

Interactive comment on “An inverse analysis reveals limitations of the soil-CO₂ profile method to calculate CO₂ production for well-structured soils” by B. Koehler et al.

Anonymous Referee #2

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Review of Koehler, B., Zehe, E., Corre, M.D., and Veldkamp, E., An inverse analysis reveals limitations of the soil-CO₂ profile method to calculate CO₂ production for well-structure soils. Submitted for publication in Biogeosciences. BGD 7, 1489-1527, 2010

This is an interesting study intended to test the validity and limitations of the soil CO₂ profile method. The study is important because this method is quickly gaining popularity in the biogeosciences community, yet the intrinsic assumptions of the method have long been questioned and rarely tested. I consider this manuscript should be of interest to the audience of Biogeosciences, and in my concept it should be suitable for publication provided the authors address the comments I provide below.

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General Comments:

1. My main criticisms about the paper are the hypotheses of this manuscript; they should be re-written as they are currently difficult to understand – they require to be read more than once to make full sense. Specifically, H1 and H2 are not well stated; the structure of each sentence should be flipped around to improve their clarity. For example, I suggest to modify H1 like this: “H1: Production and fluxes of CO₂ calculated by the profile method are strongly dependent on the methods to interpolate between measurements of CO₂ concentrations.” Please do the same with H2 to improve its clarity. As for H3, I consider this is not a true hypothesis that uses the scientific method; as it is, it will not lead to ‘new’ knowledge. Thus I suggest this hypothesis be removed and the sentence added to the first sentence of the next paragraph.

2. The experimental design of this study involves soil pit sampling performed horizontally to reach target depths of 0.05, 0.2, 0.4, 0.75, 1.25, and 2 m. Typically in soil CO₂ profile studies, lateral (horizontal) transport of CO₂ is neglected, because the vertical CO₂ gradient is typically two (or more) orders of magnitude higher than the horizontal gradient. However, in my concept this assumption might be violated in this study due to an effect caused by the proximity of the soil pit itself, especially for the last two depths (1.25 m and 2 m). In theory, for these two depths the vertical distance becomes comparable or larger than the horizontal distance to the soil pit. And specifically for the case of the 2-m depth, the CO₂ gradient is steeper from the point of sampling to the wall of the pit than from the point of sampling to the soil surface. Thus I consider the discussion around the depths of 1.25 and 2 m is biased because of the soil pit, and it should be demonstrated or stated how this problem was circumvented.

3. Figure 6: after reading the manuscript and analyzing this figure, I’m still puzzled as to whether the disagreement in the different curves of this figure are caused by a wrongly estimated D or by differences in production rates in the soil profile that are not captured by their experimental design. My point is that a level of discussion should be expected around the mechanistic interpretation of the estimated CO₂ production and

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D. In my concept, the manuscript lacks mechanistic interpretations and instead focuses mostly on the mathematical explanations.

Specific comments:

Abstract. Line 12. In my concept Objective 3 (i.e., assumptions of the soil CO₂ profile method) also includes Objectives 1 and 2. Thus, Objective 3 should be re-worded to be different from 1 and 2, or else it should be deleted.

Abstract, Line 28: I don't think this finding is new. It has been long hypothesized that decaying roots, soil macropores, or animal disturbances can introduce heterogeneity in the soil profile affecting processes such as soil wetting after precipitation, and/or lateral and vertical changes in diffusivity. This finding should be mentioned as 'corroboration' of previous findings.

Introduction, Line 27. Replace "i.e. changes over time are negligible" by "i.e., changes over the time step of the calculation ("dt") are negligible."

Page 1494, Line 23. How often was soil air sampled?

Page 1495, Line 1. The authors state "three to four standard gases;" however, in the text they specify five different concentrations. Please revise to match clauses.

Section 4.4. (Conclusions) I think it is important to note what scales these findings might be relevant to. For example, are these findings relevant at the profile scale, and also when comparing sites with strongly distinct CO₂ effluxes? I don't think that would be the case. Please specify.

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