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. Especially the idea to numerically test the validity of the inverse method to derive *D* was very helpful to improve the presentation of the inverse analysis and the results.

We address the referees comments in the following answer.

General comments:

The referee is correct in stating that we are working with an underdetermined inverse problem. We deal with this problem by introducing prior knowledge of the physically meaningful solution. The inverse solution should fulfill the criteria to both well approximate the measured CO_2 concentrations and the (independently determined) empirical/physically modeled diffusion coefficients *D*.

Our (analytical) inverse analysis is based on the fit of a function to the measured soil CO_2 profiles. We first conducted the parameter optimization without any constraint for the parameter space. Our unconstrained solution is valid if there is only one solution (i.e. set of parameters *a*, *b* and *c*, eq. 3) which well approximates the measured CO_2 concentrations (or, alternatively, if different solutions would give similar results for *D*). To test this, we repeatedly ran the optimization procedure with random starting values for several profiles (conducted for five CO_2-profiles of randomly chosen dates, 100 runs for each profile). In 71 ± 4% of all runs, the optimization ran into the same convergent solution, i.e. the one shown in the discussion paper. In the remaining cases, a different solution was reached which, however, was clearly physically meaningless as it did not reproduce the measured CO_2 profiles/did not converge. This shows that, concerning the parameter optimization, there was only one physically meaningful solution in the unconstrained case.

However, knowing the parameters *a*, *b* and *c* the solution was not yet fully determined as we still needed to determine the integration constant (eqs. 11-13) in a physically meaningful way. Before doing this, we only knew the shape of the inversely modeled *D* but not its position on the x-axis (i.e. eq. 13 is an inequation which reflects the point addressed by the referee that the problem is underdetermined). The next calculation step is crucial: We determined the integration constant by setting the upper boundary ($z = 0$) of the inversely modeled *D* to the one which was experimentally determined for 0 to 0.05 m depth (please compare page 1501, lines 8-11 in the discussion paper). We now knew both the shape of *D* and its position on the x-axis, and the solution was uniquely determined.

Please note that the inverse method could only be useful to independently determine *D* for data from sites where the assumptions of the soil-CO_2 profile method would be met (this was not the case in our study). Please also note that we can only derive *D* from our calculations, but not the production S_t.

In the constrained simulations, the problem is indeed not uniquely determined but several solutions may be reached depending on the starting values during the optimization procedure.

In this constrained case, we chose the solution that best reproduced our empirical *D* (please note that we used the same starting values for all simulations). Due to this criterion, the solution was again well constrained. The constrained parameter optimization was only an additional step to find out how the CO_2 profiles which correspond to a *D* as observed would look like, and to test if that CO_2 profile is consistent with our suggested explanation that an additional CO_2 sink is missing in the mathematical description of the soil-CO_2 profile method.

We followed the suggestion of the referee to show with artificial profiles of *D* and S_t and numerical calculations that the inverse method to calculate *D* from steady state soil gas profiles works. 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). We also expanded the methodological description of the inverse analysis (Sect. 2.2.3) to include the information which we gave above (e.g. more details on how the simulations were conducted, e.g. concerning the starting values).

Specific comments:

Comment 1: We have revised the respective sentences to account for the referees comments. We did, however, not follow the suggestion to pool the references because we would like to make clear that the method was proposed by DeJong & Schappert, and separate this information from references to studies in which it has been used subsequently.

Comment 2: We agree with the referee that, though not representative for that depth, these concentrations are real and so should be included (calculations and figures were updated). It did, however, not make a pronounced difference as the concentrations in the respective pit and depth were clearly larger than in the other pits on only a few dates.

Comment 3: D_0 is now defined.

Comment 4: Yes, in a second step *D* was constrained to decrease monotonically with depth as was the case in our site according to the calculated empirical diffusion coefficients *D* (Millington & Shearer, 1971; please see Sect. 2.1.6, Fig. 4a, b in the discussion paper). This pattern of a decreasing *D* was consistent with the observed decreasing air-filled porosity with increasing depth in our site (Table 1). We agree with the referee that this situation can not be generalized. There may be conditions where *D* is larger in deeper soil layers than above (e.g. reported for 8 m deep soil profiles in an Amazonian tropical forest). In the Brazilian site, macroporosity was larger in deeper soil layers than above (but clearly smaller than in the surface soil layers; (Davidson & Trumbore, 1995). The decreasing *D* with depth observed in our site, however, is typical for several ecosystems on soils of similar texture throughout the profile, where soil porosity decreases and soil water content increases with depth. Based on the referees comment we have now clearly stated in the respective sentence that this constrained calculation was conducted because the pattern of a decreasing *D* with increasing depth has been observed in our site, to avoid an understanding that this might be a generally expected pattern which is not the case.

Comment 5: We have conducted this simulation and included the result in the respective figure (Fig. 1 in the revised manuscript).

Technical corrections: We have conducted the technical corrections.

References

Davidson EA, Trumbore SE (1995) Gas diffusivity and production of CO_2 in deep soils of the eastern Amazon. *Tellus,* **47,** 550-565.