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.

We wish to thank the reviewers for their helpful and constructive comments. We believe that they contributed significantly to improve the manuscript and we hope our replies will satisfy their expectations. Our detailed responses are reported after each comment (in Italic).

Response to Anonymous Referee #1

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.

We wish to thank the referee for the positive evaluation of the manuscript. We appreciated the overall analysis of the manuscript as well as the punctual comments which have been pointed out. They helped us to clarify and correct relevant aspects that we did not sufficiently considered in the original version of the text. We hope our answers will produce the level of detail required for a better and univocal understanding of the paper.

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

Following this indication, we defined CUE in the abstract as the ratio of net primary production (NPP) over gross primary production (GPP).

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, R_h and R_a.

We agree. Thus, according to this indication, we changed the text with the following sentences:

'Estimates of GPP are increasingly robust, both if derived from flux networks (Beer et al., 2010) or satellite observations (Peng et al., 2013), while reliable climatic and biological predictors of net ecosystem productivity (NEP) are still unavailable at the global scale. Given the relevance of CUE estimates and the paucity of existing reliable values for different ecosystem types and climates, we addressed as a main question in this study if current methodologies used to quantify C stocks and fluxes can be combined to robustly quantify the CUE in an apple orchard chosen as a simple model ecosystem.

Increasing our knowledge on the magnitude and spatial distribution of CUE and heterotrophic respiration (R_h) could allow for a better linkage of the GPP estimates with those of Net Primary Production (NPP = GPP - R_a) and those of NEP (NEP = NPP - R_h).

In fact, CUE is the ratio between NPP and GPP

$$CUE = \frac{NPP}{GPP} \qquad (1)$$

and can be related to all the different ecosystem C cycle components as following:

$$NPP = GPP - R_a = NEP + R_h \qquad (2)$$

$$GPP = NPP + R_a = NEP + \mathrm{R}_{eco} \qquad (3)$$

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.

The leaf area index (LAI) was calculated destructively on living tissues, as following: LAI= total number of leaves × mean leaf area. The number of leaves was assessed monthly by direct counting on selected branches (9 branches on 6 trees, 48 in total), and then upscaled based on the total leaf counting done in April; the mean leaf area was determined by measuring the leaf surface on other 9 branches, collected at three different heights above ground, and taken to laboratory and measured (by LI-3000 + LI-3050) in the same day. The obtained leaf surface of each tree was referred to surface unit (m^2) considering that each tree occupied a surface of 3 m^2. Our LAI estimate takes into account only the tree leaf area and not the contribution of the grassed alley. We changed the text accordingly.

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).

We agree. We added the information about the tube length (12 m) and changed text as following:
"The Eddysoft software uses the algorithm proposed by Eugster and Senn (1995) to correct for the underestimation of covariance signal due to damping of high frequency fluctuation of CO2 concentration. The high-frequency loss was estimated comparing the cospectra of the virtual heat flux, that we assume free of high frequency damping, with the cospectra of the turbulent CO2 flux".

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

Done.

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!

The guidelines described by Law and colleagues (2009, and the effective publication instead of the URL address is reported in the text) led us to set up a correct field plot design and helped us to be aware of all the relevant elements of the ecosystem that needed to be monitored biometrically. That protocol is optimized for forest sites and it is intended for annual measurements, so we adapted their prescription to meet the needs for our orchard, and the desired sampling frequency as follow:

- Woody biomass ($wood_{AG}$ and $wood_{BG}$) was estimated via allometric equation using the diameter measured at 10 cm above the grafting point instead of using the suggested DBH (diameter at breast height), that would have been too small, or the basal diameter, that in our case coincides with the anomalous trunk swelling due to the graft.
- Leaf biomass (and LAI) was not measured with optical instrument as suggested by Law et al. (2009), but we implemented a new protocol as described in a previous comment (P. 14095, L. 13).
- Fruit biomass, which is not considered in the protocol by Law et al. (2009) was estimated using the same procedure adopted for the leaves, thus counting once a month the presence of fruits on selected branches and then determining the mean fruit weight from the fruits present on the randomly collected branches.
- The total fine roots biomass was obtained by coring and spatial interpolation, while their production was obtained by image analysis from minirhizotrones.
- For the understory biomass we substantially followed Law et al. (2009) prescriptions.

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

The knowledge of their possible relevance is deduced from the literature. The following reference was added to the text:

"Cannell and Dewar, 1994".

P. 14102, L. 16: Why did you choose the linear regression to derive the CO_2 flux? Is the saturation type function, which the LI-8100 software also provides, not more accurate to derive the initial slope?

Our choice of using linear regression, instead of the exponential one, was done to facilitate the reproducibility of the experiment and was based on the fact that, in nearly all the papers published until now, the computation of soil respiration fluxes was performed similarly, i.e. maximizing the correlation coefficient of the linear interpolation of time evolution of CO_2 concentration inside the chamber. Some instrument softwares, like that of EGM4, PPsystem, Amesbury, MA (USA), provides only linearly interpolated flux values, so comparison would be difficult if a different interpolation algorithm would be used.

On the other hand, LiCor technicians provide good arguments in favour of exponential interpolation, which probably will be the standard in a near future. To have an idea of the possible systematic error arising from linear Vs exponential interpolation, we calculated also the fluxes with the latter method. Average difference was 4.0%, with the largest differences found in the highest flux range, therefore in the control plots, while the difference was found to be lower in the trenched plots. This analysis was performed on one week of half hourly data collected in 2010 (from 23/07 to 30/07, n = 320) and results are reported in Table 1.

We believe however that stressing in the text the possible systematic error induced by linear interpolation computation is out of scope in the current study.

Interactive comments Table 1. Results of R_s (control collar) and R_h (trenching collar) obtained using the exponential and linear fitting.

Operation		R_s		R_h	
		exp	***lin***	***exp***	***lin***
1	**Deleting observation with R^2 < 0.95:**				
	Number of observations below threshold	23	23	61	58
	Observations left	297	297	259	262
2	**Average flux of the period (µmol m^{-2} s^{-1}):**				
	Mean	4.06	3.85	1.76	1.71
	SD	1.93	1.75	0.66	0.65
3	**Average R^2 and standard deviation of the fluxes**				
	R^2	0.971	0.971	0.917	0.93
	SD	0.114	0.113	0.208	0.184
4	**Maximum and minimum value recorded (µmol m^{-2} s^{-1})**				
	Max	7.48	6.85	3.12	3.02
	Min	0.27	0.27	0.26	0.23
5	**Cumulated value emitted in the period (g C m^{-2})**				
		26.12	24.73	10.05	9.82
6	**Correlation with temperature (van t´Hoff equation)**				
	R_{10}	2.99 ± 0.21	2.82 ± 0.19	1.37 ± 0.08	1.30 ± 0.08
	Q_{10}	1.38 ± 0.09	1.38 ± 0.08	1.30 ± 0.07	1.34 ± 0.07

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?

We measured the dry biomass for all the different plant organs and we analysed C and N concentration separately in each of them ($wood_{AG}$, $wood_{BG}$, leaves, fruits, fine roots and understory). In March these components were limited to three organs (manuscript, Table 2). Results from the analyses performed in June and in August are reported below (Interactive comments Table 2).

The use of N biomass content (instead of C or dry biomass) to upscale the measured belowground R_a to the R_a of the whole plant allows to better consider the contribution of the different organs on the base of their activity, thus giving for example more weight to fine roots with respect to the coarse roots on the same unit of dry biomass. For these reasons, we do not believe that this may be the cause of the higher value of R_a given by this method with respect to methods 1 and 2 reported in Table 6 of the manuscript.

In the case of using the C biomass or dry weight (instead of N) to upscale R_{a_BG} to the whole tree, the scaling factor would have been 3.63 ± 0.31 or 3.60 ± 0.31, respectively, resulting in a total R_a of 657 ± 417 or 653 ± 414 g C m^{-2}.

Interactive comments Table 2. Standing biomass of the considered ecosystem components in three different periods of the season expressed as g m^{-2} of dry weight (DW), carbon (C) and nitrogen (N).

		Standing biomass (Sb, g m^{-2})						**k factor**
		AG				**BG**		$Sb_{tot}\,Sb_{BG}^{-1}$
		wood_AG	leaves	Fruits	understory	wood_BG	fine roots	
March	**DW**	1852.7 ± 177.3				419.0 ± 9.3	292.3 ± 32.3	3.60 ± 0.31
	C	840.6 ± 80.5				189.9 ± 4.2	129.7 ± 14.3	3.63 ± 0.31
	N	14.1 ± 1.3				3.3 ± 0.1	4.1 ± 0.4	2.92 ± 0.27
June	**DW**	2011.4 ± 343.4	189.5 ± 8.4	223.9 ± 41.4	33.4 ± 4.5	686.9 ± 28.3	488.8 ± 96.9	3.09 ± .41
	C	913.6 ± 156.0	86.2 ± 3.8	90.3 ± 16.7	13.7 ± 1.9			
	N	16.1 ± 2.8	4.7 ± .2	0.7 ± .1	0.9 ± 0.1			
August	**DW**	2160.7 ± 368.8	220.1 ± 9.7	763.9 ± 89.8	70.6 ± 5.5	696.3 ± 29.8	521.7 ± 126.4	3.64 ± 0.52
	C	979.2 ± 175.3	101.3 ± 4.5	302.7 ± 35.6	29.0 ± 2.2			
	N	15.5 ± 2.8	4.8 ± 0.2	2.1 ± 0.2	2.0 ± 0.2			

From the table above it is possible to see that the variability of the k factor along the season in the case of dry weight is relatively low, because the growth of above ground organs (leaves, fruits and understory) is balanced by the contemporary growth of fine roots.

P. 14107, L. 5: Replace "Sr" with "Rs".

Done.

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

We suppose that the reviewer refers to P. 14108, L. 1-2, where this comment appears to be more pertinent. If we look at the root:shoot ratio of NPP, we obtain a lower value (0.17 ± 0.03) than the root:shoot ratio of the standing biomass (0.38 ± 0.04). We believe that these differences can be largely explained by the large amount of fruit production and pruned wood which have a relatively low impact on 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.

We agree with this comment. We therefore added the following sentence to the text:
… smaller LAI, "since they are kept structurally similar to young forests but with a lower stem density (Luyssaert et al., 2008)".

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.

We provided information regarding interannual variability of fruit production in the Sect. 2.1 (site description).

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).

Mismatches between respiratory fluxes measured with different approaches are common (e.g. Lavigne et al., 1997). They are possibly due to selective systematic errors that easily occur at night by using the eddy covariance technique (Aubinet, 2008). Results obtained by Montagnani et al. (2009), Etzold et al. (2010) and by van Gorsel et al. (2009), suggest that the mismatch between fluxes measured with different approaches can be reduced at some sites if CO2 advection is taken into account or if only evening values of turbulent and storage fluxes are considered. It is not clear however to which extent these results can be generalized to all sites. An alternative approach to estimate the different CO2 fluxes above and within the ecosystem is the modelling one. Kutsch et al. (2008) obtained a realistic picture of ecosystem fluxes and stocks, by combining EC, chamber and biomass measurements.

Reasons for observed inconsistencies between different measurement systems include chamber measurement accuracy, the mismatch of flux footprint, the under-representativeness of the points chosen for the measurements, and the modelling itself (Janssens et al., 2003). In classical Rs measurements, carried out by survey chambers measuring in single days over the year, one source of uncertainty relates to flux modelling in the remaining days. However, this problem is virtually removed with the continuous measurements, since a very small percentage of data, or no data at all, are modelled.

A shortcoming also exists when automated chambers are used, since the opportunity for applying an optimal sampling strategy (Rodeghiero & Cescatti, 2008) is limited due to the small spatial coverage of a multiplexed soil chamber system. We don't have necessarily to expect numerical congruence between Rs data, obtained from one or few automated chambers working continuously on a limited area, and EC measurements. In fact, the CO2 efflux from the soil can vary largely in space and a small number of chambers can be representative of some specific conditions, such as gap vs. understory, but are hardly quantitatively representative of the broadly varying conditions existing at the ground level. A mismatch between the fetch of chambers and of EC sampling point can be also expected.

From the substantial mismatch between EC and chamber measurement also obtained in this study, besides the apparently optimal condition for the measurements, such as the presence of a simplified ecosystem, and the use of quite a large number of chambers allowing nearly continuous measurements and various replicates, we argue that a multiple approach is needed to obtain quantitatively reliable values of fluxes and their ratio, which possibly includes modelling and cross checking among measurements.

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).

We better described the tree structure feature in the site description section.

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

Done.

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

Done.

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!

Done.

Response to Anonymous Referee #2

This MS presents a full ecosystem carbon balance for a Mediterranean apple orchard, using multiple methodologies, including eddy-covariance, automated soil respiration chambers and detailed biometric measurements. While representing only one site for one year, this study is commendable for its use of multiple methodologies, thoroughness of those methodologies and attempts to quantify uncertainties related to these measurements and their scaling to the site level over the entire year. The main result highlighted, a CUE of approximately 70%, is surprising when put in the context of managed and unmanaged 'natural' forests, being very near the upper estimates made by physiological ecologists for these systems. As the authors themselves point out, this estimate is even above the theoretical range estimated by Amthor (2000), which leaves one wondering if this result is a representative value for this ecosystem or the product of some measurement bias. Given the lengths the authors go to answer this question, I recommend publication with the addition of a few minor clarifications considering the assumptions implicit in the approaches taken.

The authors highlight well the factors that make this agro-ecosystem different from other forests in ways that may lead to a higher CUE. Indeed, the detailed partitioning of NPP from biometric components is likely of great interest to those studying similar systems. Representative values of respiration values from the literature also provide good support for the contention that the large NPP investment in fruits may lead to lower respiration per unit biomass than other forests. Overall, the authors do a very good job finding support from the literature for why this ecosystem should have a higher CUE than most.

The authors provide estimates of uncertainty throughout the tables and results for most measurements. However, it is unclear what sources of uncertainty are included in each (other than the EC NEE). I would request some clarification of this in the methods and/or results.

We wish to thank the referee for his/her comments and for the helpful suggestions. Below we added the answers to specific questions.

For example, it seems that chamber based respiration measurements were scaled spatially in two ways. One is what I would term a 'horizontal' scaling factor (about 0.8) based on the June 2010 measurement campaign of spatial distribution of soil respiration (which suggested the automated *sampling area had higher respiration than average across the site). In order to construct an estimate of total autotrophic respiration independent of eddy-covariance measurements, the authors also use a 'vertical' scaling factor based on the distribution of nitrogen in the ecosystem in March 2010 (Eq. 6, Table 2). From my reading of the paper, it is unclear whether uncertainty in these correction factors are included in the estimates.*

After this comment, we computed the standard error of the "horizontal" scaling factor for the tree line (0.77 ± 0.06) based on the standard error of the mean of the soil respiration fluxes obtained during the survey campaign (n=210) and the standard error of the mean of the respiratory fluxes obtained contemporaneously by the multichambered system.

We then computed the standard error of the mean nitrogen content in root biomass of the tree line (0.872± 0.057 g N m-2) and that of the mean N content in root biomass of the grassed alley (0.214± 0.005 g N m-2). They were used to assess the uncertainty in the Ra_BG of the grassed alley following Eq. 8 and then the total Ra_BG following Eq. 7.

We calculated also the standard error of the "vertical" scaling factor (K = 2.92 ± 0.27) and we included it in the uncertainty calculation of total Ra. To compute this value we used Eq. 8.

During the review process we found a typing error in the Eq. 8 of the manuscript, which didn't affect previous computations. We corrected it in the text as following:

$$SEM_z = Z \times \sqrt{\left(\frac{SEM_x}{X}\right)^2 + \left(\frac{SEM_y}{Y}\right)^2}$$

Another point concerning these two scaling factors is that they are both based on measurements during one period of the growing season (June and March respectively), then used for scaling throughout the year. It is unclear if any attempt was made to estimate how they might vary seasonally. Some more detail about the methods or assumptions concerning this is crucial. Of particular concern is whether seasonal changes in N distribution would cause the vertical scaling factor (k in Eq. 6) to change throughout the year. As the effects of these scaling factors are multiplicative, they could generate considerable uncertainty in estimates of Ra (Table 6, number 3) and NPPbiom, as well as the CUE derived from them (Table 7).

We agree that considering the seasonal variability of the "horizontal" scaling factor is recommendable, however this operation requires the contemporary measurements of Rs by two analysers. In our case this was feasible only once in the season since the second instrument was borrowed us for that purpose. We tried to cover the largest possible temperature variability in the measurement period by performing measurements in summer during night and day. To make the reader aware about the relevance of these aspects, we added the following sentence in the text:

"The limited availability of a second analyser for Rs measurements, didn't allow us to repeat this parallel survey campaign in other periods of the season. We recommend this operation to be done, in order to take in proper account the seasonal differences in the patterns of local and spatial values of Rs".

As pointed out by the referee, the vertical scaling factor based on nitrogen ($k = 2.92 \pm 0.27$) was assessed only in March. As it is possible to see from Table 2 in this interactive comment, due to the lack of N and C concentration measurements in belowground organs in June and August, we calculated only the seasonal variability of the dry weight (DW) standing biomass, finding that the seasonal variability of k based on DW was relatively small, ranging from 3.09 to 3.64.

The absence of N concentration measurements in belowground organs in June and August didn't allow us to consider the effect of internal cycling of this element (Millard and Grelet, 2010). Similarly, we didn't assess the seasonal pattern of C content in the living biomass, although it's variation in much less pronounced (Akburak et al., 2012).

Based on the data on N concentration in fine roots reported by Ceccon et al. (2011) for apple trees and by Akburak et al. (2012) for different tree species, we can assume that roots N concentration tends to increase along the growing season, thus partially compensating the growth, and the related N allocation, to AG organs.

In any case, we agree with the reviewer: this point can be relevant for the extrapolation of belowground Ra to ecosystem Ra, so we explicitly added in the revised version of the text a recommendation for the need of repeated measurements of C and N in biomass organs along the season.

Finally, it is unclear how representative 2010 is of NPP at this site. As the major component of NPP, it would be interesting to know how 2010 is situated to other years in terms of fruit yield.

The 2010 was an highly productive year. We added in the text the values of the fruit production for the years 2009 and 2011 in the site description section.

The remaining comments are smaller details:

(P14095, L20) Is this a typical yearly application of fertilizer? What were the fertilization dates?

The reported value of fertilizer application is a standard rate for the organic farming protocol followed by the field owner. Similar quantities of fertilizer were applied also in the years 2009 and 2011. The organic fertilizer was applied in a single date the 18th of March. This indication was added to the text.

(P14097, L24) What % of total measurement period consisted of gaps for NEE?

Gaps represented the 7.2% of the yearly half hours prior to the QC/QA assessment. We added this information in the text.

(P14102, L7) I think ’multichambers’ should be ’multichambered’

We changed this term in the text according to this indication.

(P14102, L23) What % of total measurement period consisted of gaps for Rs and Rh (averaged across chambers)?

Soil respiration measurements with the automated chambers were done during the snow-free period, since the moving parts of the chambers are sensitive to ice formation. Until March measurements were performed at a single location to measure R_s. Overall, gaps represented the 46.2% for R_s and the 57.1% for R_h of the 17520 half hours.

(P14103, L13) It would seem that these first two methods both are derived partially from EC measured NEE. I therefore would remove the word ’independent.’ It may also be worth noting how Ra and GPP are both inferred from NEE, rather than being actually measured, perhaps by a reference to the section on EC above.

We removed the word “independent” as suggested.

(P14106, L10-17) In giving GPP, Reco and NEP for the different methods, keep the order consistent.

Done.

(P14116, L17) I think ’favorites’ should be ’favors’.

Done.

Response to Anonymous Referee #3

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 (R_{soil}) 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.

We wish to thank the Referee for the overall positive comments of the manuscript, for the deep analyses of the text and for the helpful suggestions. We went through the main points of concern highlighted hoping that the revision carried out on the text, and these answers, will accomplish his/her expectations.

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 (R_h) 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 R_a 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?

We appreciate this suggestion and we changed the text as reported in the answer to the observation "P. 14093, L. 20-27 and P. 14094, L. 21-22" of the Referee #1.

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.

> We agree with this comment and added the following reference to the text:
> "The crop class considered is globally relevant, being cultivated worldwide over 4.75 million hectares (FAOSTAT, 2010)".

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.

> The NEE missing data were the 7.2% of the total 17520 half hourly values prior to QC/QA assessment. This information was added to the text.

Page 14098- lines 26-28. I agree that it is reasonable to neglect VOCs, non-CO_2-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.

> To our knowledge, reliable data on the contribution of VOCs and non CO_2-C emissions to NPP in apple orchards are unavailable, while an estimate of root exudates of young apple trees growing in different soils, is reported by Scandellari et al. (2007). To give an idea about the role of this components on total NPP, in addition to Scandellari et al. (2007), we added to the text a reference to an estimate of root exudates proposed by Malhi et al. (2011) for tropical forests, and we gave references to two works on VOCs, from which it appears

that their contribution to NPP is negligible, even in tropical forests, where they are supposed to be produced at the highest rate (Arneth et al., 2011; Malhi et al., 2009).

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.

We agree with this observation. We realized that the the notation ΔNPP, intended to distinguish monthly values from annual NPP, was unclear. We changed Equation 1 into:

$$NPP = L_{t+1} + Sb_{t+1} - Sb_t \quad (1)$$

Where L is the litter collected from the nets and Sb is the standing biomass.

In addition, after this comment we noticed that an overall framework of the equations used to compute each NPP component was missing. For instance, for fine root production, it was not clear which equation was used. We therefore added in the text a new Eq. 2

$$NPP = Sb_{t+1} - Sb_t \quad (2)$$

where the litter is not considered, and we indicated for all the NPP components whether they were computed following Eq. 1 or Eq. 2.

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?*

Minirhizotron images were taken with the same frequency of the other biometric samplings, although not necessarily in the same days.

The image analysis process was done as following: all the roots appearing at the beginning of the season (March 2010) were tracked and related to the root biomass obtained from the soil coring campaign. In the following images, the new formed white roots were tracked and added to the previously determined ones. In this way the dead roots, that were disappeared or undistinguishable in the images from the suberized ones, were considered as being still part of standing biomass (Sb). The amount of yearly root litter production was estimated assuming a constant ratio between plant organs from one year to the next.

Since our main goal was to quantify NPP, and our image analysis software (WinRHIZO software, Regent Instruments, Canada) allows to track newly formed roots in addition to older ones which track remains in the image, we computed the root growth according to the new Eq. 2.

The only new assumption that was done in this study is about the constancy of the root to shoot ratio from one year to the next, and about the constancy of the ratio between large and fine roots, which was used to quantify the portion of roots feeding the detritus cycle on a yearly basis.

Page 14102- lines 8-9. There are two problematic issues related to using a trenching approach to separate R_{soil} into R_{het} and R_{auto} components. (1) Trenching often creates a pulse of decomposing fine roots, which must be accounted for in the calculation of R_{het} and R_{auto}. That is, trenching may artifactually increase the observed soil CO_2 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 CO_2 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 CO_2 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 NPP_{flux}.*

As mentioned, we discussed the possible problems due to the trenching technique in section 4.1. Root decomposition can be modeled according to exponential [$X=e^{-kt}$] or asymptotic models [$X=A+(1-A)e^{-kt}$] as reported by Hobbie et al. (2010), where X is the proportion of initial biomass remaining at time t. It means that a progressively reduced part of excised roots are still present in the trenched plot for some time, and this portion of decaying matter is necessarily part of the trenched plot. Based on available literature, where the effect of the increased fresh decomposable matter due to root excision on soil respiration was considered negligible after four (Fahey et al., 1988) or nine months (Bowden et al., 1993), we considered ten months an adequate period of time before the beginning of the measurements.

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

The use of air T vs. soil T is discussed in Mahecha et al. (2010 and 2011). The goodness of the relationships between soil CO_2 efflux and soil temperature decreases with the depth of the soil T measurement (Subke et al., 2003; Pavelka et al., 2010). Using soil T (at -10 cm) instead of air T in our dataset did not increase the coefficient of correlation with R_{soil}. It is therefore unlikely that significant improvements in term of modeling efficiency would have been obtained by using soil T instead of air T. Air temperature was preferred to soil T also because it was measured without gaps throughout the season, differently from soil T.

Page 14103. The second and third methods of estimating R_a are not independent, as the authors suggest. Both methods rely on soil CO_2 efflux measurements within the trenched plots, which the authors call R_h.

We agree with this comment and removed the term "independent" from the text.

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.

Done.

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

We changed decade into: "ten days".

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 (NPP_{biom} and NPP_{flux}) and two estimate of GPP (GPP_{EC} and GPP_{BS}), and they calculate CUE based on all possible combinations. The first two combinations, for example, are NPP_{biom}/GPP_{EC} and NPP_{flux}/GPP_{EC}; these terms are not independent, as they both rely on GPP_{EC}. Furthermore and more importantly, the NPP_{biom} and the GPP_{BS} are inherently autocorrelated, as NPP_{biom} is one of the two terms used in the calculations for GPP_{BS} (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 R_a, which creates an autocorrelation between NPP and GPP. I think the authors should highlight their best estimate of CUE, which I think is NPP_{biom}/GPP_{EC}, 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 NPP_{flux} and GPP_{BS} estimates are less useful, as NPP_{flux} is highly derived and subject to assumptions about the measurement of R_h using the trenching approach, and GPP_{BS} is autocorrelated with NPP_{biom} and subject to the uncertainties regarding estimating R_a based on tissue N content. This comment also applies to Figure 8.*

We agree with this comment stating the non-independency of the methods and, as already mentioned, we changed the text accordingly. In our view, however, the four estimates that we provide can better constrain advantages and disadvantages present in each estimate. It is necessary to consider that GPP_{EC} is computed according to a footprint which is not strictly limited to the plots where biometric sampling were carried out to compute NPP_{biom}. See also the notes to P. 14114, L. 18 of Referee #1. We believe that the multi-source estimate of CUE that we present is more robust that any single estimate.

Page 14116. As discussed, the CUE estimate for this apple orchard of ~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 R_a 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.

We added the consideration about the presence of simple sugars in the fruits in the conclusion section.

Figure 3. I believe this figure is meant to demonstrate that the R_{soil} 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.

We modified the legend of figure 3 in order to clarify the meaning of the comparison proposed by the figure.

Table 2. Could you add total soil C and N components here? These values are quoted in the text on page 14105, line 21.

We added this information in the table as requested.

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 CO_2 *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.*

Interactive comments cited references:

Akburak, S., Oral, H.V., Ozdemir, E. and Makineci, E.: Temporal variation of biomass, carbon and nitrogen of roots under different tree species. Scand. J. Forest Res., DOI:10.1080/02827581.2012.679680, 2012.

Arneth, A., Schurgers, G., Lathiere, J., Duhl, T., Beerling, D. J., Hewitt, C. N., Martin, M., and Guenther, A.: Global terrestrial isoprene emission models: sensitivity to variability in climate and vegetation. Atmos. Chem. Phys., 11: 8037–8052, 2011, doi:10.5194/acp-11-8037-2011.

Aubinet, M.: Eddy covariance CO_2 flux measurements in nocturnal conditions: an analysis of the problem. Ecol. Appl., 18(6): 1368-1378, 2008.

Beer, C., Reichstein, M., Tomelleri, E., Ciais, P., Jung, M., Carvalhais, N., Rödenbeck, C., Arain, M.A., Baldocchi, D., Bonan, G.B., Bondeau, A., Cescatti, A., Lasslop, G., Lindroth, A., Lomas, M., Luyssaert, S., Margolis, H., Oleson, K.W., Roupsard, O., Veenendaal, E., Viovy, N., Williams, C., Woodward, F., and Papale, D.: Terrestrial gross carbon dioxide uptake: Global distribution and covariation with climate. Science, 329: 834-838, 2010.

Bowden, R.D., Nadelhoffer, K.J., Boone, R.D., Melillo, J.M. and Garrison, J.B.: Contribution of aboveground litter, belowground litter, and root respiration to total soil respiration in a temperature mixed hardwood forest. Can. J. For. Res., 23: 1402-1407, 1993.

Cannell, M.G.R., Dewar, R.C.: Carbon Allocation in Trees: a Review of Concepts for Modelling. Adv. Ecol. Res., 25: 59-104, 1994.

Ceccon, C., Panzacchi, P., Scandellari, F., Prandi, L., Ventura, M., Russo, B., Millard, P. and Tagliavini, M.: Spatial and temporal effect of soil temperature and moisture and the relation to fine root density on root and soil respiration in a mature apple orchard. Plant Soil, 342: 195-206, 2011.

Etzold, S., Buchmann, N., and Eugster, W.: Contribution of advection to the carbon budget measured by eddy covariance at a steep mountain slope forest in Switzerland. Biogeosciences, 7: 2461-2475, 2010.

Eugster, W., Senn, W.: A cospectral correction model for measurement of turbulent NO_2 flux. Bound. Layer Meteorol., 74: 321-340, 1995.

Fahey, T.J., Huges, J.W., Pu, M. and Arthur, M.A.: Root decomposition and nutrient flux following whole-tree harvest of northern hardwood forest. Forest Sci., 34(3): 744-768, 1988.

FAOSTAT: Statistical Databases. Agriculture Data Collection (Primary Crops). FAO Food and Agriculture Organization of the United Nations, Rome, http://faostat.fao.org/site/567/DesktopDefault.aspx?PageID=567#ancor (Data referring to the year 2010, retrieved in March 2013).

Hobbie, E.S., Oleksyn, J., Eissennstat, D.M. and Reich, P.B.: Fine roots decomposition rates do not mirror those of leaf litter among temperate tree species. Oecologia, 162: 505-513, 2010.

Janssens, I.A., Dore, S., Epron, D., Lankreijer, H., Buchmann, N., Longdoz, B., Brossaud, J., and Montagnani, L.: Climatic influences on seasonal and spatial differences in soil CO_2 efflux. In: Canopy fluxes of energy, water and carbon dioxide of European forests / Valentini R. [edit.], Berlin, Ecological Studies, Springer. Pp. 235-256, 2003.

Kutsch, W.L., Kolle, O., Rebmann, C., Knohl, A., Ziegler, W., and Schulze E.D.: Advection and resulting CO_2 exchange uncertainty in a tall forest in central Germany. Ecol. Appl., 18(6): 1391-1405, 2008.

Lavigne, M.B., Ryan, M.G., Anderson, D.E., Baldocchi, D.D., Crill, P.M., Fitzjarrald, D.R., Goulden, M.L., Gower, S.T., Massheder, J.M., McCauhey, J.H., Rayment, M. and Striegl, R.G.: Comparing nocturnal eddy covariance measurements to estimates of ecosystem respiration made by scaling chamber measurements, J. Geophys. Res., 102: 28977 - 28986, 1997.

Law, B.E., Arkebauer, T., Campbell, J.L., Chen, J., Sun, O., Schwartz, M., van Ingen, C. and Verma, S.: Terrestrial Carbon Observations: Protocols for Vegetation Sampling and Data Submission. Report 55, Global Terrestrial Observing System. FAO, Rome. 87 pp., 2008.

Luyssaert, S., Schulze, E.D., Börner, A., Knohl, A., Hessenmöller, D., Law, B.E., Ciais, P. and Grace., J.: Old-growth forests as global carbon sinks. Nature, 455: 213-215, 2008.

Mahecha, M.D., Reichstein, M., Carvalhais, N., Lasslop, G., Lange, H., Seneviratne, S.I., Vargas, R., Ammann, Ch., Arain, A., Cescatti, A., Janssens, I.A., Migliavacca, M., Montagnani, L., and Richardson, A.D.: Global convergence in the temperature sensitivity of respiration at ecosystem level. Science, 329: 838 – 840, 2010. DOI: 10.1126/science.1189587.

Mahecha, M.D., Reichstein, M., Carvalhais, N., Lasslop, G., Lange, H., Seneviratne, S.I., Vargas, R., Ammann, Ch., Arain, A., Cescatti, A., Janssens, I.A., Migliavacca, M., Montagnani, L. and Richardson, A.D.: Response to Comment on "Global Convergence in the Temperature Sensitivity of Respiration at Ecosystem Level". Science, 331: 1265-1266, 2011. doi:10.1126/science.1197033.

Malhi, Y., Aragão, L.E.O.C., Metcalfe, D.B., Paiva, R., Quesada, C.A., Almeida, S., Anderson, L., Brando, P., Chambers, J.Q., da Costa, J.C.L., Hutyra, L.R., Oliveira, P., Patino, S., Pyle, E.H., Robertson, A.L., and Teixeira, L.M.: Comprehensive assessment of carbon productivity, allocation and storage in three Amazonian forests. Glob. Change Biol., 15: 1255–1274, 2009.

Malhi, Y., Doughty, C., and Galbraith, D.: The allocation of ecosystem net primary productivity in tropical forests. Phil. Trans. R. Soc. B., 366: 3225–3245, 2011. doi:10.1098/rstb.2011.0062

Millard, P. and Grelet, G.A.: Nitrogen storage and remobilization by trees: ecophysiological relevance in a changing world. Tree Physiol., 30: 1083-1095, 2010.

Montagnani, L., Manca, G., Canepa, E, Georgieva, E., Acosta, M., Feigenwinter, C., Janous, D., Kerschbaumer, G., Lindroth, A., Minach, L., Minerbi, S., Mölder, M., Pavelka, M., Seufert, G., Zeri, M. and Ziegler, W.: A new mass conservation approach to the study of CO_2 advection in an alpine forest. J. Geophys. Res.-Atmos., 114, D07306, doi:10.1029/2008JD010650.

Pavelka, M., Acosta, M., Marek, M.V., Kutsch, W. and Janous, D.: Dependence of the Q10 values on the depth of the soil temperature measuring point. Plant Soil, 292: 171-179, 2007.

Peng, Y., Gitelson, A.A. and Sakamoto, T.: Remote estimation of gross primary productivity in crops using MODIS 250 m data. Remote Sens. Environ., 128: 186–196, 2013.

Rodeghiero, M., and Cescatti A.: Spatial variability and optima sampling strategy of soil respiration. Forest Ecol. Manag., 225: 106-112, 2008.

Scandellari, F., Tonon, G., Thalheimer, M., Ceccon, C., Gioacchini, P., Aber, J.D. and Tagliavini, M.: Assessing nitrogen fluxes from roots to soil associated to rhizodeposition by apple (Malus domestica) trees. Trees, 21: 499-505, 2007.

Subke, J.A., Reichstein, M. and Tenhunen, J.D.: Explaining temporal variation in soil CO_2 efflux in a mature spruce forest in Southern Germany. Soil Biol. Biochem. 35: 1467–1483, 2003.

van Gorsel, E.N., Delpierre, R., Leuning, A., Black, J., Munger, W., Wofsy, S., Aubinet, M., Feigenwinter, Ch., Beringer, J., Bonal, D., Chen, B., Chen, J., Clement, R., Davis, K.J., Desai, A., Dragoni, D., Etzold, S., Grünwald, Th., Gu, L., Heinesch, B., Hutyra, L.R., Jans, W.W.P., Kutsch, W., Law, B.E., Leclerc, M.Y., Mammarella, I., Montagnani, L., Noormets, A., Rebmann, C., and Wharton, S.: Estimating nocturnal ecosystem respiration from the vertical turbulent flux and change in storage of CO_2. Agr. For. Meteorol. doi:10.1016/j.agrformet.2009.06.020, 2009.