: What is "sufficient"?

Response

We revised it to full irrigation.

Line 405: This paragraph needs a topic sentence.

Response

This part is removed when we thoroughly revised the discussion.

Lines 419-420: This was not reported in the results. Please add. Figure 8 shows standardized results so it cannot simply be inferred from the figure.

Response

We appreciate the comment. We provided more explanation regarding the effect of groundwater depth on the CO_2 fluxes, and the water logging effect discussion is removed as no direct measurements can provide strong support to our assertation. We present such results in the "The inter-annual variations in the NEE, GPP and ER" sub-section, and is pasted here for your convenience:

"The NEE, GPP and ER for both wheat and maize were correlated with the three main environmental variables of R_{si}, T_a and WD using the multiple regression (see Appendix B for details). In the wheat season, T_a showed its relatively greater importance to all the three CO_2 fluxes with a higher T_a increasing both GPP and ER, and also enhancing NEE (more negative) (Fig. 8a), but R_{si} showed negligible effect to all the three CO_2 fluxes; higher WD correlated negatively with GPP, thereby reduced net carbon uptake. In the maize season, WD had good correlations with all the three fluxes of GPP, ER, and NEE, but T_a showed negligible effect to all the three CO_2 fluxes; WD showed relatively greater importance to both GPP and ER, and a deeper WD drove higher net carbon uptake (more negative NEE); R_{si} had a good correlation with ER, but a bad correlation with GPP (Fig. 8b), ultimately, higher R_{si} in maize season lowered the net carbon uptake (more positive NEE). Overall, R_{si} and WD showed its relatively greater importance in influencing the inter-annual variation of maize (Fig. 8b)."

Lines 421-423: Given that the paper is framed within the context of the climate change mitigation, this is an important caveat and one on which you should elaborate further. Can you provide a sense from your work or from nearby studies on how large methane emissions might be and – when converted to CO2-equivalents – what they might imply for their source/sink status?

Response

We appreciate the reviewer for this comment. We did a literature search and realized that CH_4 measurement remains lacking for similar cropping system in the area. So we did not expand the discussion that much, instead, we added such text to motivate future study "As CH_4 emission of this kind of cropping system over the North China Plain cropland remains lacking, additional field experiments are required to understand how irrigation and water saturation field condition impact the overall carbon budget.”

Lines 451-452: “cropland is more efficient in sequestering CO2 from the atmosphere than forest” - this is a terribly misleading statement and should be removed. In fact, this whole carbon use efficiency section and table 2 should be removed. It doesn’t relate to either of the questions you pose in the introduction. Moreover, It’s well established that the principle source of greenhouse gas emissions from croplands is not CO2 but N2O a greenhouse gas (e.g. Carlson et al 2017) and any assessment of relative climate impacts should fully account for that. Simply comparing NPP to GPP is thus not a relevant way of assessing sequestration potential. It also says nothing of the longevity of any sequestered C. Carbon sequestered in forests, for example, is likely to remain stored on the landscape for far longer than cropland residue (which may only persist for a year or two). This is why agriculture has been attributed to the rising annual variance in northern hemisphere CO2 concentrations (Gray et al 2014, Zeng et al 2014) – there are lots of really productive plants (high NPP) – that are then abruptly removed from the land surface and quickly decomposed.

Response

We appreciate the comment. We removed this related content of carbon use efficiency. However, we sustained other parts of the table, which gave us the information of a few important ratios of the CO_2 budget components.

Line 489: The Jackson et al (1996) number here is relatively low in comparison to crop specific estimates for corn (15%) and wheat (17%) compiled in (Wolf et al 2015).

Response

We appreciate the comment. We cited the recommended paper in the revision.

Line 495: Was your Q10 model “well validated”? If so that validation should be reported in the results section. If not, that should be discussed here.

Response

We appreciate the comment. The soil respiration Q_{10} model is validated by a previous independent study (see the Fig. R1). Please also see response to previous comments.

Lines 496-499: These SOC comparisons should instead be reported in the results section. What is the p-value of the SOC loss rate? If not less than 0.05, this section should be removed. How was bulk density calculated? You say that it is “about” 1,300 – does that mean that this is an approximation? If so, based on what? Since this value is used to calculate the SOC stock with which you ‘validate’ your soil respiration results, it’s critical to know from where this number is coming.

Response

The correlation did not pass the significance test. We removed this part by following your advice. Nevertheless, the bulk density of the soil was measured independently.

Line 503-514: Once again, these data should have been reported in the results section.

Response

We appreciate the comment. We moved this part to results section.

Line 507: What does “sufficient” mean here? And “insufficient” in the line preceding it?

Response

Sufficient means the sample number is big enough, and the ‘insufficient’ means the number of samples is low. We revised these expressions to “These differences may result from the small wheat sample number. However, the sample number at harvest was sufficiently big and no discernible difference was found between the two NPPs at harvest.”

Lines 511-514: This is not an acceptable ‘validation’. The cause of the difference in signs (+/-) between the two independent estimates needs to be determined before deciding whether maize is a source or a sink. Simply calculating an average is not acceptable in this case. Doing so bases your final determination of source/sink on which ever estimate has the greater absolute value regardless of whether it was right or wrong. Why is one positive and the other negative?

Response

We realized our explanation of the analysis had some problem. As we used two independent methods to estimate NPP, so we used the averaged NPP of the two; we also used the averaged NEP of the two methods to estimate NBP to avoid the confusion. The updated texts are pasted here for your convenience:

"We used the average of these two methods for NPP measurements, which were 783 (SD$\pm$46) gC m^{-2} for wheat and 562 (SD$\pm$43) gC m^{-2} for maize. We also used the average of NEP by two independent methods for the measurement, and the NEP was 406 gC m^{-2} for wheat and 269 gC m^{-2} for maize. Furthermore, when considering the carbon loss associated with the grain export, the NBP values were 59 gC m^{-2} for wheat and 5 gC m^{-2} for maize, respectively. Considering the net CO_2 loss of –104 gC m^{-2} during the two fallow periods, NBP of the whole wheat-maize crop cycle were –40 gC m^{-2} yr^{-1}, suggesting that the cropland was a weak carbon source to the atmosphere."

Line 513: This is not an "uncertainty analysis". Nor is it a true "validation" If anything, it would be a "comparison" of methods.

Response

We agree with the reviewer. According to the previous comment, we removed such discussion.

Line 526: This seems like a key finding to me.

Response

We appreciate the comment, we highlight this in the abstract. We also further present this in the results section.

Figure 12: A p-value needs to be reported here. Please also add error bars to illustrate the variation associated with the 10 measurements from each date. If the slope of this relationship is not significantly different than zero ($p > 0.05$), it should not be used to 'validate' your heterotrophic respiration numbers.

Response

By incorporating the reviewer's previous comments, we removed this part as the correlation did not pass the significance level $p<0.05$.

Main references used in this response:

Cristianini, N., and Shawe-Taylor, J.: An Introduction to SupportVector Machines and Other Kernel-Based Learning Methods, Cambridge Univ. Press, Cambridge, UK, pp. 189, 2000.

Fargione, J. E., Bassett, S., Boucher, T., Bridgham, S. D., Conant, R. T., Cook-Patton, S. C., Ellis, P. W., Falcucci, A., Fourqurean, J. W., Gopalakrishna, T., Gu, H., Henderson, B., Hurteau, M. D., Kroeger, K. D., Kroeger, T., Lark, T. J., Leavitt, S. M., Lomax, G., McDonald, R. I., Megonigal, J. P., Miteva, D. A., Richardson, C. J., Sanderman, J., Shoch, D., Spawn, S. A., Veldman, J. W., Williams, C. A., Woodbury, P. B., Zganjar, C., Baranski, M., Elias, P., Houghton, R. A., Landis, E., McGlynn, E., Schlesinger, W. H., Siikamaki, J. V., Sutton-Grier, A. E., and Griscom, B. W.: Natural climate solutions for the United States, Sci Adv, 4, doi: 10.1126/sciadv.aat1869, 2018.

Gray, J. M., Frolking, S., Kort, E. A., Ray, D. K., Kucharik, C. J., Ramankutty, N., and Friedl, M. A.: Direct human influence on atmospheric CO_2 seasonality from increased cropland productivity, Nature, 515, 398-401, doi: 10.1038/nature13957, 2014.

Griscom, B. W., Adams, J., Ellis, P. W., Houghton, R. A., Lomax, G., Miteva, D. A., Schlesinger, W. H., Shoch, D., Siikamaki, J. V., Smith, P., Woodbury, P., Zganjar, C., Blackman, A., Campari, J., Conant, R. T., Delgado, C., Elias, P., Gopalakrishna, T., Hamsik, M. R., Herrero, M., Kiesecker, J., Landis, E., Laestadius, L., Leavitt, S. M., Minnemeyer, S., Polasky, S., Potapov, P., Putz, F. E., Sanderman, J., Silvius, M., Wollenberg, E., and Fargione, J.: Natural climate solutions, P. Natl. Acad. Sci. USA, 114, 11645-11650, doi: 10.1073/pnas.1710465114, 2017.

Kang, M., Ichii, K., Kim, J., Indrawati, Y. M., Park, J., Moon, M., Lim, J. H., and Chun, J. H.: New gap-filling strategies for long-period flux data gaps using a data-driven approach, Atmosphere-Basel, 10, doi: 10.3390/Atmos10100568, 2019.

Poorter, H., Niklas, K. J., Reich, P. B., Oleksyn, J., Poot, P., and Mommer, L.: Biomass allocation to leaves, stems and roots: meta-analyses of interspecific variation and environmental control, New Phytol, 193, 30-50, doi: 10.1111/j.1469-8137.2011.03952.x, 2012.

Wang, Y. Y., Hu, C. S., Dong, W. X., Li, X. X., Zhang, Y. M., Qin, S. P., and Oenema, O.: Carbon budget of a winter-wheat and summer-maize rotation cropland in the North China Plain, Agric. Ecosyst. Environ., 206, 33-45, doi: 10.1016/j.agee.2015.03.016, 2015.

Zhang, Q., Lei, H. M., and Yang, D. W.: Seasonal variations in soil respiration, heterotrophic respiration and autotrophic respiration of a wheat and maize rotation cropland in the North China Plain, Agric. For. Meteorol., 180, 34-43, doi: 10.1016/j.agrformet.2013.04.028, 2013.