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Biogeosciences 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.

Anonymous Referee #1

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The manuscript presents a new data set about carbon allocation within an apple orchard and the total carbon balance of this ecosystem. The authors compare their results with typical values from deciduous forest ecosystems at similar latitude and they provide a detailed discussion about the various similarities and discrepancies between the carbon fluxes in these two ecosystem types and about possible control mechanisms. The paper is well written, the data seem to be of high quality and most of the data analysis was apparently carried out thoroughly.

Despite being only a case study with one year of data, two novel aspects make this

work interesting and valuable. Firstly, woody agro-ecosystems with their specific carbon allocation patterns are getting increasingly important in terms of land use change but are still underrepresented in carbon flux studies, and secondly, the authors provide a thorough and exemplary uncertainty analysis of the resulting carbon budgets on the basis of several completely independent measurement methods, which are only available at very few research sites. In my view, these new aspects justify the publication of the manuscript in BG, provided a couple of minor changes and clarifications will be made by the authors.

I suggest making the following changes.

P. 14092, L. 2: Please define CUE.

P. 14093, L. 20-27 and P. 14094, L. 21-22: It will make things easier for the reader if some equations are provided that clearly define the linkage between NPP, GPP, NEP, CUE, Rh and Ra.

P. 14095, L. 13: You might add that LAI was calculated from leaf litter collection (if I interpreted this correctly?) and explain whether it refers to the tree canopy only or whether the grass in the alleys was considered as well. The very low value reported here (only 40% of the average forest LAI according to Table 8) would be relevant for the interpretation of any differences in GPP between the orchard and a forest.

P. 14097, L. 8-9: Please add the tube length. This can be relevant when you don't apply low pass filtering corrections (line 17).

P. 14097, L. 10: Please replace "Nueberger" with "Neuberger".

P. 14098, L. 21-22: The cited "Law et al. 2008" document is apparently not accessible at the FAO website for "normal" readers. Are there any journal papers that could serve as a reference for this method? It is not clear to me whether all the methodological details given on the following two pages were actually developed by Law et al. or by the authors themselves, and it is important that the readers can check this!

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P. 14099, L. 6-7: What do you mean by “beside their relevance”? How do you know how relevant they are without measuring them?

P. 14102, L. 16: Why did you choose the linear regression to derive the CO2 flux? Is the saturation type function, which the LI-8100 software also provides, not more accurate to derive the initial slope?

P. 14104, L. 1-7: Please explain how exactly you calculated total nitrogen content. Did you include all aboveground biomass? Most of the wood is dead tissue that should hardly respire any carbon! Could this be an explanation for the fact that this method overestimates the respiration rate, compared to the other two methods?

P. 14107, L. 5: Replace “Sr” with “Rs”.

P. 14109, L. 1-2: Can these estimates somehow be confirmed by the observed root:shoot ratio of the standing biomass?

P. 14110, L. 8-9: I suggest being a bit more cautious with generalisations when comparing orchards and forests, because the stand age is very different, according to Table 8. An 11-year old, planted forest might be more similar to the investigated orchard. Check for example the paper by Luyssaert et al. (2008) in Nature 255, 213-215, for the influence of stand age on forest carbon budgets.

P. 14113, L. 18-23: Do these studies say anything about interannual variability in fruit production and C allocation to fruits? Or can you provide some rough estimate of such variability through the land owner of your study site? It would be good to know whether we can consider the 2010 budget as typical.

P. 14114, L. 18: I agree that this is the most important discrepancy, however it might therefore deserve a slightly more detailed discussion, see above (calculation of LI-8100 fluxes, estimation of aboveground respiration).

P. 14116, L. 21: It would be good to provide this information about the tree structure earlier, i.e. within section 2.1 (site description).

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P. 14117, L. 8: I suggest adding the phrase “as well as management activities such as irrigation, fertilisation and pruning”.

P. 14118, L. 2: Please add “11 year old” before “apple orchard”.

P. 14135, Table 8: You are showing large differences in net radiation between forest and orchard without mentioning them in the text, which is a little bit irritating. However, rather than discussing them, I suggest deleting those two lines from the table. Actually, I suspect that the numbers given by Luyssaert et al. might be wrong since they are named “radiation sums” in the original paper but are given in “W m-2” which is not consistent, because Watt is a rate (Joule per second) and not a sum. Anyway, better leave this out!

Interactive comment on Biogeosciences Discuss., 9, 14091, 2012.