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Biogeosciences Discussions

Interactive comment on "An inverse analysis reveals limitations of the soil-CO_2 profile method to calculate CO_2 production for well-structured soils" *by* B. Koehler et al.

Anonymous Referee #1

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General Comments The authors present a useful study in which they seek to determine the reasons for inconsistencies between soil efflux measurements using surface and subsurface approaches. They present a possibly useful mathematical framework for calculating diffusivity based on concentration profiles, which are considerably easier to measure in situ. Finally, based on their analysis, they conclude that the description of gas production and transport in the sub-surface at this time is incomplete and other processes must be considered to fully describe the subsurface gas regime.

Overall I found this manuscript interesting to read, carefully prepared and feel that it would be well suited to the readers of Biogeosciences. This means my comments on

the paper are minimal. My only substantial comment is that I have some reservations concerning the inverse mathematical approach used to determine diffusivity. It would be my first instinct to say that the relationship derived for inverse estimation of D is underdetermined, leading to erroneous results. I may be wrong, but to strengthen the applicability of the method I would suggest a portion of the paper be used to show that the inverse method works on data derived using gas diffusion theory. This would be fairly straightforward to do by solving the steady-state diffusion equation with various polynomial equations substituted for the diffusion and production functions. Without this analysis it is hard to say whether the inconsistencies in flux measured by the profile method vs. the chamber method are because of incomplete knowledge of subsurface processes or simply a mathematical artifact of the inverse analysis.

Some specific comments are listed below:

Page 1491 Lines 12-17– The author should make it clearer what is meant by "Mathematical models" (Line 12) since the CO2 profile method is also a mathematical diffusion-production model. Also give a few examples of the parameters (Line 15) that you would need to run such a model. Line 18 – This sentence probably isn't strictly needed, perhaps you could just pool all of the profile method references in with DeJong and Schappert.

Page 1495 Lines 3-4 – It isn't a requirement for soil gas concentration to monotonically increase with depth so removing the consistently higher concentration data points at 5 cm is likely causing a loss of information from this plot. This increase could be caused by decreased diffusivity due to a layer of clay in the soil or potentially an area of high respiration. Unless this is a sampling artifact I think it should be included in the data.

Page 1500 Eq. 10 – Define D0 after equation

Page 1501 Lines 13-15 – Should it say here that D was constrained to decrease monotonically with depth? This is what the figure 4 suggests and what is said on Page 1506. Similar to page 1495, it isn't a requirement of the physical model that D should de-

crease monotonically, is there a possibility that D deep in the profile could be higher than near the surface due to pore connectivity?

Page 1504 Figure 2 – It would be nice to see what a 20% decrease in soil diffusivity would do to radon. Are the effects of +20 and -20 symmetric?

Finally some technical corrections: Page 1492 – Line 17 – . . .method" which is used to calculate. . . Page 1494 – Line 11 – "among" should be "between" Page 1496 – Line 10 – "is" should be "was" Page 1506 – Line 22 –"implying" should be "applying" Page 1510 – Line 20 – remove "e.g."

Interactive comment on Biogeosciences Discuss., 7, 1489, 2010.