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***Interactive comment on* “Measuring and modelling the isotopic composition of soil respiration: insights from a grassland tracer experiment” by U. Gamnitzer et al.**

Anonymous Referee #1

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The manuscript “Measuring and modeling the isotopic composition of soil respiration: insights from a grassland tracer experiment” by Gamnitzer et al. is an innovative exploratory exercise to reveal physical mechanisms behind, what the authors describe as, transient dynamics in the flux of $\delta^{13}\text{CO}_2$ from soil. The information presented is highly relevant to the audience of Biogeosciences and especially for the audience of the forth coming special issue “Stable isotopes and biogeochemical cycles in terrestrial ecosystems”. This article should be accepted for publication after the changes outlined below are implemented.

Main comments: I think the conclusions the authors draw about the modeling results

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are largely overstated. On page 100 line 24, the beginning of the discussion, the authors state they provide “direct” evidence for isotopic disequilibrium effects when, in fact, they provide the exact opposite. The results are indirect because the actual measurements of the system, especially of dissolved CO₂, were not taken. Related to this point, there are too many assumptions made in the modeling process for these results to be considered conclusive (I discuss below the assumptions made regarding static diffusion with depth, homogenous soil pH, and the “known” respiratory source). In order to provide direct evidence for the dynamics presented in this article, a sophisticated experiment would need to be designed with precise hypotheses. The results, however, are provocative, and the value of this research will primarily lie in methods development and formulation of new hypotheses. Therefore, in the revision the conclusions as currently stated should be de-emphasized.

I found the characterization of diffusion in the soil profile severely insufficient. There have been numerous articles published concerning the estimation of the diffusion of CO₂ in porous media (Moldrup et al. 2000; Hashimoto et al 2002; Davidson et al. 2006; Resurrecion et al., 2008; Koehler et al., 2010; Vargas et al., 2011). First off, I think it is important to recognize that most of the reports on this subject are actually providing estimates of “effective diffusivity”- they are not direct measures. These models essentially rely on soil air porosity (as affected by soil moisture and temperature) and an estimate of tortuosity- the actual path of CO₂ is unknown. However, the authors provide no justification for the model they chose (Millington-Quirk) and they don’t include in their discussion how Ds model choice might affect the Ds values they used in the respiration model and the subsequent impact on their allocation of variation in fractionation. The paper will be improved by considering multiple Ds models which should be included in the revision.

I found the characterization of advection in the article to be misleading. The authors essentially deal with advection as an artifact of chamber methods (sensu Phillips et al. 2010). The simulation does not address wind pumping (Takle et al., 2003 & 2004;

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Massman, 2006; Massman and Frank, 2006), advection as described by Lewicki et al. (2003) or as described by Camarda et al.(2007). In the revision, the discussion of advection should be limited to chamber measurement artifacts.

I think the authors need to be very careful in the assumption that the open chamber measurements of Gamnitzer et al. (2009) represent estimates that have not been influenced by “disequilibrium effects”. It is also likely that many transient fractionation events are in effect, cancelling each other out (Nickerson and Risk, 2009). This introduces a level of uncertainty in the subsequent modeling estimates and general conclusions that is not really discussed in the paper. I recognize this is difficult to balance. One of the real strengths of this paper is the use of field data rather than a simulated dataset. However, for this paper, it is important to be transparent in the modeling process so that readers are able to judge the impact of the findings appropriately.

The assumptions behind soil input parameters are another weakness in the modeling analysis, specifically, the assumption of static porosity and pH from the surface to a depth of 25 cm. I would expect the variability in porosity with depth to have a significant impact on the diffusivity of CO₂ at different depths, yet, the model treats the soil as an homogenous block. This criticism also applies to soil pH. The pH measurements in soil can be highly variable (Hinsinger et al., 2009), especially when considering the rhizosphere, ostensibly where we see much of the CO₂ exchange in and out of solution. Again, the manuscript does not address the impact of this assumption on the model results. Without direct measurements of the variability in pH the authors cannot support the conclusion regarding the importance of dissolution fractionation of $\delta^{13}\text{C}_{\text{CO}_2}$ that occurs during CO₂ and its subsequent impact on estimates of $\delta^{13}\text{C}_{\text{R}}$.

General notes on manuscript construction: In general, I found the manuscript fairly underdeveloped. I think the authors should take their time in crafting the manuscript so that readers may appreciate the work involved in this project. The introduction begins with a discussion of the isotopic signal of ecosystem respiration and the impact of the isotopic signature of soil respiration on these estimates. Then the following discussion

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relates only soil respiration issues, only to come back to ecosystem respiration at the conclusion of the introduction. This paper is primarily about soil respiration and I am not sure of the relevance of ecosystem respiration, especially since there is no reference to potential plant respiration effect throughout the text. While it is clear from fig 2. that there is an abiotic mechanism behind the depleted isotopic signal in respiration, it is difficult to discern what is actually being investigated (ecosystem or soil respiration).

Please spend some time on differentiating between the word use of disequilibrium effect and transient effect in the text. Often times you used transient as an adjective then later on as a noun (be careful with the use of the word transient as a noun because it also has another more common meaning). I think when you write “transient effects” you simply mean the system is not at steady state.

I would suggest the use of $\delta^{13}\text{C}_{\text{sub(Rs)}}$ (isotopic signature of the carbon source respired) and $\delta^{13}\text{CO}_{\text{sub(2)}}$ to distinguish between source and flux.

Specific comments/recommendations Abstract: Page 84

Line 19 Biogeosciences asks you to avoid using references in the abstract. This reference is unnecessary and should be removed. Introduction: Page 85

Lines 14-15. Certainly there have been more investigations (especially in the field) on this subject: Susfalk et al., 2002 ;Millard et al., 2008; Kayler et al., 2008, Kayler et al., 2010; Masey et al., 2009.

Line 24. I thought Subke et al., 2009 was a quantitative explanation. Thus, this sentence and the previous contradict each other. Re-write lines 24-25 to clarify this.

Page 86

Line 3 what precisely do you mean by enhance here? Be more specific with your word choice.

Lines 11-12 Bowling et al. (2009) is not in soil, but rather in snow over soil- a potentially

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large difference in dynamics will result. There have not been many studies that address advection and $\delta^{13}\text{C}$ although these two come to mind: Camarda et al., 2007; Kayler et al., 2010.

Line 22 Which disequilibrium effect? You have outlined several before this and it is difficult to gauge what will be discussed in the following paragraph.

Page 87 Line 2: I would include Koehler et al. 2010 here. Line 5. There have been several field and experimental papers that you should include in your discussion.

Lines 13-15 The sentence construction of this sentence needs improvement.

Line 16 -29 This paragraph needs a thorough makeover. It is not clear at all what you are doing. Only by reading the article through and this paragraph several times over could I make sense of this paragraph. You should also specify at this time which dataset you use as the “true” respiratory signal.

Lines 16-18 I think you want to specify you are only investigating these three phenomena. The way this sentence currently reads is that the disequilibrium is attributed only to these phenomena which I don't think is what you are trying to convey.

Line 20 perhaps specify the data as ecosystem respiration data or as the isotopic signal of ecosystem respiration.

Line 22 It is not clear that you are referencing the older study and not the present one.

Page 88

Line 12 Can you describe how shoot respiration is modeled in the equation set? It is not clear to me. Why don't you discuss the shoot dynamics at all in the paper? Are they just stable? If it is something you describe in the previous paper then mention it here (or better yet in the introduction).

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Line 1 Please add the fractionation factor for each process you model.

Line 7 Define “Volume flux”- how can a volume have a flux?

Line 11 Please justify the use of this model. As described above, you should also include other possible models to see how it impacts estimate of CO₂ diffusivity.

Line 20 What value (s) did you use for n?

Page 91

Line 1 Do you describe the soil somewhere?

Line 5 equation 11 needs to be displayed much better- it is a bit confusing the way it is currently stacked.

Line 11 What kind of impact on diffusion estimates do you expect from neglecting the gravel influence? The impact was substantial for Davidson et al., 2006. You should discuss this in the “discussion” section as well.

Line 17 Reiterate that you are measuring ecosystem respiration

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Line 2-3 Why did you measure concentration and isotope ratio on separate instruments? What kind of error do expect from that- especially if you use this data for Keeling plots. You should definitely have a full analysis of the error in your estimates. Perhaps the range in error is negligible since you are labeling but please discuss this in your revision.

Line 11 Perhaps a brief description of the chambers is necessary, explicitly stating that the chamber is opened to the atmosphere, hence, the name “open top”.

Page 93

Line 11 Isn't 12 minutes a fairly long time for a chamber measurement? In the discussion you should describe how the time of measurement will impact the degree of

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fractionation by each mechanism you test.

Line 12 calculate the turnover time of the chamber headspace.

Lines 12-15 Isn't this a leak? How can you be sure that the isotopic signal of the CO₂ entering the chamber from the leak is the same as the surrounding atmosphere? Did you measure it? Couldn't this be a diffusive-advective flux into the chamber- will it affect the isotopic composition? You should state your assumptions accordingly.

Line 23 Pataki et al., 2003 is a better reference for terrestrial ecosystem research. Which regression did you use? Why did you use the keeling plot versus miller-tans? How did you quantify the error in your measurement? There is plenty of research (Ohlsson et al. 2010, Zobitz et al., 2006; Kayler et al., 2010, Nickerson and Risk, 2009) that discusses mixing model theory and application, please justify your model choice.

Page 94

Line 5 What do you mean advection was implemented? You mean it was modeled as...something or perhaps characterized by...? Again, I would stress here that you are investigating advection as a measurement artifact.

Page 95 Line 8 Do you really mean "replacing"? Or do the two end-members mix?

Line 16 What exactly does "was adapted" mean here? Was it calculated somehow differently? Why did you have to do this?

Lines 26- line 2 Page 96 You should include in your table of input parameters the different partitioning of carbon with time (I can only assume it changes over the day). Also, what did you use for the isotopic composition of these different sources? Does this matter for the model? I think this methods section is really incomplete. Additionally, how do these assumptions affect the fractionation partitioning, if at all?

Page 96 Line 14 This information should not be in the results section but in the methods. You assume that this isotopic signal is the real value of $\delta^{13}\text{C}_{\text{sub}}(\text{Rs})$ and you

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use it to compare with the “disequilibrium” data set.

Page 97 Lines 4-5 What is the Bowling et al. study in snow referencing here?

Page 98 Lines 18-19 Which mechanism are you discussing here? And, what is a “disequilibrium tracer flux”?

Line 24 Use a more precise word than “attainment”- you are discussing a time delay.

Page 99

Line 24 This study does not provide direct evidence.

Finally, be consistent with your units of CO₂ concentration. You often use mole fraction to quantify CO₂ but you use different units in figure 3. Personally, I prefer $\mu\text{mol mol}^{-1}$.

References used above that are not included in the current ms:

Davidson, E. A., Savage, K. E., Trumbore, S. E., and Borken, W.: Vertical partitioning of CO₂ production within a temperate forest soil, *Global Change Biology*, 12, 944-956, 2006.

Hashimoto, S., and Suzuki, M.: Vertical distributions of carbon dioxide diffusion coefficients and production rates in forest soils, *Soil Sci. Soc. Am. J.*, 66, 1151-1158, 2002.

Hinsinger, P., Bengough, A., Vetterlein, D., and Young, I.: Rhizosphere: biophysics, biogeochemistry and ecological relevance, *Plant Soil*, 321, 117-152, 10.1007/s11104-008-9885-9, 2009.

Kayler, Z.E., E.W. Sulzman, J.D. Marshall, A. Mix, W.D. Rugh and B.J. Bond, (2008). A laboratory comparison of two methods used to estimate the isotopic composition of soil $\delta(\text{CO}_2)\text{-C-13}$ efflux at steady state. *Rapid Commun. Mass Spectrom.*, 2216: 2533-2538.

Kayler, Z. E., Sulzman, E. W., Rugh, W. D., Mix, A. C., and Bond, B. J.: Characterizing

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the impact of diffusive and advective soil gas transport on the measurement and interpretation of the isotopic signal of soil respiration, *Soil Biology and Biochemistry*, 42, 435-444, 2010b.

Kayler, Z., Ganio, L., Hauck, M., Pypker, T., Sulzman, E., Mix, A., and Bond, B.: Bias and uncertainty of $\delta^{13}\text{CO}_2$ isotopic mixing models, *Oecologia*, 163, 227-234, 2010a.

Koehler, B., Zehe, E., Corre, M. D., and Veldkamp, E.: An inverse analysis reveals limitations of the soil- CO_2 profile method to calculate CO_2 production and efflux for well-structured soils, *Biogeosciences*, 7, 2311-2325, 2010.

Maseyk, K., Wingate, L., Seibt, U., Ghashghaie, J., Bathellier, C., Almeida, P., Lobo de Vale, R., Pereira, J. S., Yakir, D., and Mencuccini, M.: Biotic and abiotic factors affecting the $\delta^{13}\text{C}$ of soil respired CO_2 in a Mediterranean oak woodland, *Isotopes in Environmental and Health Studies*, 45, 343 - 359, 2009.

Millard, P., A.J. Midwood, J.E. Hunt, D. Whitehead and T.W. Boutton, (2008). Partitioning soil surface CO_2 efflux into autotrophic and heterotrophic components, using natural gradients in soil $\delta^{13}\text{C}$ in an undisturbed savannah soil. *Soil Biol. Biochem.*, 40: 1575-1582.

Moldrup, P., T. Olesen, P. Schjonning, T. Yamaguchi and D.E. Rolston, (2000). Predicting the Gas Diffusion Coefficient in Undisturbed Soil from Soil Water Characteristics. *Soil Sci Soc Am J*, 64: 94-100.

Resurreccion, A. C., Moldrup, P., Kawamoto, K., Yoshikawa, S., Rolston, D. E., and Komatsu, T.: Variable Pore Connectivity Factor Model for Gas Diffusivity in Unsaturated, Aggregated Soil, *Vadose Zone Journal*, 7, 397-405, 10.2136/vzj2007.0058, 2008.

Susfalk Rb, W.C., Dw Johnson, Rf Walker, P Verburg, S Fu, (2002). Lateral Diffusion and atmospheric CO_2 mixing compromise estimates of rhizosphere respiration in a forest soil. *Canadian Journal of Forest Research*, 32: 1005-1015.

Takle, E. S., Brandle, J. R., Schmidt, R. A., Garcia, R., Litvina, I. V., Massman, W. J.,

Zhou, X., Doyle, G., and Rice, C. W.: High-frequency pressure variations in the vicinity of a surface CO₂ flux chamber, *Agricultural and Forest Meteorology*, 114, 245-250, 2003.

Vargas, R., Carbone, M., Reichstein, M., and Baldocchi, D.: Frontiers and challenges in soil respiration research: from measurements to model-data integration, *Biogeochemistry*, 102, 1-13, 10.1007/s10533-010-9462-1, 2011.

Zobitz, J.M., J.P. Keener, H. Schnyder and D.R. Bowling, (2006). Sensitivity analysis and quantification of uncertainty for isotopic mixing relationships in carbon cycle research. *Agric. For. Meteorol.*, 1361-2: 56-75.

Interactive comment on *Biogeosciences Discuss.*, 8, 83, 2011.

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