

Interactive comment on “Life cycle of bamboo in southwestern Amazon and its relation to fire events” by Ricardo Dalagnol et al.

Anonymous Referee #2

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General comments: This paper proposes methods to address a very challenging problem in remote sensing - distinguishing between two spectrally similar types of land cover (bamboo forest and bamboo-free forest) using moderate spatial and spectral resolution imagery (MODIS and Landsat). The aim is to map a little-studied, yet spatially expansive, ecosystem in the southwest Amazon - and to establish evidence (or lack thereof) for one hypothesis of bamboo forest establishment and expansion. The study focuses on a fascinating ecosystem, and remote sensing provides the most realistic means of collecting data over such a vast spatial extent in a very remote region. However, I have a number of concerns about the style, methods, and conclusions of the current manuscript, as follows:

1. The methods and results sections include overwhelming detail without clearly out-

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lining goals - both in terms of holistically linking the steps in the methods, and in terms of how the methods relate to broader scientific questions. I am left wondering how the conclusions inform our understanding of the origin and/or biogeochemical processes that maintain/promote expansion of the bamboo forest. 2. The great amount of detail included in the methods swamps any discussion of why specific methods were chosen. As a result, there are many places in the manuscript where the reader may be left feeling that they must take things on faith, or, that the steps presented are the result of a circular logic. 3. Several of the steps in the methods depend on thresholds which are determined from sample means, completely ignoring the impact of spectral (and/or spatial) variability. This topic is briefly addressed in the discussion section, but should be treated more rigorously throughout the methods and analysis sections. In fact, I am left feeling that the authors have failed to demonstrate the practical significance of some of their conclusions because the within group variability seems to overwhelm the between group differences. 4. In a similar vein, because many of the steps in the methods depend on sampling statistics, it is problematic that the sampling unit (point, pixel, polygon?) and sampling protocol (sampling strategy, sample size) remain unspecified in most sections of the paper. 5. Finally, I find it difficult to follow how each step and/or product is validated - in terms of methods, reference datasets, and sampling units and protocols.

Specific examples: 1. The level of detail presented in the methods often seems like a list of what was done, versus a careful retelling of the salient details. For example, the detailed comparisons of NIR1 to NIR2 get confusing and are perhaps unnecessary - an alternative would be to simply state that the two bands were each tested as input data and compared based on some criteria. The NIR 1 was determined to be more useful based on specified criteria. Then the discussion and figures that follow could focus on the results for NIR1 only. But, also see question below. Perhaps more importantly, additional discussion is needed to link specific results to broader scientific questions.

2. Steps described in the methods (and the results) often lack a presentation of the

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logic behind why specific methods were tested in the first place (and/or ultimately, selected). A few places where more justification is needed about proposed methods: - Why only test NIR bands (rather than combinations of other bands, standard vegetation indices, etc.)? Why assume a linear model? Why choose thresholds vs. probabilities? How confident are you in Carvalho's estimates? What percent of pixels are highly correlated?

3. The first two conclusions of this study - first, that bamboo-dominated forests have lower tree cover values than bamboo-free forests, and second, that the MODIS NIR values have different distributions over the two forest types - are not supported by convincing evidence. In each case, the differences reported are so small ($< 0.1\%$ tree cover and $< 0.1\%$ reflectance) that I would predict other sources of variability and/or error (e.g., radiometric calibration, sensor signal-to-noise ratio, atmospheric correction uncertainties) might overwhelm these differences.

4. In many cases, the methods lack a description of the sampling procedure employed to inform classification strategies. In all cases where data are sampled for "training" statistics, the authors should provide a concise description of the sampling unit, sampling protocol, and datasets sampled. For example, Section 2.3.1 mentions five pixels, 78 patches, and 380 geolocation points - but I am not sure how these numbers are related, nor what datasets are being compared. Section 2.3.4 mentions different numbers of pixels, patches, and geolocation points, and perhaps uses a different reference dataset? Additionally, no information is provided to place the samples within the larger context of the entire study area - i.e., what percent of pixels (or percent area) of the entire study area is sampled, and how might this impact confidence in the resulting predictions. That is, the predictive model is developed based on a very small sample of the entire study area, but little information is provided to assess what impact this has on the "predictive accuracy" for unknown pixels?

5. Building on the previous point, each validation dataset should be clearly (and concisely) described (perhaps summarized in a table) - and discussions of predictive un-

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certainty should be included in the results section (not just the discussion section).

Technical corrections/suggestions: Consider refining methods to state the goal of each step first, as well as to identify how each of the steps in the methods sections are related. Currently, the steps are presented in isolation from each other, and in some cases, the almost overwhelming detail makes it difficult to follow how the individual steps are related. Start in the abstract by clearly stating research questions and goals.

Clearly cite previous work and clearly identify which steps were followed in the current study.

Consider including a table to present the imagery used in the analysis, including Landsat WRS-2 and MODIS tile coordinates and image dates.

Check the use of the term “cross-validation.”

Limitations of the MODIS active fire detection product are mentioned at the very end of the discussion section - what implications does this have for ecological process? Could it be that the bamboo fire hypothesis is still an open question because we are not measuring understory fires?

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