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Discussions

Interactive comment on "Intra-aggregate CO_2 enrichment: a modelling approach for aerobic soils" *by* D. Schlotter and H. Schack-Kirchner

Anonymous Referee #1

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GENERAL COMMENTS

In the discussion paper 'Intra-aggregate CO2 enrichment: a modeling approach for aerobic soils' the authors aim to approach the question of how soil intra-aggregate and total CO2 storage is influenced by soil aggregates. They conducted a model simulation study in which they combined a one-dimensional gas diffusion model in the inter-aggregate pore space with a cylinder diffusion model for the intra-aggregate pores. They conclude that, independent of the level of water saturation, the effect of intra-aggregate CO2 accumulation on soil CO2 storage is negligible.

In general, the paper is well and concisely written. The model approach itself seems sound (i.e. Sects. 2.1 and 2.2, 2.3.1 and 2.3.2). However, I have reservations towards the chosen model parameterization. The authors aim to model a depth profile of total

and intra-aggregate CO2 storage for a 0-1m soil column, with a central result shown in Fig. 8, and soil depth-specific conclusions found in the discussion and conclusion section of the discussion paper. The authors assume an exponential decrease of soil respiration, which is reasonable. However, they also assume (Sect. 2.3), throughout the upper soil meter, a constant total porosity of 50%, consisting of a constant inter-aggregate porosity of ∼20% (which is always air filled) and a constant intra-aggregate porosity of 30% (which is mostly water filled). More typically, however, bulk density increases with soil depth causing a decrease in total porosity with soil depth, soil water contents increase with soil depth causing a decrease in air-filled porosities and consequently gas diffusion coefficients, and inter-aggregate porosity decreases with increasing depth. For example, in an aggregated forest soil in Brazil, total porosity decreased from 0.63 cm3 cm-1 at 0-30 cm depth to 0.53 cm3 cm-1 at 50-100 cm depth, simultaneous with a decrease in inter-aggregate porosity from 0.26 to 0.13 cm3 cm-1 and a decrease in wet-season air-filled porosity from 0.34 to 0.16 cm3 cm-1. Consequently, the soil gas diffusion coefficient decreased from 0.028 to 0.013 cm^2 s-1 along the same depth gradient (Davidson & Trumbore, 1995). Or, in an aggregated forest soil in Panama, total porosity decreased from 0.78 cm3 cm-1 at 5 cm depth to 0.57 cm3 cm-1 at 125 cm depth. Along the same depth gradient, the percentage of inter-aggregate pores decreased from 30% to 5%, and air-filled porosity decreased by a factor 4-5 depending on season, again causing a severe decline of the gas diffusion coefficients with depth (Koehler et al., 2010). These common depth patterns are not considered by the authors during model parameterization. I suppose that the soil depth-specific results as well as some conclusions (please see in the specific comments below) are quite strongly influenced by these assumptions.

Also, the authors conducted just one model run for each soil chemical system concerning the distribution of air- and water-filled porosity inside the aggregates. It would be interesting to see how the model results are affected by inclusion of further air-filled pores inside the aggregates, and consequently higher respiration rates. The amount and distribution of air-filled pore fractions inside aggregates are highly variable depending on soil drainage status (Carminati et al., 2008). That paper (Carminati et al., 2008) suggests that air-filled pores inside aggregates are often more common than suggested by the parameterization of the authors of this discussion paper, and this assumption may strongly affect the results and conclusions presented in this discussion paper as well.

In conclusion, this is a well-conducted and well written study. The analysis is highly relevant for the biogeochemical and soil trace gas model community. However, as it is, the results and conclusions are based on just one and partly rather untypical model scenario. I recommend that the authors need to conduct a major revision in this aspect of model parameterization. I advise that they need to add further model scenarios to test the sensitivity of the results towards the respective changes, and the robustness of their conclusions. One needed case study is a model simulation in which they parameterize the model according to above mentioned depth patterns (i.e. depth-specific profiles of total, inter- and intra-aggregate and air- and water-filled porosities, as well as gas diffusion coefficients). Also, it would be nice to see additional results when assuming larger air-filled proportions and hence aerobic soil respiration rates inside the aggregates. Once the discussion paper is revised accordingly, the manuscript can be suitable for publication in Biogeosciences. However, it is essential that this important aspect is dealt with before further consideration for publication.

SPECIFIC COMMENTS

Abstract

- P14796/L11-12: I am thinking how it can be concluded that this level of CO2 partial pressures is reached 'independent of water saturation' if model sensitivity to changes in soil moisture/air-filled porosities etc. were not assessed, but rather depth- and time-constant air- and water-filled porosities were assumed? - P14796/L16-17: Please test this statement by conducting further model simulations as advised in the general comments. - P14797/L9-11: I find this concept of 'water-filled intra-aggregate pores' some-

what too simplified. While, in general, this is the tendency water- and air-contents inside aggregates change considerably with drainage conditions (Carminati et al., 2008). Please adapt this statement to make it less general.

Materials and methods

-P14799/L5-6 and L11/12: Please include references for these statements. - P14802/L4: Was this 'cylinder diffusion model' newly developed by the authors, or is there a reference? -P14802/L5-7: While this is shown in Fig. 3, please be explicit here about which proportion of air and water you assumed for the intra-aggregate pores. P14804/eq.8: Is there a reference to include for this equation?

Results

-P14805/L8-10: I suppose that this statement depends strongly on the assumed depth distribution of mainly total porosity, inter-aggregate porosity and diffusion coefficients, which were assumed to be depth constant in this study. Please test this statement and expand on this by adding further model scenarios as advised in the general comments. Currently, assuming depth-constant soil porosities and diffusion coefficients and exponentially decreasing soil respiration rates, this result given here seems to be quite obvious to me. - P14805/L22-27: Also this result seems to be rather obvious to me considering that the intra-aggregate pores were assumed to be nearly completely water filled. Please test and expand on this by adding further model scenarios as advised in the general comments. - For all figures, I suggest to transfer the legends from the figures into the figures captions.

Discussion

-P14806/L25-28: In this article to which the authors of the discussion paper refer (Koehler et al., 2010), I don't find a quantitative statement in terms of 'strength' of CO2 enrichment. The authors only make a comparative statement arguing that, at deeper depths, the inter-aggregate porosity and soil gas diffusion coefficients are smaller resulting in 'a stronger CO2 accumulation in the intra-aggregate pores' (Koehler et al., 2010). In the current discussion paper, the authors question this suggestion by running a model with depth-constant inter-aggregate porosity and diffusion coefficients, which is in contrast to the conditions observed in the respective study (Koehler et al., 2010). The authors of the discussion paper add that 'strong CO2 enrichment in the intra-aggregate pores at deeper depths' seems only possible if 'the respiration inside aggregates is high at these depths, or if the diffusive conductivity of the intra-aggregate pore space is extremely low'. However, again, in their model simulations they assumed that diffusion coefficients remained constant across depths. What are 'extremely low' diffusive conductivities? In the discussed study (Koehler et al., 2010), for example, soil gas diffusion coefficients at 1 m depth were around 0.5 mm^2 s-1 and smaller still at 2 m depth. Is this 'extremely low'? Please revise, and test these statements with further model simulations as advised in the general comments. -P14807/L16-24: Please test this statement by further model simulations. -P14807/L29-P14808/L1: I find this statement rather speculative, please consider to revise. -P14808/L11-13: I think it is difficult to conclude this from the results based on the current depth-constant model parameterization for porosities and diffusion coefficients. Please test by further model simulations.

References

Carminati A, Kaestner A, Lehmann P, Flühler H (2008) Unsaturated water flow across soil aggregate contacts. Advances in Water Resources, 31, 1221-1232.

Davidson EA, Trumbore SE (1995) Gas diffusivity and production of CO2 in deep soils of the eastern Amazon. Tellus, 47, 550-565.

Koehler B, Zehe E, Corre MD, Veldkamp E (2010) An inverse analysis reveals limitations of the soil-CO2 profile method to calculate CO2 production and efflux for well-structured soils. Biogeosciences, 7, 2311-2325.
