Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2019-494-RC2, 2020

Interactive comment on "Decadal variation of CO_2 flux and its budget in a wheat and maize rotation cropland over the North China Plain" *by* Quan Zhang et al.

Seth Spawn (Referee)

spawn@wisc.edu

Received and published: 2 February 2020

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 short-wave 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.

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.

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).

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

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.

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.

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

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

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.

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

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.

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?

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

Line 194: Please change "groundwater table" to "groundwater table depth".

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

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

Line 230: How were parameters "inferred"?

Line 233: Please define the "contribution ratio".

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.

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.

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

Lines 307-309: Consider reporting these as percentages.

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.

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

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.

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.

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

Line 403: What is “sufficient”?

Line 405: This paragraph needs a topic sentence.

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.

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?

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.

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).

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.

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.

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

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

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?

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

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

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.

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Jackson R B, Canadell J, Ehleringer J R, Mooney H A, Sala O E and Schulze E D 1996 A global analysis of root distributions for terrestrial biomes Oecologia 108 389–411

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