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Peer Review History of

**Cropland Carbon Uptake Delayed and Reduced by 2019 Midwest Floods**

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## Peer Review Comments on 2019AV000140

### Reviewer #1

Yin et al.'s manuscript demonstrate the detectability of the 2019 flood impact on vegetation productivity in the US Midwest cropland, combining space-based observations, solar-induced chlorophyll fluorescence (SIF) by TROPOMI and $XCO_2$ by OCO-2, and aircraft $CO_2$ measurements. They present that the extreme 2019 Midwest flood resulted in the shift of SIF seasonal cycle and the reduction of peak value over the cropland. Based on TOROPOMI SIF, the anomalies of gross primary production and net $CO_2$ flux (NEE) are estimated. The estimated positive NEE anomaly in June-July 2019 are examined for the consistency with the two independent $CO_2$ measurements, one from ground-based aircraft measurements, ACT-America, other from space-based measurements, OCO-2, employing an atmospheric transport model.

The manuscript is well structured and reasonably concise. It is a timely topic to utilize space-based observations to constrain net carbon fluxes on a regional scale and country scale. It is also of broad interest to monitor impacts of extreme events on terrestrial carbon cycle at a near-real time. Thus, this study is suitable for AGU Advance. For all the above reasons, I suggest the paper be accepted pending minor revision. My comments are mainly regarding the top-down and bottom-up analysis on NEE anomaly in June-July 2019.

Minor comments

In Figure 3(C), besides the summertime difference of OCO-2 $XCO_2$ between the zonal and the eastern US domain (Corn Belt and its downwind area) means, more differences are apparent. For example, there are some off-zero (positive) periods for both means, and negative period (May) in the eastern US domain mean, which is lower than the zonal mean. OCO-2 $XCO_2$ are de-trended. What cause these differences between 2019 and 2018? Are they noise or uncertainty in this analysis? The zonal mean is also enhanced in July, as well as the eastern US domain. If these zonal enhancements are real, they should be a background contribution to $XCO_2$ over the domain of interest and could be a bias to the top-down $\Delta XCO_2$ analysis.

The agreement of slopes for both OCO-2 and ACT-America is impressive. However, the slope for OCO-2 $XCO_2$ (0.37) has larger uncertainty than ACT-America $CO_2$ (0.07). The authors do not explicitly mention possible contributions to the $XCO_2$ slope uncertainty. Long-term transport could be one possibility. Another possibility might be inherent in the optimized 2018 NEE. Figure S5 shows that OCO-2 data-model mismatch of June-July for 2018 and 2019. Since 2018 OCO-2 $XCO_2$ data are assimilated to estimate the 2018 fluxes, one could expect the mean data-model mismatch for 2018 to be close to zero. However, Figure S5 shows that the mean data-mismatch of 2018 is -0.64 ppm. This off-zero value is relatively large, given the mean data-model mismatch for 2019 of 0.49 ppm. What is the implication of the 2018 negative (off-zero) mean to top-down $\Delta XCO_2$ for 2019?

The definition of cropland (US Midwest) area seems not consistent. According to the caption of figure 5, the observations and modelled $CO_2$ and $XCO_2$ are sampled over the area of 100-60˚W and 20-65˚N. It is a much larger area than the one discussed earlier as Midwest cropland. For example, in Figure 3 (c) and Page 12, Line 380-381, the domain of 75-95˚W and 35-50˚N, is discussed. Is there any reason why the domain of analysis is expanded for the top-down and bottom-up analysis? North of 50˚N and up to 65˚N, it is Canada and covers boreal ecosystem there, which is not apparently downwind area of the Corn Belt.

According to Page 12, Line 387, "Bottom-up $\Delta CO_2$ and $\Delta XCO_2$ estimates are all positive". Why are negative bottom-up $\Delta CO_2$ and $\Delta XCO_2$ seen in Figure 5?

Specific comments

Page 2, Lines 40-42: The enhancements, up to 10 ppm of $XCO_2$ in OCO-2, and ~1 ppm of $CO_2$ in ACT-America, in Abstract, are not read clearly in the main text, section 5 (page 13).

Figure 3 (b): $gCm^{-1}$ day-1 should be $gCm^{-2}$ $day^{-1}$.

Page 15, Line 453: "it in unclear" should be "it is unclear."

Page 15, Line 477: What s detection limit of the satellite $XCO_2$ observations.

**Reviewer #2**

The manuscript by Yin et al. documented an interesting study on using multi-source observation and simulated data to quantify the impact of 2019 spring wet anomaly in the U.S. Midwest. The key idea is to use area-aggregated TROPOMI SIF data in both 2018 and 2019 to quantify the shift in seasonal trajectories and change in peak magnitudes, and then related the SIF-based quantification of GPP & NEE changes to the changes in boundary layer atmospheric CO2 and column-average CO2 estimates from both bottom-up and top-down approaches. The authors found that flood-induced delay in crop planting shifted the 2019 SIF seasonal cycle by 16 days and reduced the peak value by ~15% compared to 2018, and the decrease of cropland NEE was consistent with increase in atmospheric CO2. Overall, this work is interesting and provides new insights on using satellite-based observations to quantify the perturbations of hydro-climatological extremes on regional-to-global carbon cycles. The manuscript is well organized and written. I have the following comments for the authors to consider.

Firstly, although the authors put "2019 Midwest floods" in the title, the definition of floods in this manuscript seems to be not clear. Delayed planting could also be due to field inundation or too wet field condition (but not necessarily floods). I would suggest the authors to consider change it to something like "wet anomaly". Also, "2019 Midwest floods" include all the flood events happened in 2019 and over the Midwest, say the one around the middle of March or those after July, which are not necessarily a direct factor contributed to late planting.

Secondly, the authors did not provide any uncertainty information to the reported numbers. Without the uncertainty information, we cannot really interpret those increasing/decreasing rates. For TROPOMI or OCO-2 SIF products, what is the signal to noise ratio in the early growing season, say in June? I would like to see some comparison between uncertainty in SIF retrievals and the SIF difference between 2018 and 2019.

Thirdly, the definition of "crop production" and "crop yield" seems weird to me. At least, those definitions are not aligned to the general understanding of ag-community. This could be resolved by providing detailed definition or calculation methods in the appendix.

Fourthly, I would suggest the authors add an overall framework graph for the processing of multi-source data and linked them with scientific goals. Considering more than 10 datasets are used in this study, having such a figure or table would be valuable for readers.

Lastly, from the USDA reports, we see that delayed planting is mainly for soybean (the authors talked about this around L443 and Table S2). For soybean, the planting or harvested area decreased about 14%; yield decreased about 7.3%; and final production decreased about 20% for soybean. For corn, planting or harvested area did not decrease at all but with a slightly increase; yield decreased about 5.3%; and final production decreased about 5.3%. Are there any clues from the observations on this crop-specific trigger?

Detailed comments:

L48: J. Liu et al., 2018->Liu et al., 2018. Please check it across the whole paper, say in L70, L82, L127.

L55: Corn Belt never goes to "southern U.S."

L56: >40% is not accurate here. From the USDA reports, the ratios of US production to total world production should be less than 38%.

L67: there should be a period before "SIF"?

L153: TWS from GRACE is not a climate variable.

L184: Did you use the simulated NEE in 2018 from these three models? If you take the data from a existing dataset, please clarify that and acknowledge the data source in the acknowledge part. Otherwise, please describe the simulation protocols.

L186: Why not assimilating TROPOMI here? From my understanding, assimilating TROPOMI footprint data could provide better constraints.

L187: it would be good to show the optimized scaling factors in the supplementary materials. That will be critical for diagnose model uncertainties.

L198: there should be a period before "linear"?

L208: how did you differentiate SIF for corn and soybean here? If not, it's actually a mixing of these slopes.

L269-L270: could poor growth condition also be a factor for this decline??

L279: is this really instantaneous SIF without day length correction?

L282: how did you define planting date using the USDA progress report (the date with 50% area planted)? To my knowledge, the USDA report is weekly, which means the uncertainty there is about 1 week. Please clarify these points in the manuscript.

L360: "crop production" is misleading here. USDA defines the crop production as the yield times harvested area and the unit is in tons. Are you talking about grain biomass per m2 here or NPP?

L380: were all longitudes or only those over the Corn Belt included in the zonal mean?

L364: "crop yield" of which crop? Again, crop yield has specific meaning in agricultural domain and it's grains in bushels per acre or kg/ha.

L371-L372: harvest index=grain biomass/AGB. This statement is not accurate.

L412: Figure. 5->Figure 5

L513: why is the tropomi data only available before July of 2019 at this site? Will the authors make the data available to the public?