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***Interactive comment on* “Environmental forcing does not induce diel or synoptic variation in carbon isotope content of forest soil respiration” by D. R. Bowling et al.**

Anonymous Referee #2

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This manuscript reports on a detailed experiment examining the stable isotopic composition of CO₂ in soil respiration and in soil air at various depths. The large dataset is used to evaluate the theoretical model based on steady-state diffusion theory and demonstrate that for the most part, the observations support the theory. The big surprise of this paper is that there were no diurnal or seasonal changes in the isotopic composition of soil respiration, in contrast to much previous work by the lead author and others indicating that autotrophic responses to environmental conditions can impose a physiological signal in the d¹³C of the respired CO₂. Indeed, the title of the paper points out this important observation, but most of the paper is focused on the comparison between observations and theoretical expectations. Unfortunately, the re-

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searchers do not have a very satisfying explanation for their unexpected results. Overall, the paper could be improved by considering why no variations were observed.

The introduction was well written, and provides a nice theoretical framework for the biophysical processes involved in $\delta^{13}\text{C}$ of soil respiration and production. This is a major strength of the paper. The experiment was purportedly developed to test three main predictions (not really hypotheses), but the basis for these predictions is not completely clear. For instance, why were diel variations expected? In what way would they change? Why would heterotrophic activity alter the $\delta^{13}\text{C}$ of soil efflux following rain, and not autotrophic activity? (indeed the authors have shown no effect of soil moisture on $\delta^{13}\text{C}$ in incubations as stated on p. 6377, L 23-25.) The importance of pressure pumping has also been well studied and it is not clear why it is set up as a hypothesis.

The discussion section includes a lot of background that might be better suited to the introduction in that it sets up the expectations/predictions (see below for more detailed comments). Although the research is rigorous, the authors could consider re-casting the objectives/hypotheses to better match the overall results of the manuscript, which is mainly focused on rigorously testing diffusion theory with a lot of data.

The manuscript could have higher impact if it was shortened up to focus on the data, and to directly address (and explain) the unexpected results of no temporal variations in $\delta^{13}\text{C}$ of respiration or production. There is a nice discussion of autotrophic vs. heterotrophic respiration, and potential biological processes that should induce temporal changes in $\delta^{13}\text{C}$, but no real discussion of why these processes did NOT affect the $\delta^{13}\text{C}$ in this study. Overall, the discussion should be shortened and focused to explain the results, and in so doing, should improve readability and impact.

Specific comments:

The methods section should explain that the soil gas wells collected a large volume of air (2.35 L of air, which would be equivalent to 4.7 L of soil volume if porosity was 50% and the soil was completely dry, or 9.4L if the air-filled porosity was 25% of total

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soil volume). This large volume equates to a radius of at least 10-15 cm, so samples collected at the O/A interface and 5-cm depth would be incorporating substantial air from above the soil. What is the influence of this large sampling volume on the model fits? Would the modelled profiles in Fig 7 be much different if the range of depths for each sample was increased (ie 10-cm depth sampled air from at least 0-20 cm)?

Restructuring the text in the Results section a little bit could improve the readability and streamline the manuscript to improve the overall impact of the paper.

The paragraph starting on P. 6374, L 3 could be revised to start with a stronger topic sentence that conveys the main message of the paragraph, such as “The theoretical expectation for fully diffusive mixing between the CO₂ produced by respiration and the forest air was supported by most of the observations from the chambers, but not all of the gas wells (Fig. 6).” Then go on to explain the Keeling-type regressions and describe the main points in the paragraph. However, P. 6374, L 13-28 could be removed because these details are clearly presented in the table for those interested.

Figure 6: Please check the caption, the regression equation from the chamber inlet data seems to have a typo (+/- -26?).

Similarly the following paragraph starting on L 24 could be restructured with a strong topic sentence of a main result, rather than just describing the figure caption. This will allow readers to more fully appreciate the importance of each figure.

P. 6375, L 6, please clarify that delta-J was calculated from gas well data, not “measured with gas wells”. Figure 7: The legends in part a and b are almost big enough to be able to read. In the caption, the horizontal dotted lines indicate the O/A interface and the top of the O horizon, because the O horizon (organic layer) is usually above the A horizon (mineral soil).

Discussion:

P. 6376, L5-28, this first paragraph reads more like an introduction to why diel variations

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in d13C would be expected, and could be moved to the introduction. It concludes that no such variations were observed, but we are left wondering why not. Any ideas?

P. 6377, L. 4-29, similarly these two paragraphs provide a nice literature review (better suited to the intro) but again leaves us without even an educated guess about why no change following rain was observed.

P. 6378, L4-16, this discussion of partitioning between autotrophic and heterotrophic components would be more appropriate if the paper presented direct observations of d13C in autotrophic and heterotrophic components. Based on the data, and considering the objectives of the paper did not address this topic, it is not clear why the recommendation on lines 14-16 is made. (likewise in the conclusions).

P. 6379, Lines 3-29, considering that the gas wells sampled such large volumes of soil air, it seems likely that the pressure pumping effects could be challenging to observe.

P. 6380, L1-11, this first paragraph is a major strength of the manuscript; it could be the best dataset ever produced to illustrate the utility of diffusion theory to describe d13C in soil respiration.

P. 6380, L.12-29, in the opinion of this reviewer, too much emphasis is placed on the artifacts induced by pressure pumping from the shallow depths. It needs to be discussed but this section (as well as the results) could be shortened a bit. The recommendations to minimize gas sampling volume and to avoid disturbing the diffusion gradient are well supported, and could be quantified with some simple calculations indicating the depth range (radius) of the volume sampled.

P. 6381, L. 5-8, a recent study by Ogle and Pendall (J. Geophys. Res. Biogeosci., 120, 221–236, doi:10.1002/2014JG002794) found the opposite (gas wells were more variable than chambers), possibly because the gas well volumes were much smaller. Please comment.

P. 6381, L. 9-18, this discussion addresses the main finding that was highlighted as the

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title of the overall manuscript, yet it is buried in a paragraph that starts out discussing the difference between chambers and gas wells. If this section was moved higher in the discussion section it could help address the question why no temporal variations were observed.

P. 6381, L18 to P.6382, L.7, this section could be condensed a bit. Start with a stronger topic sentence such as, “the isotopic composition of soil air in upper organic and mineral horizons is susceptible to advection, or pressure pumping, both due to natural weather dynamics and to methodological artifacts.” Then go on to (briefly) explain the details.

Conclusions should be revised after considering the other recommendations provided above.

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