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Discussions

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 #4

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general comment: The article presents results from an experiment performed in controlled conditions, investigating the mechanisms behind the process of C losses through leaching of inorganic C in soil and comparing the results with the theoretical predictions. A weak point could be the presence of only two replicate "mesocosms". Anyway, because of the particularly controlled conditions occurring in the mesocosms in comparison to field studies the obtained results can be considered reliable. I think that the authors should provide some revisions to the manuscript according to the specific comments below.

Specific comments:

1. In the abstract you say that the study was conducted to assess the effect of agricultural practices on carbon fluxes, but it's not clear which practices you refer to. If you refer to irrigation, it should be clarified. Actually, in the experiment there are not different treatments simulating agricultural practices, so it should be indicated why the results from the study can be useful to understand their effect on DIC leaching.

2. I think that the introduction should be aimed more at explaining the relevance of DIC leaching within the C cycle or its importance for water contamination, more than at justifying the particular method used in the study. I would suggest to move into the discussion considerations such as "Field studies have the advantage..." (line 5 to 10, page 9949).

3. page 9953, line 18: You determined DIC concentration in percolating water with a TOC analyser, but you should specify the method used for the analysis, which allowed to distinguish between inorganic and organic C.

4. line 20, page 9956: you say that DIC concentration in your study were similar to that measured in a forest soil but lower than that measured in croplands. But why do you think this underlines the importance of root respiration? Was the root respiration, in the forest you refer to, particularly low, or the root respiration in croplands and grasslands much higher? In such a case you should explain that in the text.

5. page 9955, line 5 to 15: The term "correlated" is not correct when associated to an R^2 value (regression coefficient, while "R" is the correlation coefficient). Furthermore, you should provide the significance of the regression (p value). If your aim is to show that two variables such as measured and predicted cDIC are not different, you should also plot the data in comparison with the 1:1 line in the same graph, possibly testing that the slope of the linear regression is not different from the 1:1 line, for example using model II regression (Legendre and Legendre, 1998). Alternatively you can use a t test, as you did to compare the data from the two microcosms. Two variables can be highly correlated but have very different values.

6. I would suggest avoiding the first sentence of the conclusion paragraph, as the aim of the paper is not to show the reliability of mesocosms in comparison to field measurements. Al least, change the word "superior" with "more suitable" or something similar.

References: P. Legendre and L. Legendre. Numerical ecology. Number 20 in Developments in Environmental Modelling. Elsevier, Amsterdam, 2nd edition, 1998.

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