

## ***Interactive comment on “Variation in photosynthetic and nonphotosynthetic vegetation along edaphic and compositional gradients in northwestern Amazonia” by M. A. Higgins et al.***

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Dear Dr. Stoy,

Many thanks again to you and to the two anonymous reviewers for your rapid handling of the manuscript, and the clear and constructive comments on the manuscript. We have addressed all of these comments, and believe the manuscript is greatly improved as a result.

Following are our responses to the reviewers' comments, and the changes we have made to the manuscript. We believe the revised manuscript will make an excellent contribution to Biogeosciences, and please do not hesitate to let us know how we can further improve the manuscript.

Very best regards,

Mark Higgins

### **Anonymous Referee #1**

Received and published: 14 April 2014

*Overall paper is very well written and edited and was enjoyable to review. Items to be addressed:*

*1> Lack of reporting the accuracy of the PV NPV classification. Discussion of ground observations in relation to accuracy of the classification method of the Landsat data*

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*is relevant and important information for the overall impact of the study. Similarly the bare substrate class is not concluded. Were there no bare substrate pixels in the study region?*

Our sincere thanks to the reviewer for their positive comments, and we are very pleased they enjoyed reading and reviewing the manuscript.

In regards to the reviewer's first question about the accuracy of the PV and NPV maps used in this analysis relative to ground data, we were unfortunately not able to collect ground estimates of photosynthetic and nonphotosynthetic vegetation for our study sites. This was due both to the time restrictions on working in these very remote locations, and the difficulty of estimating these parameters. In total, we visited 117 sites for inventories of soils and plant species composition, and the cost of additionally measuring quantities of ground and canopy vegetation, living and dead, would have been prohibitive.

Fortunately, the Carnegie Landsat Analysis System (CLAS) has previously been tested for 197 Landsat images covering the entirety of the Peruvian Amazon, and found to be remarkably accurate in estimating these variables (Asner et al. 2005 a and 2005b, as listed in the manuscript). Uncertainties for PV and NPV estimates ranged from 1-4%, and the utility of CLAS was subsequently supported by studies in both Brasil and Peru (Asner et al. 2005a, Science; Oliveira et al. 2007, Science). These high accuracies are possible primarily because the spectral endmember libraries used in CLAS were created and optimized for Amazonian forests. We have added additional text to the methods to explain the prior use of these methods in Brazil and Peru.

In regards to the second point raised by the reviewer regarding bare substrate, bare

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ground was indeed exposed in most pixels. However, because we were primarily interested in the response of tropical forest vegetation (PV and NPV fractions) to soils and plant species composition, we decided to omit the BA fraction from our analyses for the sake of clarity. In addition, we do not expect a direct response of soil quantities to soil or plant species composition, other than the secondary response due to the thickening or thinning of the forest canopy. If desired, however, BA fractions can be approximated as 100-PV-NPV (i.e. the fraction left after PV and NPV are accounted for).

*2> Which Landsat satellite data bands were incorporated in the SMA ? Where all the data from Landsat 5 or 7?*

CLASLite uses all Landsat bands with the exception of band 6, the thermal band. In addition, we used images from both Landsat 5 and Landsat 7 (for the latter, only images prior to the scan line corrector system failure were used). This information has been added to the methods section.

*3> The terminology of Non-photosynthetic and Photosynthetic vegetation seems misleading and I would suggest using different terminology that is more reflective of the classes.*

We appreciate that the terms "photosynthetic vegetation" and "non-photosynthetic vegetation" may seem vague, but these are standard terms in the literature on this subject. We have thus decided to use them in order to remain consistent with previous publications on this subject. To address this concern, however, we have added text to the methods section that clarifies that, when talking about photosynthetic or nonphotosynthetic vegetation quantities, we are referring specifically to the products

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of the spectral unmixing process.

**Anonymous Referee #2**

Received and published: 16 April 2014

*This is a well-written manuscript that documents the relationships between Landsat-derived estimates of photosynthetic and nonphotosynthetic vegetation and field-derived estimates of soil fertility and species composition in Amazonian forests. The scientific significance of the findings and approach documented in the manuscript are of excellent quality, as is the overall presentation quality. My primary concerns and comments stem largely from insufficiently explained methods and prior results. In some instances, where key methodological details or prior results were previously published and, therefore, intentionally omitted, a very brief re-cap in this manuscript would significantly clarify important points for the readers. Specific comments are as follows:*

*Page 3537, lines 13-15: Suggest clarifying sentence – “Individual pixel values in Landsat imagery typically consist of reflectance from a mix of substances (features?) including...”*

Our many thanks to the reviewer for their careful reading of the manuscript, and for their very useful and positive comments. This sentence has been revised to make it clearer.

*Page 3537, lines 21-29: Are there any potential errors associated with the definition of endmembers from such diverse approaches? In other words, how do the aerial estimates of PV endmembers compare to the ground-based estimates of NPV and*

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*bare substrate endmembers? Are these approaches expected to yield equivalent estimates of endmember spectra? In addition, is there a sampling framework for endmember estimates?*

Yes, despite the diverse approaches used to construct the endmember libraries (from field to orbital measurements), the endmember libraries for NPV and bare substrate (generated from field measurements) are completely comparable with endmember libraries for PV (generated from orbital measurements). In addition, the errors for NPV and bare substrate, and PV, are remarkably similar. As noted above (see responses to Reviewer 1), Asner et al (2005b, Earth Interactions), using 197 Landsat images covering the entirety of the Legal Amazon, found that uncertainties for estimates of PV, NPV, and bare substrate all fell into the same range of 1-4%. In addition, CLAS was successfully used in Brazil and Peru (Asner et al. 2005a, Science; Oliveira et al. 2007, Science) to map forest loss and degradation at national scales. As noted above, the success of this algorithm is probably primarily due to the fact that these spectral endmember libraries were created and optimized for Amazonian forests.

Unfortunately, a full review of this subject is beyond the scope of this paper. To answer questions about accuracy, however, and explain the prior use of these methods in Brazil and Peru, we have added additional text to the methods on these subjects. We have also added additional text to the methods section to explain how endmember estimates are produced, in order to address the reviewer's question about the sampling framework for PV and NPV estimates. More information is also available at Asner et al 2005a, 2005b; and at Oliveira et al. 2007., as referenced in the manuscript.

*Page 3539, line 6: What transects? I assume this is in reference to the plant inventories and soil samples, but the term transects is abruptly introduced here.*

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The reviewer is correct that “transects” is in reference to the plant inventories and soil samples, which were conducted along linear transects. However, because these transects have not yet been explained at this point in the manuscript, we agree that it would be better to use “inventories” here. We have changed the text accordingly.

*Figure 1: Suggest adding country names to inset map and/or to 1a.*

We agree that it would be useful to include this information on the figure itself, and we have experimented with including text in the inset for Figure 1a. However, in order for the text to be larger enough to be readable, it begins to clutter the inset. We have thus edited the figure caption to make the location of Figure 1a and the inset clearer. We hope this addresses the reviewer’s concern.

*Table 1, and accompanying text on page 3540, lines 9-11: There is significant monthly variation in Landsat images for the Pastaza-Tigre study area. Is there potential for differences in phenology to confound the analysis?*

As the reviewer notes, the Landsat images used here vary in their months of acquisition. Because these study areas are notoriously cloudy, our primary objective was to gather all available cloud-free imagery regardless of date. Unfortunately, placing additional limits upon our selection, such as trying to maintain the same month, would have significantly reduced the size of our dataset.

This said, both of our study areas are relatively aseasonal and do not exhibit clear phenological cycles, due to their close proximity to the equator (between 2 and 3 degrees south). In general, the variation in precipitation within months at these locations exceeds the variation between months. Furthermore, the variation between

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our images in month of acquisition was surprisingly low: the majority of images were captured in the three-month period between July and September (6 of 8 images), and the remaining two images were captured in October and January. We thus do not expect phenology to have a substantial impact on our analyses, and we have added text to the methods section explaining this.

We do, however, observe date-to-date differences in the magnitudes of PV and NPV scores, and this is revealed in the relationships between PV and NPV, and soils and species composition, as shown in Figure 2. We believe this is due to illumination conditions or possibly the condition of the vegetation. Despite these differences, the form of the response of PV and NPV values to changes in soils and plant species composition is essentially identical for all images at both study areas. In addition, as described in section 2.4, we standardized for this variation between images using the difference between PV and NPV. After this standardization we continue to see strong relationships between soils and plant species composition, and the normalized PV-NPV values.

Last, if phenology or other date-to-date variations in imagery are interfering with our analyses, we would expect the strength of the relationships described here to be underestimated, and the true relationships to be substantially stronger. As such our findings are, if anything, conservative estimates of the strength of the relationship between PV and NPV, and soils and plant species composition, and we have noted this in the methods and discussion sections.

*Page 3540, lines 9-18: What about bare-ground estimates? The introduction discusses a bare-ground endmember, but it does not appear that these estimates were included here. Why not?*

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The reviewer is correct that analyses using the bare ground fraction were not included in this paper, as also noted by Reviewer 1 (please see above). Because our primary purpose was to analyze the variation in PV and NPV along soil and compositional gradients, we did not want to complicate our analyses and presentation by including the bare fraction. This could be a good topic for future study, however.

*Table 1: Could this table be expanded to also include a validation measure of PV and NPV estimates? How accurate were those models?*

As noted above (see responses to comments on Page 3537, lines 21-29; and response to Reviewer 1), systematic studies across western Amazonia have found relatively low uncertainties of 1-4% for both PV and NPV scores from CLAS. We have thus added text to the methods section clarifying the accuracy of this method. We may also be able to derive uncertainties for individual Landsat images in Table 1, but we are hesitant to include these values. To explain, our analyses in Table 1 and Figure 2 are based on average PV or NPV values for 50 to 60 transects at Curaray and Pastaza-Tigre, each of which are calculated from 500 pixels within each transect. As such, uncertainties in the PV and NPV estimates are currently averaged over 500 pixels within each transect, and then across the 50-60 transects within each study area. We could attempt to report a single average uncertainty value for image, but this might not be useful to readers, and may be confusing and difficult-to-explain.

Furthermore, the main consequence of uncertainty in PV and NPV estimates would likely be to weaken the relationship between PV or NPV, and plant species composition and soils. As such, our measures of the strength of the relationship between these variables (i.e. the coefficients of determination ( $r^2$ ) in Table 1) which are already quite high, are thus conservative estimates. We have also added text to the methods and discussion to explain this. Based on prior studies and the strength of the relationships

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observed here, we are thus confident in the relationship between PV and NPV, and soils and plant species composition.

*Page 3541, line 27: "Each of these three soil samples consisted of five random(?) subsamples: . . ."*

To clarify, we have added the following text to the methods: "These five subsamples were located such that one subsample was located in the center of the transect, and the remaining four subsamples were placed at two meters forward along the transect, back along the transect, to the left, and to the right."

*Figure 1 and accompanying text on page 3542, lines 5-11: It seems that additional information about NMDS results is warranted. What is the interpretation, for example, in figure 1, of "higher NMDS values" versus lower values?*

We are very happy to clarify this further. Ordination values such as NMDS scores indicate a dominant trend in a dataset and are generally unit-less. In this case, the trend captured by the NMDS scores at both study sites is the transition from the plant species assemblage on the poor soils of the Nauta Formation to the rich soils of the Pebas Formation, corresponding to an increase in NMDS scores. Note that this is explained on Page 3544, Lines 15-17, and we have added new text to explain the meaning of these scores.

*Page 3543, lines 3-14: Could a simple schematic help to explain this better?*

We agree that a schematic might be useful, but are concerned that this might

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detract from the main point of the paper. Fortunately, this method is fairly commonly used, and we have added a new reference to a paper that does provide such a graphic and more general information on variance partitioning (Peres-Neto et al. 2006).

*Page 3541, lines 1-14: I'm not 100% clear on how to reconcile the information presented here about only using the pteridophytes for the NMDS and the information presented on page 3543, line 22 onward about the NMDS ordinations (of pteridophytes data only) explaining 80-90% of the floristic patterns in the "original" datasets. First of all, what is meant by "original" here? Second of all, are the 147 and 127 species at the 2 sites ALL species or only pteridophytes? If these are ALL species, why were these data not used for the NMDS ordinations?*

We apologize for the lack of clarity here, and have updated the methods and results sections to make this clearer, as follows.

To answer these questions, the original datasets were the 65 pteridophyte inventories at Pastaza-Tigre, and the 52 transects at Curaray (117 inventories total); and these inventories contained a total of 147 pteridophyte species at Pastaza Tigre, and 127 pteridophyte species at Curaray. These are all pteridophytes species, and no other plant groups were inventoried for this study.

To compare our plant compositional data to the PV and NPV data, we used NMDS to reduce the changes in plant species composition at the two study areas into a single axis for each study area. Because all 147 species at Pastaza-Tigre, and all 127 species at Curaray, did not follow this axis perfectly, we measured the percentage of variation in the original datasets (containing 147 or 127 species, based on the Jaccard Index) that could be explained by the reduced dataset (containing one axis of NMDS

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scores). This is a standard mechanism for determining how well the data reduction performed, and how much of the information in the original dataset is present in the reduced dataset.

Fortunately, the NMDS performed remarkably well, and the single NMDS axes for each study area preserved 80-90% of the original data for all 147 or 127 species. As such, at each study area we were able to compare transects on the basis of four simple values: (a) soil cation concentrations, (b) NMDS scores (each transect had a single score), (c) PV values, and (d) NPV values.

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Interactive comment on Biogeosciences Discuss., 11, 3535, 2014.

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