

We would like to thank the referee for the thorough, constructive and helpful comments and suggestions on the manuscript. Thank you very much for sharing your opinion and advice.

We address the referees comments in the following answer.

**General comment:**

We agree with the referee that, based only on concentration profiles, it should be difficult to determine both the profiles of the diffusion coefficients  $D$  and production  $S_t$ . However, we only inversely determine  $D$  but not  $S_t$ . The production profiles shown in the discussion paper (Fig. 5b, or Fig. 4b in the revised manuscript) are derived using the soil-CO<sub>2</sub> profile in its ‘traditional’ way using  $D$  calculated based on relationships with measured soil parameters, they do not stem from the inverse analysis.

In the inverse analysis we use an inequation which only requires that the flux divergence, which is the source/sink term in the soil-CO<sub>2</sub> profile method, shall be larger than zero. As we are using an inequation, the problem is initially underdetermined (please also compare the answer to referee 1). We gain the shape of the inversely modeled  $D$  profile but not its position on the x-axis. We can, however, determine the position of the  $D$  profile on the x-axis by making use of the integration constant (eqs. 11-13), which we determine so that the inversely modeled  $D$  equals the empirical  $D$  determined for 0-0.05 m depth (i.e. setting the upper boundary). However, using this method we only uniquely determine  $D$  while we don’t gain additional information about  $S_t$ .

It is correct that the time derivatives are not used. This is, however, not our decision but one of the central assumptions of the soil-CO<sub>2</sub> profile method suggested by DeJong & Schappert (1972; please see also the Appendix A). As we want to assess the validity of this assumption we may not change it. We agree with the referee that a more realistic simulation of soil CO<sub>2</sub> production should be realized by using a model which does describe temporal dynamics. To explicitly include this point we have expanded the final sentence of the manuscript (conclusion, Sect. 4.4, where we state that transient simulations would be needed). We have also independently (numerically) verified that the inverse method to derive  $D$  from steady state soil gas profiles works for synthetic data. We have included this independent test of the inverse method in the methodology (last paragraph of Sect. 2.2.3 in the revised manuscript), results (Fig. 6 and Sect. 3.5 first sentence) and discussion (Sect. 4.2 second paragraph, Sect. 4.4 third sentence; please also see the answer to referee 1).

**Specific comments:**

*Comment 1:*  $D$  is now defined upon the first use in the introduction.

*Comment 2:* We replaced ‘empirical’  $D$  with the term ‘physically modeled’  $D$  as opposed to the ‘inversely modeled’  $D$ .

*Comment 3:* We have clarified this methodological description.

*Comment 4:* The model residuals of linear mixed effects (lme) models should be normally distributed. If the data-sets which are statistically analyzed are strongly skewed, the above assumption is usually not fulfilled and transformations on the raw data are used to improve model performance. Diagnostic plots of the model residuals can be used to verify that the model residuals are normally distributed (Crawley, 2002).

Before analyzing the data we checked the histogram plots. If they revealed skewness we applied first a weak transformation (i.e. the square-root transformation is weaker than the logarithmic transformation and the quadratic transformation is weaker than the cubic transformation), calculated the lme-model and checked the model residual plots. If they still revealed some kind of pattern (i.e. not normally distributed) we reanalyzed the data after using a stronger transformation, to ultimately assure that the model residuals were normally distributed. In the revised manuscript, we have included both these information (i.e. the use of histogram and diagnostic residual plots) in the methodological description (Sect. 2.2.6 in the revised manuscript).

*Comment 5:* We think that the still rather high mean CO<sub>2</sub> concentrations above the soil surface are due to restricted mixing/turbulent exchange, as was also brought up by the referee. The standard error is based on the annual average of the mean concentrations measured in three plots, so the within plot variability is a bit masked which may explain why it is relatively small. Based on the referees comment, we have included the information that litter decomposition is rapid at this site, therefore there is no organic matter accumulation on top of the mineral soil. We also included during which time of the day measurements were conducted (both in Sect. 2.1.1).

*Comment 6:* We have included the units for the root mean squared errors.

*Comment 7:* All measures of variability given in the text are standard errors (mentioned on page 1503, line 16 in the discussion paper).

*Comment 8:* We agree with the referee that the soil-CO<sub>2</sub> profile method does not require  $D$  to increase monotonically with depth, and that the condition that  $D$  increases monotonically with depth does not necessarily apply for other sites, e.g. in a situation as described by the referee where a poorly permeable layer exists. The calculation step which is discussed in this part of the manuscript was an additional test to find out how the CO<sub>2</sub> profile would look like if we try to ‘force’ the inversely modeled  $D$  to follow the empirically calculated  $D$ . To account for the referees comment that the condition we encountered in our mathematical framework does not necessarily apply/hold true for other site conditions we explicitly state now in the revised manuscript that this calculation step was based on the conditions encountered in our site.

*Comment 9:* We have corrected this mistake in the respective reference.

*Comment 10:* We sampled air for concentration analysis most intensively (both in time and space) in the top soil, with 4 sampling depths in the top soil meter and sampling in an approximately 6-weekly schedule. In the second soil meter (1-2 m depth) we have two further sampling tubes. However, at these depths, wet season air sampling was often restricted due to a high groundwater level, especially during the wet season 2007, and some deep soil data are

also missing due to analytical problems at the end of dry season 2007 (please compare the legend of Fig. 3 which is Fig. 2 in the revised manuscript). Therefore, we preferred not to use the deep soil data for spatial and temporal interpolation as contour plot. Instead, we considered it more appropriate to show the single measurement points as time series. A further advantage is that, in this way, we could include the information about the duration of dry season in panel b, which facilitates the recognition of the seasonal pattern in the CO<sub>2</sub> concentrations in panel a. Due to these reasons we still prefer to split this plot as we did, using two different plot styles, and to stick to a non-transformed scale which easily allows reading of the measured concentrations. Regarding the second question: Near the end of dry season 2007, deep soil concentration measurements are missing due to analytical problems (problems with the gas-chromatographical analysis of high CO<sub>2</sub> concentrations). This is mentioned in the legend of Fig. 3 in the discussion paper.

*Comment 11:* The different CO<sub>2</sub> fluxes and production rates in Fig. 5 (Fig. 4a and b in the revised manuscript) result from the different interpolation methods of the CO<sub>2</sub> concentrations as shown in Fig. 4c and d (Fig. 3c and d in the revised manuscript). We chose to color the lines in Fig. 4 similarly as in Fig. 3. We think that this similar coloring makes it easier to recognize how a certain interpolation method influenced the model results. Therefore, we would like to keep this presentation even though it is inferior for black-and-white printing.

## References

Crawley MJ (2002) *Statistical Computing, An Introduction to Data Analysis using S-Plus*. John Wiley & Sons Ltd, Chichester, England, 761 pp.