

## ***Interactive comment on “Landscape-scale changes in forest structure and functional traits along an Andes-to-Amazon elevation gradient” by Asner et al.***

**Anonymous Referee #2**

Received and published: 10 December 2013

Remote sensing may offer a unique opportunity to extent detailed but spatially limited field measurements to study environmental controls on tropical forest growth and mortality. The objective of the study was to demonstrate the utility of novel remote sensing tools (lidar and imaging spectroscopy) to characterize changes in forest structure and function along an elevation gradient in Peru. I particularly liked the correlation analysis between structure metrics and spectral metrics and with elevation. The manuscript (although long) is well written, and, I believe, a useful contribution to the ecological literature. I am sure it will inspire future research activities. At the same time, I wish the author's would have provided a more critical examination of the potentials and limitations of these tools. I don't believe in easy answers; and some of the ecological con-

C7252

clusions drawn in this study require more careful examination. For example, the study seeks to confirm results from another field-based study that found biomass turnover rates were constant across the same elevation gradient. However, the results of the study under review, which are based on a lidar-derived gap-size frequency metric, are inconclusive. There are probably a variety of good reasons for that, e.g. difference in plot size, but these should not be overlooked. This is important, as other studies have argued that biomass turnover rates are linked to primary productivity across the observed ecological gradients. Similarly, the (advantages) and limitations of NDVI are well documented in the remote sensing literature, and the authors acknowledge that. Yet, their conclusion as a 'proxy for production at steady state' seems overly optimistic to me. This came to a surprise to me considering the author's expertise with sophisticated methods like lidar and imaging spectroscopy. My concern is that ecologists less familiar with remote sensing get the wrong message. I recommend the authors address my concerns prior to publication, which should not be too difficult with careful editing.

15416/10: Please make clear that canopy gap and understory were estimated with lidar, e.g. lidar-based canopy gap density and understory cover.

15416/12: Elevation was negatively related to vertical profile. What specifically do you mean with vertical profile here, canopy depth?

15418/28: Is there room for alternative hypotheses? Studies in temperate forests have found a clear trend with elevation (Stephenson & Mantgem, 2005).

15419/7-10: This is an important statement, but it is not quite clear. Are you trying to answer the question or question it? If canopy gap fraction is an indicator of forest turnover rates, than your analysis could help reveal trends with elevation. You can then compare and contrast them to field-based measurements. But how would you use your analysis to explore why biomass turnover is constant while NPP decreases with higher elevation?

C7253

15418/7: Please see also Moser et al. (2011). Their analysis of belowground and aboveground carbon pools on an elevational gradient in Ecuador indicated a transition from light to nitrogen limitation with increasing elevation and decreasing temperatures.

15419/11: Which unknowns?

15427/22: This normalization of the lidar height profile adjusts for horizontal variations in the sampling density but it does not adjust for vertical canopy occlusion. Detection probabilities decrease exponentially with increasing canopy depth and LAI. Thus, the cover estimates of sub-canopy voxels are not unbiased estimates of understory cover. There are ways to correct for occlusion and convert lidar height profiles to canopy height profiles, e.g. see Lefsky et al. (1999). I am not suggesting this has to be done. The general trends found in this study may not change. However, it has to be made clear that the understory cover estimates are in fact pure, unvalidated lidar metrics and should be interpreted with caution.

15428/23: It is curious that you do not find an elevational trend with fiPAR but with NDVI and PV. fiPAR (and fPAR) is a much more direct measure of canopy traits and photosynthetic capacity than the spectral measures NDVI and PV. The saturation of a simple metric like NDVI is not surprising, but that it is more sensitive than your spectroscopy-based fiPAR estimate makes me wonder about the meaning of your observed relationships. Often a linear relationship between fPAR and NDVI is assumed where background reflectance is negligible. Your correlation between fiPAR and NDVI is indeed higher than between NDVI and PV. On the other hand there is no correlation between fiPAR and PV, which again seems counterintuitive. Note, Table 3 shows a negative correlation between NDVI and fiPAR. Shouldn't it be positive?

15429/2: Table 3 shows non-significant correlation coefficients (e.g.  $\text{fiPAR} \sim \text{NDVI}$ : -0.66) that are higher than significant correlation coefficients (e.g.  $\text{PV} \sim \text{P:H}$  = 0.53). How is that possible when the sample size is equal? Is it not?

15430/10: Although you sampled a wide range of different landscape types, it is not

C7254

clear that these generalized conclusions are properly justified. Also, this finding is only based on the spectral metrics; the lack of correlation with fiPAR is ignored here.

15431/11: I am somewhat surprised that this study only reports results from remote sensing analysis. A more direct comparison with field data may have revealed interesting synergies and limitations between field measurements and remote sensing. Field data seems to exist for the studied locations.

15431/13: Please explain this in more detail. A decrease in aboveground biomass can be associated with a decrease in stand height and/or density. But how does that explain a constant turnover rate? Other studies suggest a relationship between NPP and turn over rates (e.g. Stephenson & Mantgem, 2005).

15431/18: I am confused. It seems you observed a weak but significant decrease in turnover rates (gap-size scaling) ( $r=0.3$ ). You mention landslides as a potential cause. But your argument is no convincing. Is there no reason to believe that the observed trend is true? There are certainly good reason why 1-ha plot studies do not match 20-ha plot studies. I think a more comprehensive discussion on the sources of error, both, for the field and remotes sensing analysis is needed, before such important conclusion can be drawn.

15432/4-15: Here you partly address one of my earlier comments regarding the negative correlation between NDVI and fiPAR. Your explanation is that NDVI is influenced by regrowth vegetation in canopy gaps, which is a reasonable explanation, though an "increase in greenness" should increase NDVI with elevation not decrease. An increase in shadow fraction with increasing gap fractions may be an alternative explanation. However, the statement that 'NDVI is more sensitive to turnover' is an oversimplification of a simple spectral metric that cannot be easily generalized. An increase/decrease of NDVI can be cause by a several different factors that are usually not known a priori. Otherwise, this would suggests that NDVI can replace lidar metrics, which I don't believe is the authors intent. I strongly suggest to clarify the limitations of NDVI (which

C7255

are well documented in the remote sensing literature) to avoid confusion with readers less experienced with remote sensing.

15432/16: You seem to be using the term disturbance synonymously to tree mortality or turn over rate here. I think there needs to be a clear distinction between these different processes. Elsewhere in the manuscript you bring up the concept of equilibrium turnover rates, which assumes that the sites are undisturbed.

Stephenson, N.L., & van Mantgem, P.J. (2005). Forest turnover rates follow global and regional patterns of productivity. *Ecology Letters*, 8, 524-531

Moser, G., Leuschner, C., Hertel, D., Graefe, S., Soethe, N., & Iost, S. (2011). Elevation effects on the carbon budget of tropical mountain forests (S Ecuador): the role of the belowground compartment. *Global Change Biology*, 17, 2211-2226

Lefsky, M.A., Harding, D., Cohen, W.B., Parker, G., & Shugart, H.H. (1999). Surface lidar remote sensing of basal area and biomass in deciduous forests of eastern Maryland, USA. *Remote Sensing of Environment*, 67, 83-98

---

Interactive comment on Biogeosciences Discuss., 10, 15415, 2013.