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Biogeosciences Discussions

Interactive comment on "Regional uptake and release of crop carbon in the United States" *by* T. O. West et al.

Anonymous Referee #2

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General comments

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 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.

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 CH_4 and CO_2 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.

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?

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.

Specific comments

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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.

Pg. 4.

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

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.

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.

Pg 5

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!

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

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.

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

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current data?

Pg 6

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

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.

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 CH_4 + CO_2 as by products. How do you separate crop and pasture derived C in your estimates?

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.

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

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

Interactive comment on Biogeosciences Discuss., 8, 631, 2011.