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 comments:

Comment 1: We rewrote hypotheses 1 and 2, and deleted hypothesis 3. Following the referees suggestion, the former hypothesis 3 was included in the final sentence of the last paragraph.

Comment 2: We agree with the referee that diffusive gas loss through the pit wall must be considered if setting up a pit for gas sampling. Based on the results of earlier studies we argue that, with our chosen length of gas sampling tubes, we minimized a deep soil underestimation of concentrations to < 5% of the real concentrations at the respective depths. It is important to consider that not only the concentration gradient determines the diffusive flux, but also the diffusion coefficient *D*. Therefore, besides the parameter 'distance from the pit wall' also the magnitude of *D* is relevant to judge about the severity of the pit wall effect at a given depth. In our soil, owing to the small total and inter-aggregate porosity and a water-filled porosity which exceeds ~90% throughout the year (Table 1), *D* is very small at 1.25 and 2 m depth (Fig. 4a and b which is Fig. 3a and b in the revised manuscript). We chose our length of soil tubing based on the results of two earlier, similar studies in tropical forest soils where the pit wall effect has been specifically assessed. In the first of these studies, conducted in a well-drained Oxisol in the Brazilian Amazon, air-sampling tubes of different lengths (0.45, 0.90, 1.35, 1.80 and 2.70 m) were horizontally inserted into the pit wall at 5 m depth. The measured CO_2 concentrations increased with increasing tube length, and the asymptote concentration was estimated using an exponential fit to the measured concentrations. Using 1.80 m tube length, 80-95% of the asymptotic dry season concentration was reached (Davidson & Trumbore, 1995). In that Brazilian soil at 4-5 m depth, inter-aggregate porosity was considerably larger (33%, please see Table 1 in (Davidson & Trumbore, 1995)) than in our soil at 1.25 and 2 m depth (<6%, Table 1); also dry season air-filled porosity was larger with ~55% compared to our site where it was only ~11% (Table 1). Therefore, using the same model to estimate *D* which we used in our study (Millington & Shearer, 1971), their *D* at 4-5 m depth was larger than ours at 2 m depth (Fig. 3a and b in the revised manuscript). Similarly, 95% of the estimated asymptotic value was reached when using 1.8 m long tubing at 1.5 m depth in Oxisol forest soils in Costa Rica (Schwendenmann *et al.*, 2003; Schwendenmann & Veldkamp, 2006). Also in that soil at 1.5 m depth, *D* was slightly larger than in our soil at 1.25 and 2 m depth (calculated using the same model of Millington & Shearer, 1971). Based on these study results, conducted in similar aggregated tropical soils but with larger *D* than ours at 1.25 and 2 m depth, we expect that a deep soil underestimation due to inevitable diffusive losses through the pit wall was minimized to <5% in our soils. This shall not have introduced a considerable bias into our values or interpretation. We have inserted one sentence in section 2.1.2 where we address this issue.

Comment 3: The two production time series plotted in Fig. 6 (Fig. 4c in the revised manuscript) have been calculated applying the soil-CO_2 profile method using either a sigmoidal or an exponential function to interpolate between the measured CO_2 concentrations. The same empirical/physically modeled *D* has been used in both simulations (calculated using the model of Millington & Shearer (1971); the results are therefore not related to our inverse analysis). The figure shows that the CO_2 production calculated with the soil-CO_2 profile method depends strongly on the input of CO_2 concentrations. Even if these differ just slightly (compare e.g. Fig. 4c and d the blue and red curves, Fig. 3c, d in the revised manuscript) the modeled CO_2 production may differ greatly. This point is discussed in the discussion paper on page 1508, lines 19-21. The figure illustrates, therefore, purely a mathematical issue which is not influenced by our estimated *D* or our experimental setup. In order to put more emphasis on the aspect that the chosen interpolation function is a sensitive parameter, influencing model performance in an important manner, we have included this statement in the final section of the manuscript (please see Sect. 4.4). We have also combined Figs. 5 and 6 of the discussion paper into one graph (Fig. 4 in the revised manuscript) as they address this same aspect, and in this way the connection should become clearer.

In our opinion, we do have an amount of mechanistic discussion around our estimated diffusion coefficients (e.g. discussion paper page 1509, lines 21-23, Sect. 4.3 second paragraph). Concerning the modeled CO_2 production, however -as the referee points out- we did not include a mechanistic interpretation. Having analyzed and tested the soil-CO_2 profile method in detail we conclude that the model does not correctly describe soil CO_2 dynamics in our well-structured soils (Sects. 4.3 and 4.4). The modeled CO_2 production rates were not reliable for our site, and we did not use them as such, which is the reason why we did not discuss them mechanistically either. We do not think that this is a lack of the manuscript, because the scope of this work is to contribute to the discussion about apparent problems of the soil-CO_2 profile method, and we intended to do this using mathematical methods and explanations.

Specific comments:

Comment 1: We have excluded the third objective from the listing as we agree that the meaning is already included in the first and second aspect of our objective, and is also mentioned later on in the abstract.

Comment 2: We reworded this sentence according to the referees suggestion.

Comment 3: We have decided to exclude the explanatory part of the sentence as it was not really needed.

Comment 4: The measurements, including soil air sampling, were conducted in a 6-weekly schedule (mentioned in the paragraph about the experimental design which precedes the method description of gas measurements, page 1494 line 14/15 in the discussion paper).

Comment 5: We have reworded the section to clarify this point.

Comment 6: In our opinion, our suggested conclusions are not tied to a certain spatial scale as they affect the mathematical concept of the soil-CO_2 profile method or its solution methods in general.

References

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

Millington RJ, Shearer RC (1971) Diffusion in aggregated porous media. *Soil Science,* **111,** 372-378.

Schwendenmann L, Veldkamp E (2006) Long-term CO_2 production from deeply weathered soils of a tropical rain forest: evidence for a potential positive feedback to climate warming. *Global Change Biology,* **12,** 1-16.

Schwendenmann L, Veldkamp E, Brenes T, O'Brien JJ, Mackensen J (2003) Spatial and temporal variation in soil CO_2 efflux in an old-growth neotropical rain forest, La Selva, Costa Rica. *Biogeochemistry,* **64,** 111-128.