

## ***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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Received and published: 7 April 2009

### General comments

This paper presents data on the magnitude and controls on root standing biomass, growth and turnover from tropical forest in the Brazilian Amazon. Data of this type are relatively rare, particularly from this part of the Amazon and the white-sand soil type, the analysis of the correlations with rainfall is novel and the results should be of interest to readers.

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. I think there are two key scientific issues (see specific

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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.

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.

#### 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.

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

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(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?

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? 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? 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.

Page 3422, line 3) Testing for significance of changes over time needs to acknowledge

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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.

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.

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.

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?

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?

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?

Page 3428, line 22) This is an intriguing scenario, you may expect forests on infertile

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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?

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)

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.

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)?

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).

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.

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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.

#### Technical corrections

Page 3416, Line 2) Change to “gradient of increasing soil resources”.

Page 3416, Line 12) Can a forest be “clayey”? Change to “fine roots than the forest on clay rich soil”. To make it tie more directly to the first mention, perhaps substitute “clay rich” for “loam”.

Page 3416, Line 12) You should specify that this is a production estimate for the surface 20 cm soil layer (or is it?).

Page 3416, Line 14) See comment about “clayey forest” above.

Page 3416, Line 15) Is “fine root mass” referring to the standing crop? If so, specify this to clearly distinguish it from production (which is, after all, also fine root mass) remain consistent with conventional terminology from existing literature.

Page 3416, Line 18) This final conclusion is a very long sentence, divide it up.

Page 3416, Line 20) This doesn’t make sense. . .what aspect of carbon is different? Perhaps change to “the quantity of carbon allocated”

Page 3416, Line 21) Change to “above/belowground growth”.

Page 3416, Line 22) It safer to change “probable” to “possible”, unless you have good evidence for this?

Page 3416, Line 22) Change to “no differences”.

Page 3416, Line 23) Is this what you think your data suggests? If so, directly say it.

Page 3417, Line 2) Change to “because of its”.

Page 3417, Line 5) Change to “understanding of NPP”.

Page 3417, Line 16) This is perhaps a good point to define what diameter constitutes “fine roots”,

Page 3417, Line 20) Nice summary/justification sentence. Though perhaps “exuberance” isn’t really appropriate or very useful.

Page 3417, Line 24/25) This doesn’t really make sense. Perhaps change to “plant growth has highlighted the necessity”.

Page 3417, Line 28/29) There are also more recent papers on this from Trumbore (2006), Metcalfe (2007, 2008) and Aragao (2009) which you reference elsewhere.

Page 3417, Line 29) Change to “contributes information about carbon allocation”.

Page 3418, Line 2-8) Number these questions in bullet points to make them clearer.

Page 3418, Line 3) You need some way to make the difference between root mass and production completely clear. I suggest that you refer to mass as standing crop mass throughout the manuscript. Though I know that this term is a bit inappropriate, since it obviously originated from agricultural studies, it is used by the majority of comparable studies.

Page 3418, Line 3-5) Change to “How do these variables change with soil depth”.

Page 3418, Line 5) Do you mean variation in both production and standing crop? These are obviously related but both interesting.

Page 3418, Line 9) You also specifically look at turnover, and get really interesting results, so you should mention this in your objectives, and in the justifications/summary in the introduction

Page 3418, Line 20) These 3 paragraphs seem strange and out of place. Either incorporate a shortened version in the earlier justification, or move to the discussion. As

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it is, the introduction reads almost like two independent, self-contained introductions coming one after the other.

Page 3420, Line 5) Change to “significantly through the year”, and “Relative air humidity”.

Page 3420, Line 17) You have 13/14 areas per plot for sampling of fine roots but later (page 3421, lines 5-9) mention many more cores. You need to clarify what’s going on, am I confusing the root standing crop and production sampling systems?

Page 3420, Line 23) So the cores are < 1m apart but the fine root sampling “areas” (line 17) are 40 m apart. Clarify the sampling strategy.

Page 3420, lines 12/13) I think it’s good to distinguish these terms, and shorten them, but does FRM and FRP make sense since the FR part is essentially obsolete because it occurs in both, they may as well just be M and P! What are the acronyms used in existing literature? Also, the acronyms used are different in the table 4 title, whatever acronyms you decide on apply them consistently throughout the manuscript.

Page 3421, line 16) Do you have some estimate of how much very fine root material passed through the sieves, remained uncollected?

Page 3421, line 25) Change to “The monitoring interval”.

Page 3423, line 8) This doesn’t make sense. Change to something like: “To analyze the same time intervals for all plots, even though the length of monitoring was different between plots, we selected two years”

Page 3424, line 7) How much higher? Be specific.

Page 3425, lines 1-13) These are the key interesting numbers we want to know, so I advise you present them at the beginning of this section. With the RGR analysis, see my comment above about the validity of comparing September-December (wet season) growth before and after the 2005 dry season as some measure of the impact

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of that exceptional dry season.

Page 3425, line 18) See my comment above about the need for appropriate statistical analyses which account for the nature of the data (repeated measures from the same location over time).

Page 3428, line 6) Change to “near the surface.”

Page 3428, line 7) Change to “because they allow”.

Page 3428, line 8) Change to “in infertile soils”.

Page 3428, line 11/12) You haven’t yet presented any data on above-ground growth on the plots, so it’s not yet clear that allocation differs between plots. You need to present and discuss this data here.

Page 3428, line 18) Change to “carbon allocation below-ground with the”.

Page 3428, line 21) Perhaps change to “such as tannins, to retard litter decomposition and hence slow down the rate of nutrient cycling, could result”

Page 3428, line 23) Change to “improve the supply of nutrients to the plant.”

Page 3428, line 24) Why was this additional data not presented?

Page 3429, line 3) Change to “important factor affecting above-ground NPP”.

Page 3429, line 4) Change to “they found that there were no obvious relationships”.

Page 3429, line 6) Change to “solar radiance”, and “did find evidence for a trend between above-ground wood productivity and general classes”.

Page 3429, line 17) Change to “for above-ground NPP”. This is consistent with most of what you’ve written in the manuscript, and other literature.

Page 3430, line 16) Perhaps change to “rainfall has been found to be one of the most influential factors affecting FRM”.

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Page 3430, line 19) Change to “year there occurs”.

Page 3432, line 3) Change to “by the hardpan”.

Page 3432, line 14-16) Have soil moisture levels actually been measured at these times, or at any regular periods over the year to support this idea.

Page 3434, line 1-3) I have concerns about this drought analysis, see comments above.

Page 3434, line 4) This is a really nice result. Change to “Results from the forests studied suggest that they allocate NPP resources differentially”.

Page 3434, line 5) Change to “considering that the total NPP differences” and “exist, this raises the hypothesis that total NPP does not vary with respect to soil as it does for wood”

Page 3434, line 9) Change to “functioning of Amazonian ecosystems”.

Page 3434, line 12) Perhaps change to “fundamental considering the events” Table 2) Nice summary of interesting data, but again why was sampling frequency different between plots in establishment 2 (0.52 and 0.77 years), is there a way of analyzing whether this could have made any difference to the final annual production estimate. I think that presenting values to 3 decimal places is a bit optimistic given the substantial errors/variation, I suggest rounding everything up to 1 decimal place.

Table 3) See the decimal places comment above. ZAB production and turnover values appear to be off to the side of the corresponding plot values above them.

Table 4) The acronyms here are different to the rest of the manuscript. TR is a bit similar to TF, this might be confusing. The rhizotron estimates from Metcalfe et al. (2007) are using contrasting conversion methods on the same sand plot in the Metcalfe et al. (2008) paper, this is perhaps worth mentioning. The ingrowth estimate from the 2007 paper (3.70 t ha yr) is therefore the same as the sand plot estimate from the 2008 paper (4.00 t ha yr), so one of them should be removed (I suggest the 3.70 value is

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removed). I still think that presenting values with 2 decimal places is unwarranted.

Table 5) See comment about decimal places above. It would be nice to include above- and below-ground comparisons here of standing biomass and turnover. Most of the manuscript is in units of Mg biomass, why convert to C here?

Figure 3) How can accumulated fine root mass decrease? Were you also measuring mortality? I'm confused about your system for assignment of significant differences.

Figure 4) See my various comments about the drought analysis, above.

Figure 5) Interesting differences in growth during the 2005 between plots. I like your explanation about the white-sand hard pan, do you have additional soil moisture data or something else to back it up. Also, as always with sequential coring one wonders how much of these changes are random differences in sampling (were you just "un/lucky" to pick a few cores with lots of roots immediately after the drought in the clayey forest plot?).

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

**BGD**

6, C77–C88, 2009

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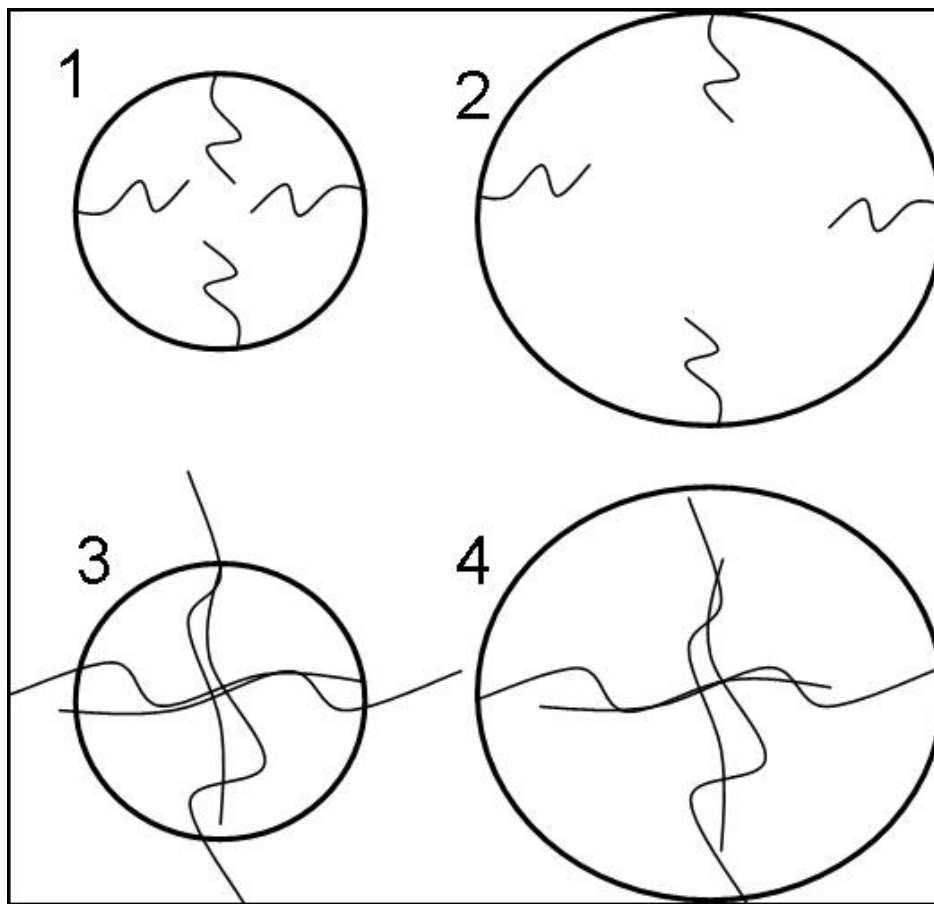
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**Fig. 1.** Effects of core size on estimates of production

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