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Interactive comment on "Seasonal and vertical variations in soil CO_2 production in a beech forest: an isotopic flux-gradient approach" *by* Emilie Delogu et al.

Anonymous Referee #2

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This study examines spatiotemporal variance in the soil isotopic signature of production, and depth of production. The study is carefully constructed, and similar to a limited number of other studies published over the past few years. The measurement science is generally good, and I acknowledge that it is challenging to make these measurements. But whether the authors are trying to improve on comparator studies, or address a particular research gap, is unclear. As a result, the analysis doesn't seem to go anywhere and despite being very familiar with work of this nature I'm uncertain as to why I'm reading the study - and how it would make a difference to my work. The findings are also not presented as clearly as they could be. At this stage, and for the reasons described below, I feel that the manuscript falls short of Biogeosciences

quality, but I would encourage more development of this dataset and of the manuscript.

This manuscript lacks context and explanation. The abstract structure suggested to me right away that this manuscript would be a difficult read, because it consists almost entirely of results, with no motive, no apparent research gap, no conflict to be resolved, and no explanation of what was done. The manuscript unfolds in a somewhat better way, but the introduction is still short and lacks a compelling objective, and some solid reasons for using THIS site. What is the motive for the study other than better needing to understand soil respiration? Can the use of this particular site be defended? Are the results going to be universally transferable to other sites? Why or why not? None of these things are clear. I can draw my own conclusions, but presumably the authors have an even better knowledge than I. Many of the citations in the manuscript are also old and weak. For example, Cerling is cited for work in describing the behaviour or isotopes but his work only describes steady state profiles, and not non-steady state ones - for which the authors have limited conclusions (and one might therefore fear a limited understanding too). Also for production profile work, there are better modern references available than deJong and Schappert, including ones that use membrane techniques - which would provide better analogs to what the authors have done. A number of studies have also examined soil isotopic gradients / production profiles, yet the authors only cite about 3 despite the fact that studies of this type are the closest comparator and would be very valuable here as context. There are also a few methodological studies in the literature related to isotopic error in chambers and soil profiles which could inform the authors about error. The authors bring too few elements of the broader discipline into the introduction, and similarly flavour the discussion too little. The manuscript must be rebuilt for improved context.

This manuscript lacks any sort of error analysis or propagation of error, which is unacceptable in an isotopic study in which small differences are expected a priori by both the authors, and the reader. Many of the studies cited here even contain error analyses, so one must expect that such is a requirement of a good study. To illustrate the potential impact of error on reader confidence, there is a described offset between Ft and Fs of a few permil, and abundant discussion of the ecophysiological explanations. But, when an offset exists in two quantities that could potentially be the same numerically under some circumstances, one would turn first to systematic bias to can explain the observed results. After all, dynamic chambers are prone to systematic bias errors of several permil when used for isotopic sampling (Nickerson et al 2009, RCM). Likely there are also errors inherent in stem flux. Only when those errors could be proven not to explain the results could the quantities be said to differ. However, authors pretend that these errors don't exist, or don't matter. To illustrate again, Figure 2 shows some wild variation in isotopic values that fall outside of what one might expect. Are these errors the result of an over-sensitivity in the equations to one parameter - and a resultant random error? Through I appreciate the fact that all the data are shown, they must also be defended better so that I can trust these data. As a final illustration, I would offer that the explanations of wind pumping and other mechanisms is weak and quantitatively unsubstantiated. Given some assumptions about the amount of wind pumping, are the values abnormal by a similar amount? Some meat is needed on these bones, and would be expected in a careful methodological study such as this one. Overall, I have a hard time understanding what is measurement error, what relates to transport, and what relates to physiology. These uncertainties undermine my confidence in the study's conclusions. It will take time for the authors to rework their data and text to first address and rule out measurement error (systematic and random). I expect that some conclusions might change along the way.

The authors should flag when quantities are statistically different, and when they are not. Many of the differences between depths for example do not appear to be statistically significant, yet they are described as though they are.

Finally, this manuscript was hard to read. Language did not seem to present any barrier, but the authors did break many of the tenets of good writing. Some transgressions included: -Starting sentences frequently with abbreviations -Having abundant run on

sentences exceeding 40 or 50 words -Overuse of abbreviations when in most cases common language would suffice -Lack of consistency in terms like “max.” and “maximum” -Parentheses as part of each and every sentence through some sections -Use of text simply to connect numbers, when effective tables would have been better for communication of quantitative metrics I would recommend that the authors read Josh Schimel's book, and/or early chapters of Pinker's “Sense of Style”, then come back at it. This manuscript needs serious improvement because the standard of communication is too low for a leading journal.

Overall I think this study has promise because the methods were carefully constructed and carried out. But analytically it is not adequately nuanced for a journal of this calibre. My view is that a very major revision will be required. The revision might somewhat alter the conclusions of the study once the authors consider the effect of error, what is actually statistically significant, etc. I am sorry to deliver this less than positive news, but hopefully the comments above are constructive and helpful in redevelopment. Best of luck.

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