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Biogeosciences Discussions

Interactive comment on "Carbon allocation to biomass production of leaves, fruits and woody organs at seasonal and annual scale in a deciduous- and evergreen temperate forest" *by* M. Campioli et al.

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We thank the Reviewer #1 for the detailed revision and the many constructive comments. We considered them carefully and we are confident the Reviewer #1 will be satisfied by the next version of the manuscript. On the other hand, we had to make a major change to the manuscript. Reviewer #2 had serious concerns on GPP estimations for the pine stand. Although we re-analyzed the data and evaluated possible amendments, we realized that due to the large spatial heterogeneity there was no standard way to produce reliable GPP estimates from the EC system for the pine stand. In-

stead of estimating GPP in different ways between the two sites (e.g. EC-derived GPP for beech and process-based modeled GPP for pine), which would have introduced inconsistency, we preferred to remove the pine study site. Reviewer #1 had also important concerns about the pine site, some of which were of difficult amendments without new data collection (e.g. tree sample size judged too low) and, overall, datasets at the beech stand are more extended and richer. In place of the inter-species comparison, in the new manuscript we will focus on the temporal variability of the NPP-GPP ratio in a more complete way i.e. not only seasonal variability but also (and in even greater details) interannual variability. We believe that the new insights on temporal variability at both interannual and seasonal scales are more relevant and robust of the previous inter-species comparison. The need for more details on interannual variability was also pointed by Reviewer #1 in a specific comment. The important comments of Reviewer #1 on pines will be very useful for preparation of another paper fully focused on pine after new field data collection.

Main points of concern:

(1) –R. Most seasonal data (LAI, circumference increment) are expressed as ratios, and no absolute values are given by the authors. I would like the authors to add a figure illustrating the seasonal evolution of LAI and DBH / CBH data in absolute numbers (not only a value of leaf and stem NPP as in figure 4). –A. The next version of the manuscript will comprise a figure presenting seasonal dynamics of LAI, circumference increment and wood density for the experimental year (1998) investigated for the seasonality of the NPP-GPP ratio at Hesse.

(2) –R. The methodology section is quite elliptic. I would like to have more precisions regarding the allometric equations employed to calculate biomasses and increment from CBH data. How was height increment considered? –A. More details on allometric relationships will be given. Allometric relationships were established for trees at the site. In such conditions, DBH scales well with stem height. Difference in stem biomass at two points in time derived from allometric relationships between DBH and

stem biomass inherently comprises the stem biomass increment due to stem height increase. Height increase was not considered separately in previous tree biomass study at Hesse based on allometric relationships.

(3) –R. Provide more details regarding the calculations of LAI and leaf biomass data. –A. Details will be provided.

(4) –R. I'm surprised by the seasonal dynamics of wood growth in Pine. Please address this question by making a figure reporting the measured CBH data (maybe on a relative scale to illustrate different trees), and compare your data to already published patterns (Schmitt et al., 2004; Makinen et al., 2008 and, more relevant for temperate pines, Zweifel et al., 2006) –A. Pines will not be included in the next version of the manuscript (see above), thus this remark will not be applied.

(5) –R. The authors completely overlook the question of data uncertainty and argue that errors on NPP are too many to be computed. I disagree with this argument. One can at least assess the error introduced by the use of allometric equations (given known uncertainties on the parameter values). –A. Reviewer #1 is right. We have realized that the lack of uncertainty analysis was an important flaw in the previous version of the manuscript. This will be amended in the new version presenting uncertainty for annual NPP and GPP estimations, evaluation of the significance level of difference of the NPP-GPP ratio among years and preliminary uncertainty assessment on seasonal data (both NPP and NPP-GPP ratio).

–R. One additional source of uncertainty is, at least for Pines, interannual variations in wood density, which is usually very important and not that easy to link with variations in late/early wood proportions (as suggested by the authors). I therefore expect the authors to precise this point. –A. Pine will not be anymore investigated in the next version. Our (commonly used) method to determine wood organ NPP based on allometric relationship overlooks interannual difference in wood density and C content. However, at Hesse, interannual variation in wood density was found to be limited (max 6-7% vari-

ation from the mean; Bouriaud et al. 2004 Trees) whereas interannual variation in C content are typical within 1-2% (Lamlom and Savidge 2006 Tree Physiology).

(6) –R. Do not overstate the few results originating from published papers (Granier et al., 2000 and Janssens, 2002) to draw strong conclusions about fine roots. This compartment is indeed extremely difficult to monitor and is still an important source of uncertainty in C budgets. (7) One conclusion of the paper is wrong and should therefore be corrected. The authors state that the C sequestration capacity of the Beech stand is higher than that of the Pine stand on the basis of an incomplete C budget, based on leaf and woody compartment, but completely ignoring fine roots and soil organic matter. –A. The text sections on fine roots and C budget will be removed.

Specific comments

(8) –R. Title: correct “at seasonal and annual scales” –A. Amendment applied (the title will be slightly changed emphasizing the new focus on the overall temporal variability of the NPP-GPP ratio).

(9) –R. L2 P7576: “because tree organs have different construction and maintenance costs, life span...” –A. Amendment applied.

(10) –R. L1 p7577: no estimate of autotrophic respiration is (or can be, considered the data) given in the paper –A. This section will be removed.

(11) –R. L22 p7578: change “Thornely” to “Thornley” –A. Amendment applied.

(12) –R. P7580: the authors do not document the influence of storm Lothar (December 26th, 2009) on stands' structures. It damaged a number of forest stands, notably in Northern France and Belgium. Should moreover precise if height and DBH should be understood as “mean” or “dominant”. –A. We will mention the impact (low) of storm Lothar in 1999 and precise that “mean” height and DBH are considered.

(13) –R. P7581: indicate that eddy covariance device was changed following the storm Lothar in Hesse (cf Granier et al., 2009 AFS). –A. We will do so.

(14) –R. P7581Change “Euroflux” to “CarboEurope”. L26 p7581: Change “biased” to “biases”.P7581: the cited reference (Nagy et al.) does not include a comparison of GPP calculated with / without footprint corrections. Please state the numbers and briefly remind the reader of the methodology used to compute both estimates. –A. The section to which Reviewer #1 refers to will be changed and improved. Nagy et al. paper is not anymore mentioned as irrelevant in the new version.

(15) –R. P7582: the 2-step procedure for annual biomass calculations in Pine should be precised. Cite the allometric equations used. It is formally not possible to compute interannual variations in woody growth (i.e. including stems, branches and coarse roots) from weighted averages based on CBH dynamics (you need to do so 2 more hypotheses not cited in the text: (a) that the ratios of branch and coarse root biomass to total woody biomass are equal from year to year and (b) that wood density does not vary from year to year). –A. Pine (and the mentioned 2-step procedure for annual biomass calculations) will not anymore be considered in the paper. Allometric relationships used for beech will be reported. In the paper, it will be mentioned that our method to determine wood organ NPP based on allometric relationships overlooks interannual difference in wood density and C content. Support from the literature for the irrelevance of such assumption will be reported.

(16) –R. P7582: Eleven trees is a low number for CBH sampling given the inter-tree variability (even within a given social class)... –A. This point can be improved only with new field collection and dendrometric analysis. However, because of the lack of reliable GPP estimates for the pine stand, pines will be excluded from the final analysis and the remark does not apply anymore.

(17) –R. P7583: cite Granier et al. 2009 illustrating CBH dynamics for some years at Hesse –A. Citation will be added and, even more important, CBH dynamic for one year (1998) will be reported as illustration.

(18) –R. P7583: I'm concerned with the data used by the authors. Three trees are

not sufficient to correctly sample the seasonal dynamics of CBH (consider for instance the strong inter-tree variability of extractable water linked to soil properties spatial variability). More I would like to have details regarding the computation of 3-week interval growth increment from automatic dendrometer which typically provide 1 measurement per half-hour/day. Make these data apparent in a new figure. –A. As mentioned above in answer to remark (16) on the limited number of sample trees used for annual NPP estimation, number of replicates for seasonal dynamics of pine circumferences can be improved only with new measurements. However, as explained above, pines will be excluded from the final analysis and the remark does not apply anymore. Detailed seasonal pattern of circumference increment and wood density will be reported for beech.

(19) –R. P7584: I acknowledge the effort of the authors to consider wood density variability. It is still extremely difficult to infer from year to year, particularly for Pine. I have the impression that the early/latewood transition was fixed from year to year in this study (and ring density calculated thereafter). Considered the very strong wood density difference between early and late wood, I would expect the transition date to be determined more precisely. It is, at least in the ring-porous Pine data, easily feasible on the basis of stem core data. –A. As mentioned above, pines will be excluded from the final analysis and the remark does not apply anymore. Seasonal variation in wood density for beech will be considered in details. Interannual variation in wood density for beech is expected to be low at the site; this will be mentioned in the paper with support from the literature.

(20) –R. P7585: please provide details regarding the calculation of seasonal LAI from hemispherical photographs (+ frequency of acquisition). The authors carefully detail the use of CH reserves for the formation of Beech leaves in spring. What about Pines? The authors seem not to even consider the possibility of needle construction depending (at least partly) on CH reserves (the construction cost of needles stay important and it is not sure that photosynthates provided by previous year needles are enough to make

the new cohort). –A. At Hesse, LAI was derived from canopy absorption of global radiation by using the Beer-Lambert law and an extinction coefficient of 0.4 following Granier et al. (2000 Functional Ecology). This will be reported in the paper as well as an example of seasonal LAI dynamics for 1998. The remark of Reviewer #1 on pine reserve is correct but it does not apply anymore as pines will be excluded from the final analysis.

(21) –R. P7586: it is not clear at all what the different NPPi components refer to. Please state it clearly. I also remind the authors that stand total NPP can not be computed unless one can assess the NPP of CH reserves and fine roots compartments. So NPPT should definitely be renamed. –A. This will be clarified.

(22) –R. P7586: see the "general comments" regarding error assessment. –A. Lack of uncertainty analysis will be amended in the new version presenting uncertainty for annual NPP and GPP estimations, significance level of difference of NPP-GPP ratio among years and preliminary uncertainty assessment for seasonal data (both NPP and NPP-GPP ratio).

(23) –R. L24 p7586: replace "annul" by "annual" –A. Amendment applied.

(24) –R. P7586: cite absolute ranges of variations, they are much more easy to interpret than CV. –A. We will do so.

(25) –R. P7586: I'm surprised by the high NPP values attributed to coarse roots for Beech. Considered that NPPstem is 55% of NPPw, NPPbranches is 15% of NPPw and that NPPcoarse roots is 30% of NPPw, one can compute a rootshoot ratio of investment of NPPcr/NPP(st+br)=30/70=42% which is much higher than the biomass rootshoot of 20% showed by Genet et al. (2009, Tree physiol) for Beech. Two possible reasons for that : (a) the coarse root biomass estimates are wrong (from allometries) or (b) the life span of coarse roots and wood are very different (i have no reference for that, sorry). The authors should detail and conclude on this point. –A. There was a mistake in the allometric calculations, we are sorry for that. The new mean wood NPP

proportion are 68% for stem, 14% of branches and 19% for coarse roots. This gives a biomass root-shoot ratio of 23%, in good agreement with Genet et al. (2009).

(26) –R. P7587: calculated Pine productivities are low but realistic when compared to yield tables (see e.g. Class 3 of productivity for Pinus sylvestris in Decourt, 1965, Annals of forest science. Le Pin Sylvestre et le Pin Laricio de Corse en Sologne: tables de production provisoires et méthodes utilisées pour les construire. Readily downloadable from scholar.google.com). Please mention this (or make reference to an other yield table) –A. This remark does not apply anymore as pines will be excluded from the final analysis.

(27) –R. P7587: present results of the stats analysis in a Table. Regarding correlation with climate variables, why not using a multiple regression model (df problem ?). –A. We substantially extended this part, following these suggestion. In the new version of the manuscript we will use 8 replicates years for the interannual analysis. This allows producing regression models with relatively high degrees of freedom (in particular for 1 predictor-models). Results will be reported in a Table and they will be discussed in detail within the interannual analysis on NPP-GPP ratio.

(28) –R. P7588: for GPP seasonality the beginning and end of season follow increasing / decreasing temporal trend can not be said to be "exponential" –A. We will amend this section.

(29) –R. P7588: i'm quite surprised by the seasonal dynamics of wood growth in Pine (fig. 4). I would expect most of the growth to occur in May-June, as commonly observed in Scots pine, either in boreal (Schmitt et al., 2004; Makinen et al., 2008) or temperate (Zweifel et al., 2006 + my personnal data) environment. I'm wandering about possible biases in auto-dendrometer data (see Makinen et al., 2008 for an extensive quantification of the biases). Please show your CBH data and discuss the possibility of a bias affecting the seasonal dynamics. –A. Pines will be excluded from the final analysis, thus this remark will not be applied. Our preliminary data from dendrometers (without

however checking for biases following Makinen et al., 2008) showed larger circumference increment from end April and early June and about 10-30% growth reduction in the period from mid June to mid August - early September, when most of the growth ceases. The pattern reported in the paper with largest stem growth rates in mid - late August was due to the correction for earlywood/latewood difference in density. As this section will not be included in the final analysis, we have not studied the case in details. However, we noted that the references cited by Reviewer #1 are from boreal or alpine environments. Could the shorter season in such environments result in most pronounced growth in early season?

(30) –R. P7589: you compare the NPPt/GPP ratio with the one published by De Lucia et al. (2007). Please rename your NPPt estimate (i remind that it does not include fine roots). Do De Lucia et al. included fine roots NPP in their estimation (in a word aren't you comparing apples and pears?). –A. DeLucia et al. are not clear about fine roots in their study. In M&M of their study they mention that they considered both above- and belowground growth but also that "the major components of forest NPP are wood increment and litterfall" and only the way they were measured was reported. We think that fine roots were not systematically considered but considered only for forest sites with published estimates of fine root NPP. However, because of this unclear point, we have not compared the DeLucia et al. estimates to our estimates of NPP–GPP based on leaves+wood NPP.

(31) –R. L10-15 p7590: in the case of a diversion of photosynthates towards cone production, one should also observe a negative correlation with NPPw and NPPf. This would be much more informative than the comparison of CV proposed by the authors. –A. Pines will be excluded from the final analysis, thus this remark will not be applied.

(32) –R. L22-24 p7590: how were Rauto and its uncertainty estimated in both cited studies (Nagy, Granier)? –A. Granier et al. (2008 AFS) estimated the Ra at Hesse (1997-2004) to be on average 7.56 Mg C ha-1 y-1 (SE=0.31), or 52% of annual GPP. However, in that study Ra was determined with a rather coarse approach (fixed pro-

portion of the total ecosystem respiration derived from EC) and thus the uncertainty associated to these estimates is large. This information will be reported and briefly discussed in the new version of the manuscript.

(33) –R. L24-26 p7590: considered that, for Pine, NPPt (stems + branches + fruits + coarse roots) is 17% of GPP and Ra 45% of GPP one expect an investment in fine roots+exsudates of [1-(0.17+0.45)] *GPP= 0.38*GPP. Please mention this, and give NUMBERS more than ratios of GPP which are less easily interpretable from an ecophysiological viewpoint. –A. Pines were excluded in the final analysis, thus this remark was not anymore applicable.

(34) –R. L9 p7591: refer to Jarosz et al. (AFM) for GPP numbers of understory in Pine stands (Pinus pinaster...). –A. Pines will be excluded from the final analysis (see above).

(35) –R. P7591: I'm sorry but the data presented do not allow the author to conclude that the C sequestration in Pine stand is lower than in Beech. We have no idea of the investment to fine roots and exsudates (even if the author give rough estimates from Granier et al. and Janssens et al.) and know nothing about C sequestration in soil organic matter. Hence you should moderate the statement. –A. We will remove this statement.

(36) –R. L3 p7592: Mis-citation of Hobbie (2006). The Hobbie paper refers to juvenile trees (not "plants" in general as stated by the authors). In Pinus sylvestris, allocation towards ectomycorrhiza is estimated to be at most 14-15% of NPP. No information for Beech. –A. GPP not allocated to leaf NPP, wood NPP and Ra was large for pine (see previous analysis) but limited in beech, about 10%. Considering that about 5% of GPP was estimated to be allocated to fine root NPP at Hesse, the role of C sinks as ectomycorrhizae and exudation is very minor at Hesse. Therefore, we will reduce substantially the section on the possible effect of belowground C transfers to symbionts and exudation in the next version of the manuscript. For pine, the hypothesized transfer

to unaccounted belowground sinks might have been overestimated by biased GPP estimates.

(37) –R. L8-9: This sentence is wrong. Remember that leaves grow early in the season, when most annual GPP has NOT been fixed... and i also remind the author that they cite earlier the work of Deckmyn et al. showing for Beech that current leaves are at least partly built on CH reserves accumulated on past year(s). –A. We will delete the sentence pointed by Reviewer #1.

(38) –R. L26-29 p7592: Remember that Granier et al. (2009 AFS) find the contrary when including year 2004 (following year 2003). For that year, following a particularly strong drought + heat stress, the spring resumption of cambial growth was slowed, probably due to a low level of CH reserves. Please cite this counter-example in case of strong drought –A. Inclusion of year 2003 and 2004 in the analysis changed the results, with no correlation between woodNPP-GPP and drought but woodNPP-GPP and previous-year temperature. On the other hand, as expected, the negative impact of drought on wood NPP at Hesse is clearly showed in the analysis.

(39) –R. L26 p7593: replace “at whole” by “at all” –A. The section to which the remark refers to will be removed.

(40) –R. L2 p7594: replace “aboveground and woody organs” by “wood, leaves and fruits” –A. The section to which the remark refer to will be removed.

(41) –R. L16 p7594: formation of new leaves followed by vessel production is only observed in diffuse-porous species (e.g. beech). In ring-porous species (e.g. sessile oak), vessel formation precedes leaves formation (see Breda et al., 1996). –A. This remark will be incorporated in the new version of the manuscript.

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