

Interactive comment on “Inorganic carbon fluxes across the vadose zone of planted and unplanted soil mesocosms” by E. M. Thaysen et al.

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Thaysen et al. used mesocosms to quantify carbon budgets for planted and unplanted barley crop systems. They present a substantial amount of C cycling data, clearly investing considerable effort in their measurements and analyses. The basic mesocosm approach seems reasonable (though see comments below regarding better justification for their limited sample size). However, the paper presently has three primary limitations, all of which can likely be remedied through careful revision: 1) Importantly, their estimates of net carbon balance and GPP are suspect and appear unconventional; 2) The modeling methods are elaborately described and yet the results minimally presented or interpreted; and finally 3) precise objectives and focus of the manuscript is unclear. In some ways, all of these points are related in that the authors provide a col-

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lection of often interesting results without a clear, common objective; it's as though the authors wished to present as much data, processed in as many ways as they could, without developing a compelling and cohesive focus. Also, the authors must revise their primary conclusion. I'd like to see a more focused paper with clear objectives that uses more conventional approaches to calculate key C cycling terms.

Specific comments, elaborating on above:

*Mesocosm treatments are unbalanced and seem to have been haphazardly applied (Pg. 4257, Lns 18-24). Why was the harvest schedule of 1 through 3 different from 4 and 5? Why was the sample size so low? *Much of the text is superfluous (e.g., Pg 4256, Lns 5-15; Pg 4262-4265; Pg 4270, Lns 3-23); the document reads like a thesis reporting numerous details; it's great the authors are so detail oriented in places, but this detracts from a focus. *The following primary conclusion is not supported (Pg 4253, Lns 23-25): “Our results indicate no change of the cropland C balance under elevated atmospheric CO₂ in a warmer future climate, in which plant biomass and soil 25 pCO₂ are expected to increase.” The experiments manipulated neither mesocosm climate nor CO₂. *Modeling results are scantily described. The model setup is well explained, but the justification for presenting simulation results for only a single mesocosm over a short duration is unclear (Pg. 4260-4261, Lns 25-8). *Pg. 4266, Lns 7-18. I find the C budget methods unclear and therefore potentially problematic. Why was the C balance only calculated for mesocosms 4-5? What is meant by the following?: “Post-harvest CO₂ fluxes from mesocosms 1–3 were considered, but marked as indicative due to a shorter growth period.” Most importantly, I don't understand why/how the net ecosystem C balance is calculated as the difference between NEE and aboveground biomass. Consider that annual NEE minus total (above and belowground) NPP equals heterotrophic respiration; I'm not certain how the terms the authors incorporated in their calculation derives a “net C balance” for the ecosystem. Likewise, how was GPP estimated from above and belowground plant biomass unless autotrophic respiration was added back in somehow (since $GPP = NPP + R_a$)? *Pg. 4266, Lns 22-23 The

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authors say that “the selected data are representative for all replicate mesocosms”. Then why not show means for each treatment? *Pg. 4268, Ln 14. The authors should specify that negative NEE indicates uptake by mesocosm ecosystem. *Pg 4269, Ln 20-23. Again, I do not understand this measure of ecosystem C balance: “The net ecosystem C balance of the mesocosm system (i.e. the amount of C captured in total biomass minus the ER minus the harvested aboveground biomass, but excluding leaching losses) was 1 to -10.8molCm^{-2} during 78 days of growth and 60 days of post-harvest”. Why not simply use NEE, which is the C balance (GPP – ER)? And, are the units expressed per day? *Pg 4272, Ln 12. “we have quantified the inorganic C dynamics...”. Why not drop “inorganic since biomass was also characterized?” *Pg 4275, Lns 19-21. Previously in the results the net ecosystem C balance was expressed on a per day basis (I think), and here on a per year basis; yet, mesocosm fluxes were not measured for an entire year, so how were annual estimates derived? Also, why is there such a large range in net C balance among mesocosms? Also, the sign convention (negative for C gain by ecosystem) presented by the authors is not the same as that for European croplands. Pg. 4276-4277. The authors only dedicate a single paragraph to model interpretation, making its relevance and context uncertain.

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