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# *Interactive comment on* "Technical note: mesocosm approach to quantification of carbon dioxide fluxes across the vadose zone" *by* E. M. Thaysen et al.

**Anonymous Referee #3**

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This ms may be of interest to soil scientists in that it presents a possible methodological approach to the study of dissolved carbon dioxide across unsaturated soils. The authors describe an artificially constructed, highly instrumented, soil column, which tries to capture the flow of dissolved inorganic carbon through two soil horizons: A and C. They show that they can replicate well the amount of measured dissolved carbon, which leached from the bottom of the column, by estimating the flow from measurements of gaseous CO2 partial pressure, soil pH, temperatuer and moisture taken along the column.

This ms could be improved and clarified by addressing the following:

1. The authors refer to their artificial soil column as a “mesocosm”. It would be helpful to the reader to better understand what exactly they mean by this term. According to the definition presented by E.Odum in his 1984 paper in BioScience vol.34, 9, a mesocosm is a “bounded or partially enclosed outdoor” experimental set-up. According to that definition an extracted intact soil monolith could be considered a mesocosm, which is actually the approach taken in sonme of the studies the authors cite, such as Lange et al., 2009, but a sifted, treated soil that is repacked does not seem to be representative of a mesocosm.

2. In the abstract you state that your system “was designed to assess the effect of agricultural practices on carbon fluxes within and out of the vadose zone at controlled environmental conditions”. I have two issues with that statement. a. While you state that this system can assess the effect of agricultural practices, you do not go on to discuss how in your ms. If the idea is to help capture the effects of different liming practices then this should be stated and discussed at least in the introduction, given that in the actual experiment that is presented you did not test any such practices. b. I can see how your system monitors the carbon flow within the column, but what do you mean by stating “. . . and out of the vadoze zone. . .” in that statement above?

3. The introduction can be improved by clearly stating the importance/usefulness of such a system as yours, not merely stating that laboratory studies, with their capacity for more controlled environmental conditions, are better suited for process oriented studies compared to field based studies. Again here, you should be careful of what you mean by mesocosm, as you refer to your system. Furthermore the reader would benefit from knowing what the authors mean by DIC – dissolved inorganic carbon. It seems in their introduction they confuse gaseous CO2 in soil air with that of dissolved CO2 in soil solution and bicarbonate/carbonated species. For example, their sentence on DIC production that flows over onto page 9949 into line 1 is followed by the statement that knowledge on soil CO2 production and transport is incomplete, citing Jassal et al 2005. This would imply that Jassal et al 2005 presented a study on DIC, however



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their study was on soil respiration – gaseous form of CO2 in soil air from microbial and plant respiration! Furthermore, the citation to Clark et al 1997 is missing from the reference list. The statement on lines 9-13 on page 9949 also makes no sense. You begin stating that previous mesocosm studies focused mainly on gaseous CO2 efflux, and then state that little attention has been paid to "microbial respiration rates" – but this is gaseous CO2 emissions! Furthermore, there have been past studies and efforts to measure microbial soil respiration with depth and in the absence of plant roots.

4. While on the above point, I assume the purpose of this exercise is to quantify the amount of CO2 evolved due to the addition of lime in agricultural fields and how much of that ends up in soil water and leaches out of the soil. As such, it is not clear how this system can differentiate between CO2 produced by microbial decomposition of soil organic matter that dissolves in the soil solution and that due to inorganic production of CO2 due to bicarbonate chemistry in the soil solution.

5. In methods, on page 9950, line 11 – what do the authors mean by a change from "wet to moist"?

6. Also with regards to methods – the authors describe how great care is taken to maintain the bulk density and structural integrity of the packed column – but how do you think the installation of all the monitoring equipment along the length of the column impact these properties?

7. Equations or their basic overview should be listed describing how you calculated DIC percolation rate, not simply refereeing to the software: lines 20-25, page 9953.

8. Lines 13-14 in Results – pCO2 was "strongly/significantly reduced" compared to what?

9. Discussion – lines 20-15 p.9956 – what do you mean by the comparison and stating that the differences of your results with the crop and agriculture studies, but not with forest studies, "underlines the crucial component of root respiration . . . to DIC"? Forests



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also have roots, or where the studies you cited done in trenched/root-excluded plots? Then mention this.

10. It would be beneficial, although not sure if logistically possible, if you could get DIC rates from the actual agricultural field from where you collected your soil for the columns. If those rates were comparable to those you get from your system, then this would give good justification for the system's reproducibility of field conditions, as opposed to comparison to literature cited field studies that may have been carried out in different soil types from the ones you used.

11. Conclusion could be reworded. For example, you start out stating that mesocosm are "superior" to field based studies for process elucidation and then finish off stating that they "appear to be suited for more process-based" studies. Once again – if you can get fluxes from the field where you obtained the soil for your system, then the statement on lines 2-5, page 9959, would have more strength.

12. Are the lines joining the points necessary in Figure4 - do they represent the functional fit? It is unclear. In b, the regression does not appear to be linear.

I hope the above will be of use to the authors. Thank you for your submission.

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Interactive comment on Biogeosciences Discuss., 10, 9947, 2013.