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Biogeosciences Discussions

Interactive comment on "Fine root dynamics for forests on contrasting soils in the colombian Amazon" *by* E. M. Jiménez et al.

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Answer to comments on Interactive comment on "Fine root dynamics for forests on contrasting soils in the Colombian Amazon" by E. M. Jiménez et al. by D. Metcalfe (Referee)

General comments

The structure of the paper is generally fine, though I think that the introduction should be revised to make it more concise and I found it hard to follow exactly what was done from the methods section.

We are preparing a new manuscript with the introduction and methods thoroughly reviewed.

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Interactive Discussion

Discussion Paper

I think there are two key scientific issues (see specific comments, below) that need to be acknowledged and/or addressed before the paper could be published:

1) There is a strange lack of consistency in ingrowth core sampling strategy between plots (different core sizes, retrieved after different times, different sample sizes). This, I think, makes it difficult to know what to make of plot differences in ingrowth core production estimates. Why was there such variation in sampling strategy, and is there any way of checking to see if it had an important effect on your estimates? The general agreement between your two different methods does, at least, make the plot difference much more convincing.

Differences in sampling strategy between sites responded mainly to logistic/funding limitations of the research in each site. Each experiment started as an independent piece of research, aimed to estimate productivity in each forest type. The experiments were afterwards integrated into the current analysis when the importance of the comparison was acknowledged. Even though sampling strategy was not identical, sampling differences are not substantial: sample sizes were almost identical in both sites (22 vs. 26 in the establishment 2, and 13 vs. 13 in the establishment 3 in the forest on clay soil and white sands, respectively). Though differences in retrieval times in the establishment 2 could be considered substantial (0.52 and 077 years, respectively), samples were retrieved at almost identical times in the establishment 3 (0.82 and 0.81 years, respectively). Because of the potential artifacts introduced by differences in retrieval times, we also calculated the relative growth rate (RGR) to compare FRP in standard units between forests and time intervals; results of RGR were consistent with the FRP results. A major concern rises from differences in core size; this point will be discussed below. However, as also pointed out, the two methods show the same results despite this limitation: FRP is higher in white sands than in clay soil, and differences were consistent along all the monitoring time.

2) The analysis of the exceptional 2005 drought seems flawed. If I understand correctly, to test for an effect the authors compared the 2005 dry season with the wet seasons

immediately before and after. Obviously, there will probably be a dry-wet season difference even in normal years. Surely the correct approach would be to compare the 2005 dry season with the corresponding dry season periods in the years before and after. Judging by Figure 3 you seem to have this data, my guess is that there will still be a difference but it will not be so big.

Growth rates of fine roots estimated from the approach used here usually show higher values at the beginning, in the time intervals closer to experimental set up (Figure 3 shows that in all cases the steepest slopes of curves of accumulated root mass occurred at the beginning of the time interval). Therefore, for the estimation of relative growth rates (RGR) we tried to keep constant the time elapsed since the establishment of cores, with the idea of comparing similar periods of growth. In order to present our results in standard units, familiar to readers, we converted all results to annual rates. The general idea was to compare RGR of fine roots during the 2005 dry season with that from the previous (2004) and posterior period (2006). A comparison with RGR of dry periods of those years was not possible because we do not have such data for all years evaluated. For example, in 2004 ingrowth cores were established in February and retrieval started in September, when the dry period had ended (dry season went from June to September).On the other hand, even though we have the data to compare RGR of dry seasons of 2005 and 2006, such a comparison would not bet appropriate because the time interval elapsed since the establishment until the 2006 dry period was longer than the interval used for estimating the RGR in the other years, and consequently RGR estimated for 2006 would be lower as a result of the artifact raised by the longer time period elapsed.

Even though we recognize that comparing RGR of different seasons could result in exacerbating the effect of drought of 2005, this analysis was useful to show the differential effect of dry season in each forest type and to detect the lagged response of white sand forest to drought. Results obtained through this analysis of RGR from in-growth cores concord with those from sequential cores (Figure 5), an entirely independent method.

Specific comments

Page 3420, Line 22) How is this a bias? If there are indeed higher root concentrations near to big trees, and you want to record natural (as possible) patterns of root standing crop and growth in these forests then why not go ahead and record near to big trees? I guess the real reason is that massive structural roots tend to get in the way nearer to big trees, right? If so, acknowledge this, it is not a big problem- (1) you're primarily interested in fine roots, (2) comparative differences rather than absolute values of mass/growth are still interesting, (3) most other methods (sequential cores, rhizotrons) face the same problem.

Yes, the main reason was to avoid the coarse roots. This will be modified in the paper.

Page 3420, lines 5-9) Generally there are two ways of doing ingrowth cores: (1) all the cores that are installed are retrieved each time, then they're all put back in again, or (2) only a portion of the initially installed cores are retrieved each time, so that the total amount of time that the cores have been in the ground increases with each successive sampling. Which (if any) of these approaches did you take?

Our experiment of in-growth cores followed the second option. We will clarify this point in the manuscript.

Page 3420, lines 24) generally through the next 4 paragraphs it is striking how little consistency there is between the sampling strategies on the different plots. Why is this? In some cases this is not important, but in others it could be a big problem. So, for example, why were different augers used in the different plots?

We will explain this part in the methods. As stated before, differences in sampling strategy between sites responded mainly to logistic/funding limitations and the independent origin of the research in each site. Each experiment started as an independent piece of research, aimed to estimate productivity in each forest type. The experiments were afterwards integrated into the current analysis when the importance of the comparison

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was acknowledged.

If a 15 cm long auger was used in the white-sand plot how is it possible to calculate standing mass and production down to 20 cm depth?

It was possible by controlling the soil depth to be sampled with the 15 cm long auger. We sampled two soil cores: the first one at 0-10 cm and then from 10-20 cm, separately.

These augers were used to make the holes for the ingrowth cores, right? If so, it is worrying that they are such different diameters since this will influence production estimates. Also, looking at table 2 it looks like the cores were retrieved after different periods of time on the different plots (for establishment 2). - If an equal length of roots grows into different sized cores over the same period of time, estimated production per unit ground area will be very different such that larger cores will tend to underestimate real root growth. See cores 1 and 2 in the figure at the end of the comments, both have the same amount of roots but expressed on a per unit ground area basis, estimates will be much lower on core 2. The situation is reversed after this initial colonization phase, when roots start to grow out the other side of the core, so that growth will be underestimated more often in small cores (where it will take less time for roots to completely grow through the core). See cores 3 and 4 in the figure at the end of the comments, both have the same amount of growth but only a portion of the total growth is captured by the smaller core. Am I making sense? - Thus both retrieval time and core size will affect growth estimates. Estimates of absolute root growth from ingrowth cores will always partially depend on these banal methodological details, but at least if the methods are kept identical they have some value for looking at relative differences. If, however, the methods were different between plots even relative differences may be difficult to interpret.

It has been recognized that methods used to estimate fine root production (FRP) have several limitations. We considered some of those aspects and for this reason we used two of the most common and inexpensive direct methods to estimate FRP: ingrowth

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and sequential cores. This approach allowed us to cross-check our results. Sequential cores are samples of fine root mass under the natural conditions of the site and their results are expected to be little affected by the diameters of cores used for sampling. Even though we found differences in the absolute values of FRP by each method, temporal trends and patterns of variation between forest types showed close agreement and consistency with the two methods used. This result gives us confidence in our results of ingrowth cores, despite the differences of diameters.

We do not know of any paper evaluating the effect of core diameter on estimates of FRP, so there is no evidence of the plausibility of the hypothesis of such effect. Actually, root colonization of cores could have several outcomes, different from those depicted in the examples 1-4 of Fig. 1 of the comment. For example, in the larger cores, which have a larger perimeter, more roots could colonize the root-empty space than in the smaller ones, simply because of the different size of the surface of the outer cylinder. On the other hand, after the initial colonization phase, root growth is expected to be isotropic, and in such case, ingrowth cores probably behave similarly to sequential cores, where results are expected to be independent of core size.

The comparison of our results from sequential and ingrowth cores showed that root colonization of soil cores in white sands under the ingrowth method could not reach values as high as those recorded in the sequential cores. This result suggests that probably more important than the diameter of auger is the modification of the physical properties of soil produced by the ingrowth method.

Page 3422, line 3) Testing for significance of changes over time needs to acknowledge non-independence of values taken from the same points/plots at different time periods. The most common method is to use a repeated-measures ANOVA.

As we explained in the section of methods, we divided each 1-ha plot in 13 or 14 sampling areas, located approximately 40 m apart of each other. This means that every sampling area had about 700-750 m2 of area Then, subsequent samplings were ran-

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domly done in each of these sampling areas. Even though we did not check for spatial independence of fine root distribution, we assumed such distribution is mainly random and then samples are independent. For this reason we did not see the necessity of using the repeated-measures ANOVA.

Page 3422, line 14-19) Why was annual growth extrapolated from different portions of the year, for the different plots and establishments? If growth varies seasonally, differences in annual estimates could be at least partially due to the portion of the year from which annual values were extrapolated.

Ingrowth cores were established at three dates; each establishment provided information about root growth from the date of establishment (time zero) until the first harvest, which occurred 6-10 months later; then, results were scaled to a year. Time periods were almost identical in establishment 3; comparison of root production in establishment 2 could be problematic because time periods were somewhat different (0.52 and 0.77 years for Amacayacu and Zafire, respectively), though both experiments started simultaneously. The decision of comparing fine root production for establishment 2 based on a different time period for each forest was based on the different patterns of root growth between these forests: after 0.52 years, fine roots of forests on white sands are vigorously growing (Figure 3); actually, growth rates were even faster in this forest type between 052 and 0.77 years. Therefore, estimating root growth based on the first 0.52 years would underestimate the true growth rate and therefore, we decided to estimate growth based on 0.77 years. On the other hand, growth of fine roots in clay forests slowed down after 0.52 years because at this time started the dry period of 2005, which affected markedly root growth in this forest type. Therefore, to estimate root growth in this forest based on first 0.77 years would underestimate the production in this forest type. For this reason we decided to estimate root growth based on 0.52 years in this forest type. To show these effects, we estimated root growth in both forest types for 0.5 to white sands and 0.7 years: Table Stablishment 2.

Page 3423, line 1) This seems strange. From what I understand of the drought effect analysis, you compared the clear drought period in 2005 with the normal wet seasons immediately before and after. Of course there is a difference, as there would be in any year, surely the appropriate way to specifically analyse the impact of the unusually severe 2005 drought would be to compare with the corresponding dry season periods in 2004 and 2006, I guess you would get a difference but it would not be so big.

We answered this question before.

Page 3423, line 12) I'm not very familiar with the sequential core methodology. But how do you know that the sharp seasonality in the "clayey forest" was a bias, rather than a real pattern? Presumably, if you had included this portion your estimate of growth would have been much higher for this plot?

We recognize that the way we wrote this sentence is confusing: results for the whole study suggest that fine root mass increases in December. However, we did not have samples for December 2004; actually our first sample was taken in September 2004 and the second one in April 2005. So, we lost temporal variation between these two dates, which is expected to be large. Therefore, estimating fine root production in this period (which results from the difference between the highest and the lowest value) would be greatly underestimated and for this reason we did not use data of this interval for the estimation of root production.

Page 3426, line 1) I like this analysis, but there are so many significant correlations one wonders how much to read into the detailed differences. Looking at table A1 it looks like the key, robust take-home message is (1) fine root mass increases with more rain on the clay soil, but decreases on the white sand plot. What is the explanation for the apparent negative correlation between fine root mass on the clay soil over long time lags, but not over short time lags?

Response to this question appears in the Discussion chapter, page 3430, line 25 to page 3431, line 8

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Page 3428, line 17) Here and throughout the manuscript you should consider clarifying/ changing this argument about "soil resources". This term seems too general, specifically what aspect of soil resources do you hypothesise will alter allocation?

Soil resources (water and nutrients) cannot be analyzed separately because we do not have enough data for a formal analysis. Our research shows the following:

1. Water: we used rainfall as an indirect measure of soil water and we found that rainfall is correlated with temporal variation of FRP and FRM. We also showed that response of each forest type to water is different. 2. Carbon allocation to fine roots is different in both forests. It suggests that nutrients could be playing another role, too. Which one? It will be the question to answer in the future. Soils maintain a control on FRP (there are differences in belowground allocation) and, in combination with rainfall, influence the seasonality of the response.

Page 3428, line 22) This is an intriguing scenario, you may expect forests on infertile forest to conserve resources by retaining their resource-acquisition tissues for longer, by investing more in chemical defenses. So do you also see evidence for slower turnover of roots on the white sand plot?

Our results do not allow us to conclude that root turnover is slower in white sand soils. Actually, the compilation in Table 4 shows that root turnover in forests presented high values (0.53, 0.66, and 0.84 year-1). But this is a plausible hypothesis that needs to be tested in the white sands forest.

Page 3429, line 15) I generally agree with this, though it is worth considering that you could see it the other way: : :that the soil was shaped by the poor quality litter supplied to it by the forest. The really interesting question is: how bad do conditions (climate and/or soils) have to be before a forest "tips over" into a state of producing poor quality litter which in turn further reduces soil nutrient availability? (see David Wardles work on nutrient cycling in long-term ecosystem chronosequences (Science 2004, issue 305, pages 509-513)

Even though, vegetation is a powerful factor for soil formation, there are geologic/geomorphologic influences that definitely shape the soil type. In page 3419, line 20, we described the different origin of soils of the studied forests (see below). On the other hand, we find extremely interesting the point presented in the paper cited, but it is beyond the scope of this paper.

"AME and AMU belong to the geologic unit named Pebas or Solimoes Formation; the terrain is slightly undulated and uniform, with soils moderately deep, well drained, and strongly acidic with texture moderately fine (Herrera, 1997). Soils from ZAB belong to the Terciario Superior Amazonico unit (Herrera, 1997; PRORADAM, 1979), probably originated from the Guiana Shield (Hoorn, 1994, 2006), and composed mainly by quartz. The terrain is flat and uniform, with a hard-pan at 90–100 cm depth (Quesada et al., 2009)."

Page 3429, line 19) Maybe it's a bit strong to say that your results make it clear that NPP allocation is different between your plots. There are substantial (and unquantified in table 5) errors around all of those terms. I think you can say that this study (1) suggests that there are differences in NPP allocation at these plots, and (2) provides a strong cautionary warning against assuming that patterns of total ecosystem NPP can be adequately understood/studied solely from above-ground NPP.

We recognize that this paragraph is confusing. The two conclusions of your comment are excellent. Thanks a lot. We will include them in the manuscript. We also want to say the following: 1) Carbon allocated to belowground parts –fine roots– is different between plots 2) Results of this study concord with the differential carbon allocation hypothesis. 3) Our data suggest that probably Total NPP (above plus belowground) is not very different between the plots

Page 3430, line 26/27) Do you have any theories why FRM would be positively correlated to recent rainfall but negatively correlated to older rainfall (120-150 time lags)?

This question is answered in lines 3-8, page 3431: "Differences of the response of both forest types to rainfall are explained by differences of their soils: soils of the white-sand forest contained a hard pan at 90–100 cm depth, which produces water logging during rainy season and impedes growth of fine roots; this is shown by the negative correlation of FRM with rainfall of last 4 months. This phenomenon does not occur in the clayey forest, which responds positively to the increase of rainfall."

Page 3433, line 8) Alternatively it could be caused by the combination of small core size and insufficiently frequent sampling, so that much of the growth is missed because it occurs after the roots have already passed through the core (as in Core 3, see image above).

This question was already answered above

Page 3433, line 18) Here, and throughout the manuscript, I would be careful about claiming that these results necessarily closely reflect carbon allocation to fine roots, because you haven't measured respiration and exudates which could well vary according to soil type/climate. Still, this isn't a big problem, measuring root standing crop and production is interesting enough, and certainly represents a very significant challenge.

We agree with this point. In this manuscript we will eliminate carbon allocation to fine roots and replace with: "Carbon allocation to production of fine roots".

Table 1) If you also have above-ground standing biomass, it would also be interesting to compare above- and below-ground standing biomass (as you have done for production in Table 5). Do you have plot LAI and specific leaf area estimates, then you could estimate canopy foliar biomass too.

We do not have data of belowground standing biomass, as much as data of coarse roots are lacking. We do have data of aboveground biomass as well as of LAI.

We will send the list of changes in the manuscript including the Technical corrections.

Interactive comment on Biogeosciences Discuss., 6, 3415, 2009.

Stablishment 2	Forest on clays soils					Forest on white sands			
	N	Mean		SE		N	Mean		SE
Sep 2004 - Abr 2005	***(0,52 and 0,54 years, respectively)***								
0-10 cm	22	2,104	a	0,359		26	2,248	a	0,287
10-20 cm	22	1,243	a	0,213		23	1,690	a	0,230
Total	22	3,346	a	0,474		23	4,030	a	0,434
Sep 2004 - Jun 2005	***(0,74 and 0,77 years, respectively)***								
0-10 cm	26	1,513	a	0,184		26	3,530	b	0,520
10-20 cm	26	0,316	a	0,044		26	2,404	b	0,414
Total	26	1,829	a	0,214		26	5,935	b	0,773

Fig. 1.

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