Interactive comment on “Technical Note: Mesocosm approach to quantification of carbon dioxide fluxes across the vadose zone” *by* E. Thaysen et al.

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We would like to thank all referees for providing constructive comments. We are certain that we will be able to address the issues raised by the referees. Therefore we hope to be given the opportunity to submit a revised manuscript. Suggestions for revision of our manuscript addressing each issue raised are provided below.

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, temperature 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 some 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.

According to Kampichler et al., (2001) there is terminological confusion on the mesocosm concept. Most often, mesocosms seem to be defined as outdoor experimental setups, in agreement with the mentioned reference, or as excavated soil cylinders that are placed in the laboratory. Meanwhile, several other studies (e.g., Jouquet et al., 2012; Reichel et al., 2013) refer to filled soil columns as mesocosms. In order to avoid confusion, we agree that the term should be defined in the MS. Suggestion for the in-text revision: “…Soil column studies under controlled conditions in the laboratory may be less realistic but provide potential for a detailed study in a homogeneous environment (Lewis, 2010) and may thereby offer a better process understanding. Incubated and non-incubated artificially-filled soil columns and soil monoliths are in the following collectively referred to as mesocosms.”

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 vadose zone" in that statement above?

Effects of a given agricultural practice on pCO_2, alkalinity and DIC percolation can be assessed because the mesocosm system proved to be gas tight (the estimated DIC percolation flux (derived from pCO_2, alkalinity and drainage flux) was not significantly different from the measured DIC percolation flux) and because DIC percolation fluxes were reproducible.

The wording "…and out of the vadose zone" refers to DIC fluxes into the groundwater through the establishment of the artificial groundwater table provided by the suction disc at the mesocosm bottom. We agree with referee #3 that this wording may be difficult to understand and it may well be omitted from the text.

Suggested in-text manuscript revisions:

In the Abstract:

"A soil mesocosm system, designed to assess the effect of agricultural practices on CO_2 fluxes in vadose zone at controlled environmental conditions, was here evaluated for its capability for investigating the mechanisms behind dissolved inorganic carbon (DIC) percolation to the groundwater from unplanted soil."

At the end of the introduction:

"In this work, a simple, economical soil mesocosm system that was carefully filled with homogenized, sieved soil was evaluated for its capability for producing reliable inorganic C fluxes in the vadose zone of unplanted soil. We show that the mesocosm system seems to be well-suited for investigation of the effect of different agricultural practices (e.g. cropping, liming, irrigation) on CO_2 fluxes in the vadose zone."

In the discussion (possibly line 2, page 9956):

"Our results further indicate that the mesocosm system seems to be well suited for the investigation of the effect of different agricultural practices such as liming, fertilization, irrigation or cropping."

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 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.

Through the good agreement between measured and estimated DIC percolation we show that the mesocosm system is gas tight and hence that any measurements in the system are reliable. This makes the mesocosm very useful in terms of process-oriented research where small differences in concentrations can determine the magnitude and directions of (carbon) fluxes. The mesocosm system is further original amongst ordinary lysimeter approaches because it allows for a description of the total inorganic carbon balance in the vadose zone under consideration of both *soil air C and water C by comparably simple and cost-efficient means. The mesocosm system allows for the detailed investigation of the interplay between the pCO_2, the infiltration rate and the DIC through the application of controlled conditions (temp, irrigation rate, suction, soil structure), and is hence ideally suited for subsequent modeling studies of experimental results. Suggested in-text revision (page 9949, line 7): "Studies in mesocosms that apply homogenized and sieved soil and are held under controlled conditions in the laboratory may be less realistic, but provide potential for a detailed study in a homogeneous environment and thereby offer better process understanding. Achieved understanding of the experimental results may be double-checked through subsequent modeling studies for which mesocosms are the ideal study frame."* For implementation of the economic and simple aspect to the mesocosm system see the our suggestion for the in-text revision at the end of our answer for point #2.

We realize that we failed to define DIC and apologize for the missing reference of Clark et al., (1997).

The statement on page 9949 says "DIC in the soil water derives from the dissolution of biogenically produced carbon dioxide (CO_2) and carbonate minerals, and is controlled by the partial pressure of CO_2 (pCO_2), pH and temperature (Clark et al., 1997). However, our understanding of production and transport of CO_2 in the soil is incomplete (Jassal et al., 2005)". We are aware that Jassal et al. (2005) are referring to gaseous CO_2 production. We start out by stating that DIC in soil water is a function of biogenically produced CO_2. Therefore, a change in the CO_2 production directly affects the DIC, and a lack in the understanding of the controls on CO_2 production and transport transmits to an incomplete understanding of DIC formation. We agree that the language in the MS could be improved to clarify our point. Suggested in-text revision: "Dissolved inorganic carbon in the soil water derives from the dissolution of biogenically produced carbon dioxide (CO_2) and carbonate minerals and is controlled by the partial pressure of CO_2 (pCO_2), pH and temperature (Clark et al., 1997). Our understanding of dissolved inorganic carbon formation in the soil is incomplete due to incomplete understanding of the production and transport of gaseous CO_2 in the soil (Jassal et al., 2005), and due to the control of the DIC by the pCO_2.

We agree with referee #3´s comment regarding microbial respiration rates in lines 9-13 on page 9949 and suggest replacement of "microbial respiration rates" by "pCO_2". For a suggestion for an in-text revision of lines of lines 9-13 see our answer to point 2 by referee #5.

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.

The described system cannot as such differentiate between biogenically produced or lime-derived CO_2. However, isotope analysis of the carbon in the soil water and effluent ($^{13}C/^{12}C$) can provide this information.

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

We intended to say that almost all water contained in the quartz flour suspension was pumped through the filter disc whereby the quartz flour layer would dry. Prior to complete drying, i.e. the quartz flour layer was still moist, the C horizon/quartz flour mixture was added. Suggested in-text revision: "Vacuum was applied to the mesocosm bottom outlet (Fig. 1) and the water in the suspension was sucked through the filter disc. Just before the quartz flour layer became dry, a 30 mm layer of a 0.5:1.0 mixture (w/w) of dry quartz flour and C horizon soil material was added."

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?
We believe that the installation of the monitoring equipment in the mesocosm contributed little to a change in the bulk density, as the volume of each of the samplers (i.e. $1.4*10^{-2}$ L, $2.2*10^{-2}$ L and ~$2.5*10^{-3}$ L for soil air samplers, soil moisture and temperature sensors and water extraction samplers, respectively) and the resulting total volume taken up by samplers (~$9.7*10^{-2}$ L) was very small compared to the volume of the soil-filled mesocosm (19.8 L). Suggested in-text revision: "The installation of monitoring equipment along the depth of the mesocosms is expected to have caused little alteration to the soil integrity and bulk density as the combined volume of all samplers constituted ~0.5% of the volume of the soil-filled mesocosm."

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.
The [DIC]at the mesocosm bottom was calculated from the weekly measurements of the pCO_2, soil water alkalinity and temperature at the mesocosm bottom, as written on line 23, page 9953. The weekly DIC percolation was then calculated by multiplying the weekly recharge rate (for water) with the calculated [DIC], anticipating the [DIC] at the mesocosm bottom was equal to the [DIC] in the effluent. The latter could be clearly stated in the MS but we do not think that an elaboration on these basic calculations is needed.

8. Lines 13-14 in Results – pCO2 was "strongly/significantly reduced" compared to what?
Here we mean that the pCO_2 was strongly reduced compared to the pCO_2 at 25-67cm which is constant with depth (the pCO_2 at 7 cm is strongly reduced due to diffusional loss of CO_2 to the atmosphere). Suggested in-text revision: "Significantly reduced pCO_2 compared to the pCO_2 in above-lying samplers was measured at the bottom at days 64 and 71 in mesocosm 1 and day 71 in mesocosm 2."

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 also have roots, or where the studies you cited done in trenched/root-excluded plots? Then mention this.
We agree that this phrasing is unclear. In comparison to unplanted soil, the DIC percolation flux from agricultural soil and grassland is much larger, which may largely be ascribed to root respiration, causing higher pCO_2. In forest soils roots are present too but the lower pH of forest soils causes lower DIC concentrations which are comparable to those in unplanted soil. In the revised manuscript we would change the argumentation to: "The average [DIC] in our study was similar to the [DIC] in the percolate from sandy forest soils with a topsoil pH of 3.8-4, but was far below the [DIC] in the percolate from croplands and grasslands (Kindler et al., 2011; Walmsley et al., 2011; Siemens et al., 2012). This indicates that a higher pH in cropland soil, but a lower pCO_2 in the absence of roots are acting in each their direction in terms of DIC formation".

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.
We are aware that the article would benefit from a comparison with DIC leaching rates in the field. Unfortunately, we have not been able to collect data from an unplanted field yet and can therefore not implement this comparison in the article.

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.
In response to the critics in points 10 and 11 and we suggest the following rewording of the conclusion:“ In this study simple, well-designed mesocosm systems were applied for the measurement of DIC percolation fluxes in the vadose zone. Our results show that DIC transport to aquifers in fallow soils is well described by the [DIC] calculated from the soil gas pCO_2 *and the soil water alkalinity at the mesocosm bottom and the drainage flux. Hence, mesocosms seem suited for more process-related research on dissolved and gaseous* CO_2 *fluxes in the vadose zone, potentially involving plants and various soil amendments that can aid to fill the gaps in current our understanding.”*

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.
The lines are not necessary and were merely connecting the data points. The regression lines in Fig 4b ARE linear. Please also see our answer to point 5 referee #4.

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

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.
1. In the abstract you say that the study was conducted to assess the effect of agricultural practices on carbon fluxes, but it is 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
Please see our answer to point 2 by referee #3

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
Referee # 4's suggestions may be met by elaborating slightly more on the importance of the DIC percolation flux in the carbon cycle, for example by adding: "The flux of carbon dioxide (CO_2) from the soil to the groundwater as dissolved inorganic carbon (DIC) is estimated at 0.2 Gt C yr^{-1} and is much less than the upward flux of CO_2 from the soil to the atmosphere of 59-76.5 Gt carbon (C) yr^{-1} (Kessler and Harvey, 2001; Raich and Potter, 1995; Houghton, 2007). However, dissolved C leached from soils constitutes a significant fraction of the annual net carbon loss from croplands and grasslands but estimates are few (Kindler et al., 2011)…"

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.
We disagree with referee#4 in that we have to state the method for distinguishing inorganic carbon (IC) and organic carbon (OC) as we conducted only IC analysis. In the below we provide a brief elaboration on the methodology behind IC and OC analysis on the TOC analyzer:
The TOC analyzer can measure total carbon (TC) and IC, where IC analysis does not necessitate TC measurement. TC measurement is typically coupled to IC measurement to determine the OC from the difference between TC and IC. TC is measured by burning of all carbon in the sample over a platinum catalysator at 680 °C. For IC measurement, the sample is acidified down to pH 3 using H_3PO_4 which drives off dissolved CO_2 from the water into the air. Both TC and TIC are quantified from the CO_2 evolution from the sample.

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.
Please see our answer to point number 9 by referee#3.

5. page 9955, line 5 to 15: The term "correlated" is not correct when associated to an R2 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.

In the below we have revised the mentioned paragraph considering the recommendations for a replacement of the R^2 value with the R value and the t-test, and have implemented the proposed changes to figure 4. We agree that the changes improve clarity in the presentation of our data.

Suggested in-text revision (results section): "The measured cDIC during the experimental period was 21.1-24.6 mg C (Fig. 4A) and equal to a DIC flux of 0.8-0.9 g m^{-2}. The estimated cDIC of 25.9-26.5 mg was only slightly higher than the measured values (Fig. 4A) and was closely correlated with the measured cDIC (R= 0.98 and 0.99 for mesocosms 6 and 5, respectively, and $p<0.001$ for both mesocosms). However, the slope of the regression for estimated vs. measured cDIC was significantly different from the 1:1 line ($p<0.001$, Fig. 4C). The cDrainage amounted to 149-157 mm and corresponded to 1.3 and 1.1 times the water-filled pore volumes for mesocosm 1 and 2, respectively (Fig. 4B). The measured cDIC and cDrainage were not significantly different between mesocosms (p=0.68 and 0.99, respectively). The measured cDIC was highly correlated with cDrainage in both mesocosms (R=0.97-0.99) (Fig. 4B)."

Revised figure 4:

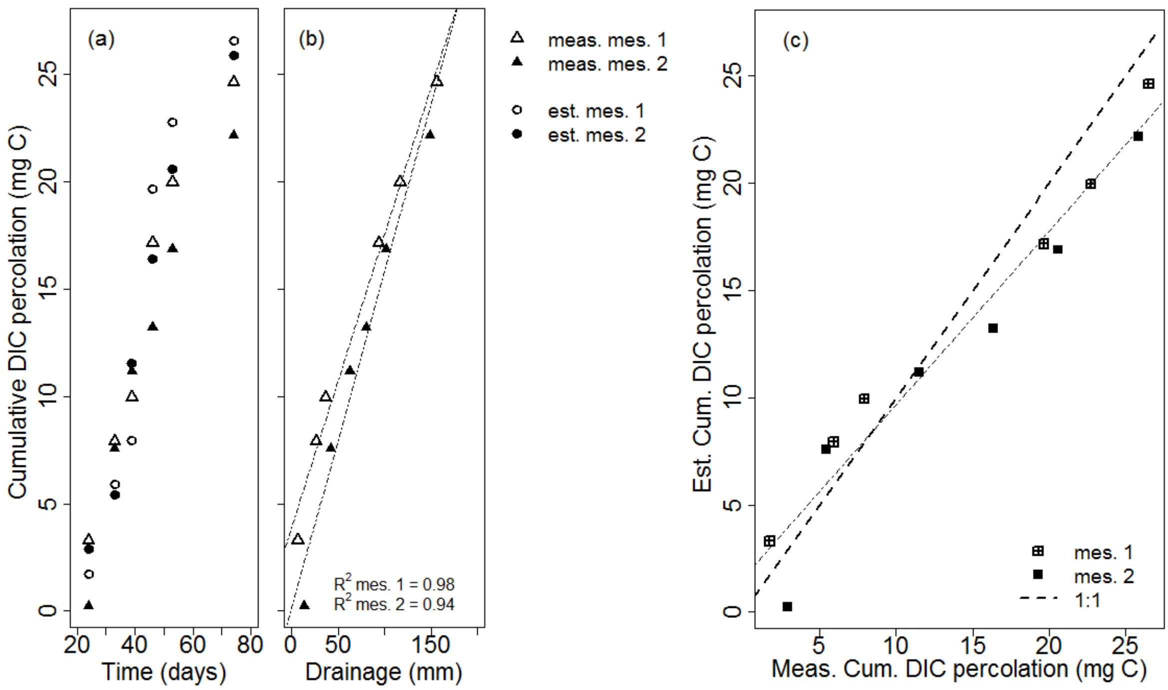

Fig. 4. Measured and estimated cumulative DIC percolation as a function of time (a), measured cumulative DIC percolation as a function of cumulative drainage (b) and estimated vs. measured cumulative DIC percolation (c). The DIC percolation was estimated from the alkalinity, pCO_2 and water flux. Narrow dot-dashed lines in b) and c) are regression lines.

We think that the difference in the slope of the regression for estimated vs. measured cDIC is sufficiently conferred on page 9955, line 25: "*Differences between the calculated and the measured cDIC, could be related to disequilibrium between gaseous CO_2 and DIC or the fact that the measured pCO_2 was a "snap shot" of possible pCO_2 whilst the measured [DIC] in the percolate was the weekly average, as suggested by Walmsley et al. (2011).*"

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. At least, change the word "superior" with "more suitable" or something similar.
We will consider this suggestion, please also see our in-text revision to the conclusion given in referee #3's point 11.

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

Anonymous Referee #5

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This technical note presents a mesocosm approach in controlled conditions as a realistic tool for quantification of carbon dioxide fluxes. While the work and results have certainly some merit for the scientific community working on models systems for ecosystem research there are several limitations that prevent me to fully recommend this work for publication in its present form.

Specific comments

1) Methodological limitations. Although I appreciate the difficulty of finding technical solutions for increasing the realism of models systems, the filter disk approach combined with high applied suction may not always be a good surrogate for what happens in natural systems, especially when the authors argue about the increase realism of their system. For example, during summer (the simulated season of the experiment), high evapotranspiration observed in many natural systems prevents the escape/leakage of water into the groundwater and, in fact, upward water infiltration from the groundwater has been often observed and which can be emulated using Mariotte's bottles. While of some originality I don't find that the proposed system is superior to lysimeters approaches which are increasingly more used in ecological research because they are able to perform the same function as the system presented here and in addition, allow for measurements of evapotranspiration and can also include the water table. Furthermore, the diameter of the soil column seems rather small for a study looking at dissolved organic carbon as it runs the risk of increased preferential water flow around the edges of the plexiglas cylinders. Light intensity is also quite low relative to field conditions and constant temperatures for day- and night-time have been used instead of daily temperature profiles. Whit these limitations in mid I suggest to downplay the achieved realism in these systems and concentrate on their reproducibility/reliability. Unfortunately, the very low level of replication cannot provide a high level of certainty that the low variability observed in the response variables did not arise by chance. Hence, the drawn conclusions are way too strong for the presented data.

We agree to downplay the aspect of realism in the mesocosm studies. Surely, the mesocosm system is artificial and its design may impact the magnitudes of the DIC percolation and the pCO_2. As correctly pointed out by referee #5, DIC percolation to the groundwater in the field is minimal in the growing season. In the mesocosms, however, infiltration is forced downwards by a combination of (unrealistically) high infiltration rates and suction at the lower boundary. The applied light intensity and the constant day- and night temperatures were not based on an close approximation to realism either, but on decade-long experience on the best conditions for growing plants in climate chambers (unplanted mesocosms were subjected to the same conditions as mesocosms with growing plants in subsequent experiments). Our results do not indicate an impact of the imposed (unrealistic) conditions mentioned in the above. However, we realize that the comparison with other (mesocosm and field) studies is weakened by the fact that no results from our own field site are available.

We further agree that too much focus has been put on the representativeness of the mesocosm system, for which documentation of the variability is essential and for which results from only two mesocosms are a limitation. We shall instead focus on the design of the mesocosm system and highlight its value for the study of processes and mechanisms for which no replicates are needed.

We have to correct referee # 5 in the following points:

- *Evapotranspiration*

Opposed to the comment by referee #5 the mesocosm system allowed us to determine the evapotranspiration. This has for reasons of simplification not been reported in the current version of the MS. Weekly evapotranspiration rates were estimated from the difference between calculated

and measured mesocosm weights. The calculated weight of a given mesocosm was obtained by subtracting the water removal due to effluent and sampling from the sum of the mesocosm weight and the volume of irrigation water.
In addition to evapotranspiration, the applied mesocosm system allowed for measurements of gaseous exchange of CO_2 which due to the same reasons of simplification have not been mentioned in the MS either.

- *Preferential Flow*

In section 2.1 we state "…This resulted in a diameter-to-length ratio of the packed soil column of 0.244 which is close to the suggested ratio of 0.25 for minimization of boundary effects (Lewis and Sjöstrom, 2010)." Hence, contrary to referee #5´s remark, we thought about the minimization of boundary effects and consequently adjusted the column diameter very close to the optimal one described in the literature.

We suggest to shorten the comparison with other studies on page 9956 as follows: "Our results are n agreement with a reported pCO2 of 0.5–1% at 20 cm depth in a fallow silt loam field at soil temperatures of 5–20 ℃ and topsoil VWCs of 15–30% (Buyanovsky and Wagner, 1983) and with 0.3–0.9% pCO_2 at 15 cm depth in loam (temp. and VWC not reported) (Smith and Brown, 1933). The average [DIC] in our study was similar to the [DIC] in the percolate from sandy forest soils with a topsoil pH of 3.8–4, but was far below the [DIC] in the percolate from croplands and grasslands (Kindler et al., 2011; Walmsley et al., 2011; Siemens et al.,2012)…"
For a description of the usefulness of the mesocosm system, please see our answer to referee#3's point 3. For a suggestion to the in-text revision of the conclusion, see our answer to referee#3's point 11.

2) Presentation. The title is too broad, as no other C fluxes have been quantified except DIC in drained water. At line 12 (page 9949), the sentence somewhat makes the reader to think that microbial respiration rates will be presented in this paper – which is not the case. The depths for the gas sampling ports presented at line 25 (page 9950) are not the same with those presented in Fig. 3 for pCO2. Overall, the English could also benefit from a bit of polishing, e.g. "Design and packaging of mesocosms" could be replaced with "Design and setup
We agree that the title could be improved by being more specific. The revised title would be: "Technical note: Mesocosm approach to quantification of dissolved inorganic carbon fluxes across the vadose zone". Regarding the statement on microbial respiration rates please see our answer to point # 3 by referee #3.
We recognize referee #5´s comment on that the reader may expect to see data on microbial respiration after reading lines 9-13 on page 9949 . Due to this, and the critics in point 3 by referee #3, we propose to reword the critical passages in the MS as follows: "... Application of mesocosms for research on CO_2 fluxes in soil has mainly been focused on studies of gaseous effluxes (e.g. Lin et al., 1999; Cheng et al., 2000; Schnyder et al., 2003) while little attention has been paid to investigation of the pCO_2 with depth in large-scale unplanted mesocosms (Lawrence and Hendry, 1995; Hendry et al., 2001) and to DIC leaching."
It is true that given sampler depths in the text are not the same as in Fig. 3., which is why we wrote in the figure legend that the given depths in the figure are interpolated depths. In a revised manuscript, we may add the word "linearly" to highlight that the presented data are derived from the measurements at the depths given on page 9950.
We do not generally believe that the language of the manuscript is poor, but agree to give it a second critical overhaul.

References

Jouquet, P., Huchet, G., Bottinelli, N., Thu, T. D., and Duc, T. T.: Does the influence of earthworms on water infiltration, nitrogen leaching and soil respiration depend on the initial soil bulk density? A mesocosm experiment with the endogeic species Metaphire posthuma, Biol Fertil Soils, 48, 561-567, 2012.

Kampichler, C., Bruckner, A., and Kandeler, E.: Use of enclosed model ecosystems in soil ecology: a bias towards laboratory research, Soil Biol Biochem, 33, 269-275, 2001.

Houghton, R. A.: Balancing the Global Carbon Budget, Annu Rev Earth Pl Sc, 35, 313-347, 10.1146/annurev.earth.35.031306.140057 2007.

Kessler, T. J., and Harvey, C. F.: The global flux of carbon dioxide into groundwater, Geophys Res Lett, 28, 279-282, 2001.

Raich, J. W., and Potter, C. S.: Global Patterns of Carbon-Dioxide Emissions from Soils, Global Biogeochem Cy, 9, 23-36, 1995.

Reichel, R., Rosendahl, I., Peeters, E. T. H. M., Focks, A., Groeneweg, J., Bierl, R., Schlichting, A., Amelung, W., and Thiele-Bruhn, S.: Effects of slurry from sulfadiazine- (SDZ) and difloxacin- (DIF) medicated pigs on the structural diversity of microorganisms in bulk and rhizosphere soil, Soil Biol Biochem, 62, 82-91, 2013.