

Interactive comment on “Seasonal and vertical variations in soil CO₂ production in a beech forest: an isotopic flux-gradient approach” by Emilie Delogu et al.

Anonymous Referee #1

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This MS presents interesting data on soil production and respiration from the Beech forest at the Hesse experimental forest. The authors measured both CO₂ and its isotopic signal at different depths within the soil profile during key periods within the growing season. The primary objectives were to quantify CO₂ production and the corresponding isotopic flux followed by an analysis of biotic and abiotic drivers that may explain the observed patterns.

The research methods are appropriate for the objectives and having the eddy tower nearby augments the soil respiration measurements. The model is explained well and is similar to those found in the literature. The research, while not necessarily novel, will be of general interest to the readers of Biogeosciences. I have only a few minor

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comments.

I am not really convinced that the empirical model based on soil moisture and temperature “satisfactorily simulated” production and, especially, the surface flux. At a minimum the variation of the estimates should be shown graphically. The authors also might investigate for a hysteresis effect with temperature. I would expect the relationship to change with the season (see Phillips et al., 2011 in *Global Change Biology* and Zhang et al., 2015, *JGR-Biogeosciences*). Furthermore, I am not aware of another publication linking both CO₂ production and its isotopic composition at multiple depths to the hysteresis phenomena, thus adding another potential dimension to the study.

The methods and results adequately represent the measurements, and even in the discussion the technical aspects (modeling, quantification of CO₂ production and fluxes) are highlighted. However, more of the discussion could be spent on the ecological relevancy of the study. For example, in figure 5, half hour interval measurements of δ¹³C_Rs (sticking with the convention of Bowling et al. on the abbreviation of the isotopic composition of soil respiration) are shown. There is a wide range in the isotopic signal within the histograms. While the frequency distributions are nice when considering potential sources of respiration, they don't help a lot if questions concerning the drivers of soil respiration are asked. This is especially relevant when we consider short time scales, which is the big advantage to TDLs and other laser approaches. I suggest a time series analysis of these data along side environmental data measured at the eddy covariance tower could give valuable insights into the patterns at short time scales. The article by Nuria Gomez-Casanovas et al. in *Global Change Biology* (2012) is an example of this analysis within grasslands.

The results of these analyses (including any hysteresis patterns) will improve the discussion beyond the speculation written in the text (L331-342). There is no evidence provided for the “tempor[ary] increase in gpp” or the impact of microbial respiration.

A lot of the article is devoted to modeling, however, there is not much information

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provided on the diagnostics of the model. For example, how the diffusion coefficient changes over the seasons might be interesting to see in addition to how the model fit the overall data.

Finally, some of references do not look like they are formatted correctly and I find that a few key studies are missing in the manuscript. I have pointed out in the line-by-line comments below where these might fit.

L43: Perhaps you mean: "As a consequence"

L48: Brüggeman et al., 2013 Biogeosciences has a comprehensive review for this that should be included here.

L51: I find this sentence is a bit tacked on at the end. Either clarify this sentence or remove it.

L54: The review by Amundson et al. 1998 Geoderma does a nice job of introducing this

L57: What is a fractionation rate? Furthermore, while Cerling et al., 1991 lays out the theoretical foundation for diffusion in porous media, while Kayler et al., 2008 Rapid Comm. in Mass Spec and Powers et al., 2010 in the same journal provide experimental evidence.

L67: "All these variables"- not exactly clear what you are referring to

L70: Different time scales are mentioned here but they are not well defined. As best as I can tell I think you mean seasons as opposed to hourly or diurnal variations. If the hysteresis and eddy covariance evaluations are performed above, then stating that the study addressed different timescales is justified.

L72: The sentence beginning in this line needs to be rewritten; "thanks to" is a little too informal for scientific writing.

L76: is it necessary to use the term one-dimensional structure? It is not clear what you

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mean, better to explain it outright.

L85: what is a storage flux?

L97: Site description: can you report when the measurements were performed and verify that the trunk measurements were made at the same time?

L140: How long was the interval between measurements made at a particular point?

L155: I recommend a summary graph showing how well the model performed. Surely, the depth of the tube was measured on insertion and stating that this "fixed" point had to be moved in order to match the diffusive and surface fluxes does not inspire confidence.

L194: contrasting not contrasted

L246: More positive sounds as if you have positive delta values. Use less negative (which is also problematic) or values that are enriched in ^{13}C .

L253: Contamination instead of pollution

L279: different word than attack

L288: choose a different word than blamed

L305: Brüggeman et al., 2013 Biogeosciences has a very good description of CO_2 isotope from the plant to the soil. This ref. should be included here as well in L323 where Griffis 2013 is referenced.

L324: Fix this sentence water use efficiency does not beget isotope discrimination; discuss changes in stomatal conductance and assimilation capacity and rubisco.

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