

Interactive comment on “Atmospheric inversion of the surface carbon flux with consideration of the spatial distributions of US crop production and consumption” by J. M. Chen et al.

Anonymous Referee #1

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General comments (overall quality): Within the bounds of the inversion literature, I felt like this was a reasonable study and the results were well-presented. However, the results were not particularly surprising, given that the different inversion results primarily reflect differences in the priors. The sink in the southeast forests is interesting, but also seems to be an artifact of the BEPS model used as priors. I feel that this study mainly reiterates the conclusions from Hayes et al. (2012), West et al. (2011), and Gourdji et al. (2012) that: a) agricultural production and consumption have an influence on the carbon cycle, and b) prior flux estimates from biospheric models need to be adjusted to take into account agricultural fluxes when performing atmospheric inversions. It's hard to know if the results presented here are any closer to the “truth”

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as compared to other inversion studies, or even how they stack up with other estimates of the biospheric CO₂ sink over North America. I realize that this is a criticism that could be directed at most inversion studies, given that there is no real validation data for regional-scale CO₂ fluxes. In general, although well-written and presented, I am not sure about the “value-added” of this study to the literature on either the carbon cycle of North America or atmospheric inversion methodology. A substantial rewrite and a further discussion of the results could potentially help, but there may also be a limit to what we can know about the North American carbon cycle from atmospheric inversion studies that mix both the top-down and bottom-up constraint.

Specific comments: p. 6092: For the first statement about running the inversion globally, please state briefly why you do this for readers who may be new to the field.

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