Response to Reviewer #2.

The study provides an interesting approach in an attempt to determine the geospatial C balance associated with crop commodities in the USA. The authors generate data for the same for the period 2000-2008. They observed that consumption by humans and livestock impact "significantly" the regional C balance. There was a net C uptake in most of the regions studied, while a few were C sources. Considering the underlying mechanisms behind some of the results provided here, the study is a gross oversimplification of details. Determination of agricultural NPP can be done with some good level of accuracy, but when it comes to consumption and eventual carbon release, the story gets a little more complicated. While I recognize the efforts employed by the authors to arrive at their estimates, there are weak points in this study that compromise their efforts.

The methods used in our study have been published elsewhere in reputable journals, and there is no evidence provided by the reviewer that we have significantly biased our results by applying these methods. The reviewer may be confused by some of the text as presented, however, and we have attempted to clarify issues in several cases. We disagree that our efforts are compromised, and we will explain the reasoning behind our methods throughout this response.

The primary objective of this study; 'geospatially locate the uptake and "eventually" the release of carbon' is misleading. It gives the impression that the authors intend to trace carbon but that is not what they do in this study. Instead they partially address the secondary objective "to investigate whether the annual crop C budget is balance". This makes the study rather simplistic.

We address uptake and release of crop-derived carbon. We did not attempt to trace or track the movement of carbon, nor do we state anywhere in the title or text that we intend to track the carbon. Page 636, line 16 states that human and livestock populations are used as proxies for where crop carbon is ultimately transported and released to the atmosphere.

Tracking the movement of commodity carbon (e.g., agriculture and forest products) may be possible in the future with the use of transportation data. However, that is beyond the scope of this paper and beyond the scope of our project. Using respiration as a proxy for the return of crop C to the atmosphere can be further evaluated in the future, but based on society's use of crop commodities this does not seem like an oversimplification as suggested by the reviewer.

A substantially weakness in this study deals with estimates of livestock derived C. While it is recognized that pasture (herbaceous vegetation) constitutes the bulk of animal feed, the authors categorically "exclude" it in their estimates of C input in the animal feed. It is obvious that >80% of animal weight/energy is derived from pasture. Weight (animal population) is used here to estimate total livestock C. Equally CH4 and CO2 are listed as C output, but from this study, it is impossible to distinguish whether they are pasture or "feed"/derived C. The level of accuracy portrayed by the authors is, therefore, doubtful.

We apologize for any misunderstanding here. In our first objective, we are looking at carbon uptake and release in crop production and crop commodities. We compile data from different sources, as noted in the manuscript, to develop these estimates. Livestock emissions obviously result from consumption of more than just crop commodities; they result from a combination of crop commodities and grazing of pasture and rangelands. Crop commodities include all crops and hay, but not pasture, which we have clarified in the text. At the county level, we are able to estimate total livestock carbon releases based on population and total feed consumption, but there are no data at the county-scale on pasture derived C. At the national level, and for balancing the crop-derived carbon budget only, we remove pasture consumption with data provided by USDA ERS on feedgrains and silage used specifically as animal feed (again, data only available at the national scale). The county-scale maps are useful for providing spatial information on crop C uptake and release even if there is some pasture-derived C. We have clarified this in the text. As a final point, you are incorrect in stating that >80% of animal weight is derived from pasture. In fact, about 30% of total livestock feed is estimated to come from pasture and rangeland, which means that 30% of the respiration is associated with pasture derived C (which was the adjustment that we made at the national scale). The rest is from feed grains and hay. You can estimate the annual percentages by comparing our total livestock consumption/emissions estimates available at http://cdiac.ornl.gov with those based solely on crop commodity statistics in Table 3 of this paper.

Estimates are provided for soil C stocks, without considering other soil C forms with short life spans. The amount of C released as root exudates and rhizodeposits exceeds organic C stocks from root biomass. Relying only on root-derived detritus as estimates of photosynthates redirected underground is a gross underestimation of total soil C. The authors also need to clarify what profile of soil is considered for these estimates. Do they take care of variations that occur as a result of rooting depths of the different crops, since there are annual crop rotations?

Soil carbon estimates are for the soil fraction only. This fraction is considered the <2mm fraction of soil, which incidentally may include root exudates and rhizodeposits. Other carbon not in the 2mm fraction is, by definition, not included in the soil. Short-term pools (<1yr) have no net impact on atmospheric CO_2 in this analysis, because the time-step is 1 year. If our analysis was a seasonal, 1-month or shorter time steps, we would then consider accounting for such short-term pools. We have clarified this in the text.

Estimates of soil carbon change are to a 30cm depth. This has been included in the text, as suggested by this reviewer. Our estimates are based on hundreds of field experiments that, when compiled and analyzed, show no statistically significant changes below 30cm (West and Post 2002). This has been found in numerous meta-analyses and has not been proven otherwise in any meta-analysis to date. It is important to realize that we are not addressing root litter which has short turnover times.

There are single data points provided for each term (data originate from only 1 or 2 sources), with no statistical analyses results. Yet there is repeated use of the word "significant" in the text. Significant, should only apply to statistical results that are proven by providing F and P values of the statistics. Lack of statistical representation of the results makes them doubtful and inconclusive.

It is difficult to address your comment without knowing exactly those data points to which you are referring. We have reduced the number of times that we have used the term *significant*, and we agree that this term should be used only with respect to statistical analyses.

The lack of statistical representation is a consequence of the inventory data used in this analysis, which does not include the necessary covariance data to derive uncertainty. However, the inventory data are used because it provides the best and most comprehensive estimates. This is a trade-off, but a necessary one.

Pg 2. Ln 15. This may not be true, considering that fiber, fuel etc may last longer. Even for processed food products, the shelf life may be much longer.

Unfortunately, the page numbers and line numbers provided by the reviewer match neither the discussion paper posted to the Biogeosciences website or the original manuscript that I submitted. As such, we will do our best to respond to these comments, but may not be able to understand the exact context in which each comment was made.

Most fuel and food products (including processed foods) are consumed within one year or soon after the growing season. In fact, we must consume these products in order to make room for production in the following year. If we have a surplus, the market signals to the producers to make less. There are certainly exceptions for longer-lived products, such as fiber. However, with the exception of cotton products, most crop commodities are cycled back to the atmosphere in 1-2 years. We discuss this in section 2.6 on Commodity Fiber, so that it is clear to the reader how we are handling this in our accounting methods. This can be an area for future improvement in this research, but its impact on the overall crop C budget in the US is 0.75% based on the amount of harvested carbon.

Pg. 4. Ln 1. You need to distinguish between respiration and decomposition, otherwise they mean the same thing.

In our manuscript, different terms are used to represent different pathways of CO_2 emissions. Respiration is a general term that encompasses both inspiration and expiration by humans and livestock. Expiration specifically refers to the exhalation of CO_2 and other trace gases. These definitions are discussed in more detail by West et al. (2009) in a paper on the human carbon budget. Decomposition is the decay of organic matter, whereas oxidation is used to refer to the actual chemical breakdown of mineral carbon to CO_2 (e.g., dissolution of agricultural lime that has been applied to croplands). While the terms refer to different pathways of CO_2 emissions, the impact on net emissions or atmospheric CO_2 is the same, regardless of source or pathway.

Ln. 5. The nature of the data used here cannot account for C fixed photosynthetically. There loose ends that are not considered, shot/life soil carbon for example.

The nature of the data here can and has accounted for C fixed photosynthetically. We are specifically referring to the net amount of carbon fixed in herbaceous plant stock. This net annual amount of fixed carbon is accounted for in our inventory-based estimates of annual NPP. We then use a number of datasets to estimate where this carbon is eventually released. There is currently a debate within the scientific literature as to the impact of short-term carbon pools on net annual carbon emissions, but such a debate is beyond the scope of our study.

Ln.10. The simplification involved here definitely compromises the study. Definitely, the authors recognize that they are simplifying a relatively complex process and in doing so, a lot of valuable information is omitted.

This study is a simplification of the processes of photosynthetic carbon fixation. However, a simplification does not compromise a study or introduce bias. The method we use here provides a comprehensive estimate of crop biomass that includes all crops species and associated land extents in the United States. We believe the reviewer is making an assumption that simplicity introduces error. Since this method is supported and recognized as a valid method (see references provided in our subsequent response below), the burden would be on the reviewer to provide evidence of bias or error.

Ln4 Unfortunately, I am unable to access West et al. 2010, but it would be interesting to see how r:s ratio data is used to calculate NPP!

Please refer to the following papers for additional studies that have used ratio data for estimation of NPP: Prince et al. 2001. Climatic Change 51: 73-99; Hicke et al. 2004. Earth Interactions 8:10.; Hicke and Lobell. 2004, Geophysical Research Letters 31: L20502; and Bradford et al. 2005. Ecology 86: 1863-1872.

Ln7. Is there a reason for the choice of these crops? The authors should care to provide reasons for their choices.

The crop species included in our analysis consist of nearly all the commodity crops included in the US NASS inventory data, with the exception of woody crops (e.g., orchards) and miscellaneous crops. The crops that are accounted for in our analysis represent 99% of total US crop production. We have added this information to the text on page 7, lines 118-119, for clarification.

Ln 21. Is 20 yrs necessary? What you need are inter-annual changes between 2000 and 2008. Maybe data of 1999 maybe interesting, otherwise the rest are not relevant to your study.

One cannot estimate changes in soil carbon without a longer-term perspective on land use and management because much of the carbon in soils has turnover times of decades to centuries. A land-use history provides information on previous carbon accumulations or losses that have occurred; it provides information on the current state of soil carbon which, in turn, allows for estimates of future changes in soil carbon. In fact, lack of information about the previous land use and management can lead to large biases that may even change the sign of the soil C stock change. This is the reason why we conducted the estimation procedure starting in 1980 in order to estimate changes in soil carbon from 2000-2008. Additional details are provided by West et al. (2008).

Again, what you need is information of carbon stocks irrespective of land use/management practice. This other part is not covered in the study. Instead, you should detail the depth considered for these estimates and how you take care of root distribution and variations that arise during annual crop rotations. Is this therefore a simplified version of data from West et al. 2008? What is new in the current data?

We are using the same procedure as that used by West et al. (2008). It is not a simplified form. We have information on initial soil carbon stocks, and that information is documented in the paper by West et al. (2008). As noted above, and in several studies on soil C stock changes in agricultural lands (e.g., Ogle et al. 2005, Biogeochemistry 72:87-121), the land use and management is important to inform the changes in soil C stocks. Scientists have recognized the impact of land use and management on soil C stock changes for many decades. Again, we are not estimating root litter because this pool turns over on less than an annual time scale. We have included information about the depth of the measurement in the manuscript.

Pg 6 Ln 16 -20. This might too much oversimplification of carbon estimate.

Please refer to our aforementioned responses regarding the documented support for our methods.

Pg.7, Ln 24 is interesting. What about the C in methane? Isn't it also originally part of plant C? The statement on Nitrous oxide is irrelevant.

This paragraph explains our methodology with respect to current IPCC methods on greenhouse gas emissions. We thought this would be useful because this method is widely known. IPCC only considers methane and nitrous oxide emissions from livestock. They do not consider CO_2 emissions from livestock, because carbon in crops is a short-lived pool. What is taken up by the crop is quickly released within 1-2 years, with no net impact on atmospheric CO2. We, however, *do* consider carbon in crops because it is our objective to understand *where* the carbon is being taken up and released . We have deleted the sentence regarding N_2O in an effort to not confuse the reader, as suggested by the reviewer.

Methane is accounted for in our estimates. The fraction released as methane per head of livestock is provided in Table 2. However, since our objective is to quantify net carbon sinks and sources, all of our estimates are in units of C, not CH_4.

Pg.8, Ln 14-19. This is a point of contention in this study. You rely on animal wt. for your most of your livestock C estimates. Here you indicate that you only consider crop carbon for animal feed. Unfortunately, this is not correct, because animal weight weight/or energy source does not originate entirely from the crop derived carbon. I believe 80% of this is derived from pasture, yet this is not taken care of. Again, you have CH4 + CO2 as by products. How do you separate crop and pasture derived C in your estimates?

Our estimates of livestock emissions are based on livestock population numbers and consumption, not on animal weight. How much carbon is taken in by the animal enables us to estimate how much carbon is emitted by the animal. Please refer to our previous responses for answers to your questions.

Pg 10, Ln 20. Significant should only imply statistical tests and F and P values should be provided. Ln. 22. Why 10 Tg C? Do you mean annual differences? Which years are considered? This is confusing.

We apologize for any confusion. The sentence has been re-written and currently states "The diversion of nearly 10 Tg C of harvested corn grain for ethanol production in 2007…". In other words, an additional 10 Tg C of corn grain was produced in 2007 for the purposes of ethanol production. As this occurred in the year 2007, it would indeed be an annual change and, hence, an annual difference.

Lns 23 and 24. Why should diversion of C into fuel production only influence livestock and carryover reserves? You need to provide supporting details.

Your question is rooted in decision making and economic market dynamics. While interesting, this is beyond the scope of this paper. Our objective was to quantify the sources and sinks associated with crop-derived carbon, but additional research will be needed to evaluate the socio-economic reasons for why some commodities increase or decrease annually.

Pg 11. Ln 7. Again, "significant" without statistical tests.

We have removed all use of the word *significant* in our manuscript, as suggested by the reviewer.