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Discussions

Interactive comment on "Net primary productivity, allocation pattern and carbon use efficiency in an apple orchard assessed by integrating eddy-covariance, biometric and continuous soil chamber measurements" *by* D. Zanotelli et al.

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Zanotelli et al. present an extensive suite of measurements regarding the C-cycle of an apple orchard. The authors measured Net Primary Production (NPP) via monthly biometric measurements, derived Gross Primary Production (GPP) from one year of eddy-covariance data, measured soil respiration (Rsoil) using a combination of continuous autochamber measurements and survey measurements, and computed the carbon-use-efficiency (CUE) of the ecosystem (NPP/GPP). This was quite a lot of work, and I commend the authors for their thoroughness in measuring the C-cycle

of this ecosystem. I have some comments and concerns regarding the measurements of some particular pools and fluxes (see detailed comments below), although I do not think that any of these concerns critically undermine the project as a whole.

One of my concerns is that this manuscript lacks a central hypothesis or question that would serve to motivate the work. That is, why did the authors go to such great lengths to collect all of these measurements? I did not find this to be set up in a compelling way in the introduction. One potential remedy is to expand upon a sentence in the introduction on page 14093, line 24-27: "Increasing our knowledge on the magnitudes and spatial distribution of CUE and heterotrophic respiration (Rh) could allow for a better linkage of the GPP estimates with those of net ecosystem productivity (NEP), for which reliable climatic and biological predictors are still unavailable at the global scale." The argument could proceed thus: (1) Satellite-derived estimates of GPP are increasingly robust, but (2) it is difficult to estimate NPP form these measurements, as Ra is difficult to quantify or model. (3) CUE may provide a method to derive NPP estimates from GPP, if robust CUE estimates can be obtained for many ecosystem types. (4) Can current methodologies be combined to robustly quantify the CUE of a simple model ecosystem?

Specific comments: Page 14094- lines 1-11, particularly line 10. These statements regarding the uncertainties of C biogeochemistry in woody agro-ecosystem would benefit from a quantitative description of the importance of this ecosystem type (i.e., woody agro-ecosystems make up X% of the global cultivated land area, or Y% of the land area of a particular region, or may contribute up to Z% of NPP in a particular region). Alternatively, the authors could choose to avoid discussing the importance of woody agroecosystems in the manner, and instead present their apple orchard as a simple model ecosystem.

Page 14097- line 25. Of the entire eddy-covariance time-series, what percentage was gap-filled? Generally, please note that I am not expert in the eddy-covariance technique, and I will assume that the measurement details were appropriate and correct.

Page 14098- lines 26-28. I agree that it is reasonable to neglect VOCs, non-CO2-C emissions, and root exudates from the NPP estimate, as these components tend to be very small and difficult to measure. However, I think it would be useful to cite some other studies where the authors have made the same decisions.

Page 14099- equation 1. I find this equation confusing for a number of reasons. The authors have just defined NPP to be the sum of six components (leaves, fruits, aboveground wood, belowground wood, fine roots, and understory); this contradicts equation 1, which calculates NPP as the aboveground biomass increment plus litterfall. Also, I do not think the notation of ΔNPP is appropriate, as this indicates would indicate the change in NPP. I would suggest removing equation 1, and simply stating that monthly NPP of each component was calculated as the total mass increment minus losses. Page 14101- line 25-27. Please clarify why this assumption was necessary, and indicate any support for this assumption. More generally for NPPfr- I am unable to determine how the described measurements were actually used to calculate NPPfr. An equation specifying how NPPfr was calculated would be helpful. The minirhizotron measurements were described as "periodic", which is not particularly informative. Were these measurements taken monthly as per the aboveground measures? Was the production of new fine roots separated from fine root mortality using the minirhizotrons, or did the authors just quantify the amount of roots present in the images? This is important, because fine root mortality and production often occur at the same time, and thus it is quite possible to have substantial NPPfr without much change in the pool size of fine roots. It is relatively common to use minirhizotrons to document the growth increment of new fine root length, and then to calculate the mass of roots produced using measurements of specific root length obtained from soil coring campaigns (Pritchard et al. 2008), but the authors have chosen a different approach. Can the authors cite other papers that have used a similar approach?

Page 14102- lines 8-9. There are two problematic issues related to using a trenching approach to separate Rsoil into Rhet and Rauto components. (1) Trenching often

creates a pulse of decomposing fine roots, which must be accounted for in the calculation of Rhet and Rauto. That is, trenching may artifactually increase the observed soil CO2 efflux in trenched plots. (2) In many ecosystems, soil heterotrophic activity is supported by new live-root-C inputs to soils, such that trenching can artifactually decrease the observed soil CO2 in trenched plots, as well as substantially change soil exo-enzyme activities and microbial community composition. There is substantial literature on this subject (as a start, see Hanson et al. 2000, Diaz-Pines et al. 2010, Comstedt et al. 2011, Drake et al. 2012). At the very least, I suggest that the authors acknowledge these issues and indicate if they have any justification for ignoring them. It would appear that these issues may apply in this ecosystem, as the measurements of soil CO2 efflux in the trenched plots did exceed the measurements in intact plots in some instances (Fig 2, Sept and Nov in particular). Later note- the authors address some of these issues in the discussion section on page 14110. It would be useful to mention these issues in the methods, where the trenching is described. I don't follow the authors' argument for why they did not address the pulse of root litter following trenching. Page 14110 line 22: "We avoided accounting for the "priming effect" due to an excess of decomposable matter (Kuzyakov et al., 2000) starting the measurements approximately 10 months after the trenching plots were set." The length of time one must wait for the pulse of root litter to decompose depends critically on the decomposition rate of these roots. Please note that these uncertainties regarding Rh affect the derived variable NPPflux.

Page 14103- line 9. I am surprised that the authors chose to relate Rsoil measurements to air temperature, rather than soil temperature.

Page 14103. The second and third methods of estimating Ra are not independent, as the authors suggest. Both methods rely on soil CO2 efflux measurements within the trenched plots, which the authors call Rh.

Page 14106- line 2. The word "allocation" has a special meaning in C cycle science, and non-standard use of this term has been the source of some confusion in the literature (see detailed discussion in Litton et al. 2007). Litton et al. stressed: “The commonly used phrase ‘biomass allocation’ refers to the distribution of biomass in different components (e.g. root : shoot). However, the use of the term ‘allocation’ for such descriptors should be avoided, as it is ambiguous and misleading” (page 2091). As the authors are reporting values for the relative distribution of tree biomass C, I suggest the authors use the terms “relative distribution of tree biomass C” and avoid the term “allocation” here.

Page 14106- around line 8. The usage of the term “decade” here was unfamiliar to me.

Page 14109- line 25. The estimates of CUE are not actually independent, as stated by the authors. In Table 7, the authors present two estimates of NPP (NPPbiom and NPPflux) and two estimate of GPP (GPPEC and GPPBS), and they calculate CUE based on all possible combinations. The first two combinations, for example, are NPPbiom/GPPEC and NPPflux/GPPEC; these terms are not independent, as they both rely on GPPEC. Furthermore and more importantly, the NPPbiom and the GPPBS are inherently autocorrelated, as NPPbiom is one of the two terms used in the calculations for GPPBS (see page 14104, line 18). When I look at Table 7, I conclude that the inherent relationships between these variables has constrained the calculated CUE estimates. This is a common theme in CUE research (e.g., see DeLucia et al. 2007, Litton et al. 2007), particularly given that GPP is often quantified as the sum of NPP and Ra, which creates an autocorrelation between NPP and GPP. I think the authors should highlight their best estimate of CUE, which I think is NPPbiom/GPPEC, as these two terms are truly independent and quantified quite well. In fact, I commend the authors for their hard work in deriving this estimate. I think the NPPflux and GPPBS estimates are less useful, as NPPflux is highly derived and subject to assumptions about the measurement of Rh using the trenching approach, and GPPBS is autocorrelated with NPPbiom and subject to the uncertainties regarding estimating Ra based on tissue N content. This comment also applies to Figure 8.

Page 14116. As discussed, the CUE estimate for this apple orchard of $\sim$0.7 is relatively

high compared to the literature on forests (DeLucia et al. 2007). I appreciate that the authors framed the discussion of this difference to focus on the high rate of fruit production in the apple orchard relative to forests. These fruits are full of sugars that the trees could have otherwise used to fuel the production of biomass components such as wood; this would have resulted in additional Ra to meet the construction costs of biomass production. This was nicely addressed by considering the construction costs of the different tissue types. I would consider highlighting this as part of the main conclusions at the end of the manuscript.

Figure 3. I believe this figure is meant to demonstrate that the Rsoil estimates derived from the continuous autochamber measurements have a higher mean flux rate when compared to the apple orchard as a whole, because the automated measurements were only taken within a tree row, which has higher rates of Rsoil relative to the space between tree rows. However, I find it difficult to easily derive this conclusion from Fig. 3, because it’s not clear how to compare the data in Fig. 3a to the survey data in Fig. 3b. That is, should the reader compare the continuous measurements to the survey measurements in plot A? A more informative legend or a description of the survey plot locations in the methods would be useful.

Table 2. Could you add total soil C and N components here? These values are quoted in the text on page 14105, line 21. References Comstedt, D., B. Bostrom and A. Ekblad. 2011. Autotrophic and heterotrophic soil respiration in a Norway spruce forest: estimating the root decomposition and soil moisture effects in a trenching experiment. Biogeochemistry 104:121-132. DeLucia, E. H., J. E. Drake, R. B. Thomas and M. Gonzalez-Meler. 2007. Forest carbon use efficiency: is respiration a constant fraction of gross primary production? Global Change Biology 13:1157-1167. Diaz-Pines, E., A. Schindlbacher, M. Pfeffer, R. Jandl, S. Zechmeister-Boltenstern and A. Rubio. 2010. Root trenching: a useful tool to estimate autotrophic soil respiration? A case study in an Austrian mountain forest. European Journal of Forest Research 129:101-109. Drake, J. E., A. C. Oishi, M. A. Giasson, R. Oren, K. H. Johnsen and A. C. Finzi.

2012. Trenching reduces soil heterotrophic activity in a loblolly pine (Pinus taeda) forest exposed to elevated atmospheric CO2 and N fertilization. Agricultural and Forest Meteorology 165:43-52. Hanson, P. J., N. T. Edwards, C. T. Garten and J. A. Andrews. 2000. Separating root and soil microbial contributions to soil respiration: A review of methods and observations. Biogeochemistry 48:115-146. Litton, C. M., J. W. Raich and M. G. Ryan. 2007. Carbon allocation in forest ecosystems. Global Change Biology 13:2089-2109. Pritchard, S. G., A. E. Strand, M. L. McCormack, M. A. Davis, A. C. Finzi, R. B. Jackson, R. Matamala, H. H. Rogers and R. Oren. 2008. Fine root dynamics in a loblolly pine forest are influenced by free-air-CO(2)-enrichment: a six-year-minirhizotron study. Global Change Biology 14:588- 602.

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