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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 #3

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General Comments

This discussion paper, "Environmental forcing does not induce diel or synoptic variation in carbon isotope content of forest soil respiration", by D. R. Bowling et al. nicely describes a very welcome observational study of carbon dioxide isotope production and transport within the soil. The study is well designed and thorough, providing quite a rigorous test of several commonly held ideas about the soil system (e.g. the three hypotheses that the authors set out to test). This is one of those clarifying papers that puts together not-so-novel pieces (e.g. chamber flux measurements, pore gas sampling, diffusion modeling) to give a view of the whole puzzle that is novel and valuable. I think this paper will be of much interest to the soil science community, as it

was to me.

The paper itself is well written; with rare exceptions (see below), the ideas are clearly expressed, well supported by explanations and evidence, and logically organized. The figures are clear and effective as well. Personally, Figures 1, 6, and 7 were gratifying to see. How often are ecosystems so kind as to conform to simple mathematics?

I have had the luxury of reading the comments of Referees #1 and #2 before posting my own, and so I will add here that I am persuaded by two of Referee #2's general suggestions:

(1) Better addressing possible reasons for the surprising lack of variation in d13C with rain and time of day would indeed strengthen the paper. You seem to address the case of rain by arguing that there is no generalizable pattern in the literature, but then why did you seem to expect a d13C response to rain in the intro? For the lack of diel variation, you seem to blame heat conduction and diffusive transport (i.e. "strictly biological interpretations. . .are too simplistic"), but the logic is not clear: adding physical causes of diel variation on top of the biological ones seems unlikely to result in such a flat line as you observed. I don't expect you have the answers (and you don't need to for this paper), but some logical speculation or even just an explicit admission of mystery would help.

(2) The paper does have a bit of a split personality, with some aspects (e.g. title) being focused on the surprising lack of variation in the d13C of respiration (this result is about patterns in time) but most others being focused on the test of diffusion theory (this result is about patterns in space). Perhaps these two foci could be separated out better, e.g. with the results section first establishing the conformity to diffusion theory (excepting the sampling artifact) and then (perhaps in a second subsection) presenting the surprising time series results.

Specific Comments

Section 2.6: Unless you are restricting your determination of delta_F to nighttime data (and if that is the case then it should be noted in the manuscript), I believe you are assuming here that the isotopic signature of whole-forest respiration is identical to that of photosynthesis on the timescale of your study, i.e. that the isotopic disequilibrium is zero. The mixing line approach you cite, if including daytime data, should give the isotopic signature of the net ecosystem CO2 source (i.e. NEE, not respiration) integrated over some time period preceding the measurement (that time period depends on the mixing time between the source and the background but is not precisely known). The signature of NEE will only be equal to the signature of respiration if the photosynthetic and respiratory signatures are identical. If you are making that assumption, then I think you should state it explicitly (and perhaps consider what error would result if the photosynthetic and respiratory signatures were actually different by, say, 1 permil, which I think is plausible).

page 6375, lines 19-20 (and page 6381, second paragraph): What is the measurement uncertainty for an individual measurement of delta_J, and what is the measurement uncertainty for an individual measurement of delta_R? (These depend of course on the spectrometer uncertainty and on how delta_J and delta_R are calculated from the spectrometer data.) The differences in variability between delta_J and delta_R will only be meaningful if the variability is larger than the measurement uncertainty, but I don't believe you have shown that to be the case.

very end of section 3: I don't understand the logic of "Due to the large number of samples, we do not interpret these small statistical differences as particularly meaningful". Shouldn't a large number of samples increase your statistical power and therefore make small differences more meaningful? The only reason I can think of to discount a statistically significant difference (which you say you have found) is on account of some systematic error or uncertainty between the delta_R and delta_J methods. If you think such a systematic uncertainty exists, I think you should discuss and ideally try to estimate it.

Figure 3: Do you know why, after the first rain event causes it to step up, the respiration rate seems to step back down between the second and third rain events?

page 6379, lines 16 ff: This is a very nice analysis of the contrast with the snow pack experiment.

page 6380, lines 12 ff: Here the relatively comprehensive nature of this study shines. It is great that you were able to discriminate between these two possibilities.

Technical Corrections

page 6363, line 8: "thus" implies that this sentence is a conclusion drawn from the previous sentences, but it is not (though the sentence is true). The previous sentence said soil respiration is the biggest flux from the terrestrial biosphere; this sentence says that the biosphere is important to predicting climate. I think in the previous sentence, you could say that soil respiration is the biggest flux of carbon to the atmosphere period (i.e. including anthropogenic sources), in which case this sentence would follow as a conclusion.

page 6363, line 14: Similar to my previous comment, you write "as a result" but I don't see how the fact that soil respiration is linked to plant photosynthesis implies that the residence time of the carbon in the soil efflux must be short.

page 6364, line 27: "biophysical" should be "physical" (the biology is in the production, not the transport of CO2 within soils)

page 6365, line 3: need closing bracket after C_s

page 6365, line 16: "more" seems redundant

page 6369, lines 18-19: how are the 10cm diameter O/A interface wells inserted without digging?

page 6370, lines 5-6: Do you mean that an individual measurement takes 10s, and 60 such measurements are made during the 10 min measurement period?

page 6370, line 8: If gas flowed in each inlet for 10 min during measurement, then how could you measure 4 gas wells in 20 min?

page 6372, line 12: should probably read "...of production, concentration of CO2 in forest air, and d13C of CO2 in forest air..."

page 6372, line 14: should probably read "described in the Results section" or "described in Results"

page 6375, line 12: should read "however, that...", although I think this sentence is redundant with one in the next paragraph and so should be cut.

page 6376, line 5: all respiration is biological, no?

page 6382, line 11: should read "...of forest air – and compared..." (dash, not comma). Also, the list of methods lacks parallelism. How about "– soil surface chambers, soil pore gas wells, and forest air inlets –"?

Figure 6 caption: I don't understand "d13C = 6997/CO2 $\pm$ -26 ‰

Figure 7 caption: on the fourth last line, delta_R should not be inside the parentheses

Figure 7 caption: in the second last sentence, I would write "Lines show the results of the diffusion model (see text) fitted to either all measurement depths..."

Figure 7 caption: the last line should read "the top of the O horizon, respectively." (not A horizon)

Interactive comment on Biogeosciences Discuss., 12, 6361, 2015.