Biogeosciences Discuss., 5, S2173–S2177, 2008
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Biogeosciences Discussions

Interactive comment on "Do species traits determine patterns of wood production in Amazonian forests?" *by* T. R. Baker et al.

Anonymous Referee #2

Received and published: 4 November 2008

This manuscript combines a substantive new data-set on wood density and maximum tree height in three clusters of plots across Amazonia with prior data on wood productivity and soil fertility to test the dependence of wood productivity on traits (species composition) vs environmental drivers. The issues are important for current debates about the patterns and causes of spatial variation in productivity; response to climate at the regional scale; and biodiversity and ecosystem function. This group is uniquely well-placed to address them for continental Latin America.

The strengths of this paper are its broad scope in terms geographical coverage and effective mining of a large data-set using a sequential hypothesis-testing approach. Set against that, I found the paper difficult to navigate. It was not always immediately clear what data were new and what had been published elsewhere; the methods and as-

sumptions for the analyses were not always made clear; terminology for the measured quantities and species functional groups were confusing; and some analyses may not be appropriate.

First, try to standardise terminology where possible, and when you cannot do so explain and justify why you use different terms for related quantities. For example, the distinction between the terminology for metrics of growth (of individuals averaged at the functional group level) and productivity (of stands) is not made explicit, and different terms are used inter-changeably. For example I encountered all of the following terms in the paper for what (I think) is probably the same quantity: stand-level productivity, ANPP, above-ground wood production, coarse wood production, biomass increment and wood productivity. The labile terminology persists even between references from the text to figures (e.g. biomass growth rates in lines 294-299 becomes wood productivity in Figure 4). It would be kinder to your readers to unify this terminology.

Essentially the authors report an interesting negative result (stand productivity is higher on more nutrient rich soils, but trait values do not contribute much explained variance in spatial variation). They need to reflect on whether this result was genuine, or whether their study lacked the precision and power to determine a trait signal if one existed. Despite the impressive data-set of individuals on censused plots, the analyses are dependent on databases of wood density and maximum height that are likely to be quite noisy; these traits are assumed to be fixed and independent of location and environment; diameter/biomass allometries determined from one location are extrapolated across the entire region; and proportional allocation to leaf vs wood production is invariant with environment. Could it be that the uncertainties associated with these assumptions might sum to a substantial bias that simply swamps the signal for trait variation in your data ?

Lines 74-75. At this stage in the paper it would be appropriate to explain the significance of the distinction between biomass growth rates and stand-level productivity for these two questions.

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Lines 104-105. Equation 1 is poorly annotated. Presumably nj is the number of entities of j (species or life-forms); Mj and RGR are the mean mass and relative growth rates of the Nj individuals of the nth class of j; and RGR requires subscript j. Why does one take 1 from the expression within the parentheses ?

Lines 134-135. Suggest delete the word ‘standardised’ from this sentence. The procedures adopted by Baker et al. (2004b) may be entirely acceptable, but can it be argued that they now represent a global standard ?

Lines 148-159. Comment on the quality of the trait data from these sources. How many individuals were measured per species ? For wood density, what range of tree size was sampled and across what geographical spread ?

Lines 182-183. I found it very hard to interpret what you meant by the sentence ‘Associations between … chi-square tests’. ? Table 1 suggests that what you’ve done is a goodness of fit test, i.e. testing the distribution of counts of species within families among stature classes against a null expectation of equal frequency among classes. Is that the case ? If so, I’m concerned that you are using species as independent data points when, by your own admission, maximum height is a phylogenetically conserved trait. It’s not clear to me that this analysis contributes anything substantive to the main themes of the paper, it isn’t mentioned in the abstract, and could be deleted without loss of impact.

Lines 187-189. What is the significance of these five new plots ? Are you giving us this information because the estimates of coarse wood production for these five plots are not available in Malhi et al. (2004) ?

Lines 193-196. Labels for stature classes (sub-canopy/canopy/emergent) are misleading, because they imply an exposure criterion in relation to the forest canopy, whereas the intention is that this is a proxy for a maximum size classification. It would be preferable to label these with a maximum size descriptor (small, medium, large). According to the original terminology a small tree could be either a shaded (i.e. genuinely sub-

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canopy) tree or indeed a small pioneer tree with its canopy exposed in a gap.

Secondly, what is the significance for these particular cut-offs for defining the nine functional groups ? Do they separate the taxa into approximately equal groups along each of the axes ? Or perhaps there is a structural reason for recognising 20 m and 30 m as key height thresholds for neotropical forests ? Or precedents in the literature ?

Lines 207-209. Do we take it that the functional-group mean values of absolute growth rate were calculated without consideration of species-level differences (i.e. these are not means of species-level means) ?

Lines 209-210. Does the abundance of palms and Strelitziaceae vary regionally in a way that could bias the analyses reported here ?

Lines 218-220. Not clear to me how resampling solves the skewness problem, nor in fact how it was done. Greater clarity here would be helpful.

Lines 272. Suggest re-word to ‘Mean wood density and maximum height per stem was negatively correlated with wood productivity per plot …’.

Lines 296-299. Wording needs some revision, as it currently reads as if biomass growth rates were significantly lower in NW and SW Amazonia than CE Amazonia in 14/24 comparisons in both cases, which is the converse of what you mean.

Lines 365-368. I suggest greater caution in inferring a biogeographical explanation for the inter-continental contrast that is reported by LaFrankie et al. (2006). Note that their sample of sites in the palaeotropics does not include any that approach the fertility of your western Amazonian sites (in terms of base cation concentrations, at least). More comparable sites do exist in the wet Asian tropics, and in my experience they possess the high density of understorey trees that you report for western Amazonia – but unfortunately they are not yet sampled by the network of large scale forest dynamics plots.

Discussion. Why ignore lessons from forest fertilization experiments in your interpre-

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tation ? It strikes me that the most direct test of whether soil fertility limits wood productivity in the absence of changes in forest composition is to experimentally increase soil nutrient availability at the whole-stand scale, using fertilizers or litter addition. Admittedly there are very few such experiments in the lowland tropics, but they do exist (Vitousek, Tanner, Sayer, Proctor). Worth a mention.

Figure 4. Line graphs are inappropriate because the x axis is not continuous. Bar charts or unconnected points would be more suitable.

Interactive comment on Biogeosciences Discuss., 5, 3593, 2008.