Anonymous Referee #1
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Remotely-sensed forest disturbance and change monitoring has been proposed as a basis for improving and constraining vegetation modeling frameworks (McDowell et al. 2015). The current research (Negrón-Juárez et al. in review) proposes and tests components of such a framework by examining the utility of Landsat time series (LTS) data for detecting forest disturbance and recovery dynamics and comparing these results to ELM-FATES predictions for several forest attributes. Using LTS data to identify disturbance is common, but assessing recovery trajectories and timing of recovery is much less common. The use of LTS data for assessing forest dynamics processes is still a relatively new area of research (Schroeder et al. 2007). More rarely still has been the use of insights into recovery dynamics to assess the capacity of vegetation models to reflect reality and, perhaps, guide model development. Such efforts are particularly challenging in the cloud-covered tropics. The primary conclusions – Landsat-based NIR observations are sensitive to forest regrowth dynamics which compare favorably to model predictions – may prove useful both for the development of monitoring frameworks and for guiding vegetation model testing and development in tropical forests. The paper is well-structured, well-written, and of appropriate length for the material. The citations seems generally appropriate, though I do identify additional literature that should be discussed in the paper (see below). The title and abstract provide a clear and concise description of the work. I do suggest that the authors consider revising the final sentence of the abstract to provide a more impactful conclusion reflecting the potential impact of this work on disturbance and recovery mapping as well as vegetation modeling research. The methods and assumptions seem appropriate, though additional justification of some methods is required (I enumerate those in more detailed comments below). It should be noted that the small sample size of locations used in this study limits the generality of conclusions, but does seem sufficient to assess whether the proposed framework for integrating remote sensing with ELM-FATES is useful. The analysis was straightforward and seems reproducible by other scientists.

Authors: We thank the reviewer for the time and dedication reviewing this manuscript and the comments provided on the use of remote sensing data to improve and develop modeling schemes on disturbance and regrowth. We have included the suggested references and the following:

[Abstract, last sentence] Our results show the potential of Landsat imagery for mapping forest regrowth from disturbance and for validation, improvement, and development of forest regrowth models.

Here I raise several issues that should be addressed to improve the quality and accessibility of the paper.

1. I found the lack of use, or even discussion, of commonly used spectral vegetation indices, such as NBR, NDVI, EVI, or tasseled-cap wetness, greenness, and brightness, to be a significant oversight. Spectral vegetation indices have seen extensive use in

remote sensing of terrestrial vegetation (Bannari et al. 1995), including disturbance ecology. For example, NBR is a common basis for disturbance severity mapping (e.g., Key and Benson 2006; Miller and Thode 2007). The signal-to-noise ratio for disturbance mapping in North America tends to be greater for spectral vegetation indices compared to Landsat spectral bands (Cohen et al. 2018). Furthermore, vegetation indices have proven useful in forest biomass mapping (e.g., Foody et al 2003) and forest regrowth monitoring (e.g., Schroeder et al. 2007). I understand that these metric need not form the basis for the current study, but recognizing their application elsewhere and their potential to contribute to future research would be valuable. In particular, some discussion of common spectral vegetation indices that include NIR and/or SWIR1 in the context of the observed sensitivity to regrowth would be valuable.

Response: Vegetation indexes (VI) are based on band combinations before and during disturbance, and forest regrowth. Yet, it is important to understand the band behavior for those conditions (Tucker, 1979). To address this reviewer concern, we have included the following in the revised manuscript:

[Section 4. At the end of last paragraph] In lieu of this development, we show that with successional aging, modeled forest structure returns to pre-disturbed values (through canopy closure) with similar recovery time as NIR, which can be compared against remote sensing metrics like vegetation indices. Nevertheless, the extent to which different vegetation indices (e.g., Normalized Difference Vegetation Index (Rouse et al., 1973), Enhanced Vegetation Index (Huete et al., 2002)) represent the gradient from pre-disturbance through recovery, remains an important area of study.

2. In lines 130-132, it is stated that the burned sites were clearcut, then burned, then maintained as pasture for a few years before forest regrowth began. I am concerned with referring to cut and then burned areas simply as “burned sites”. Fire as a mortality/ disturbance agent has a different impact than fire used as post-disturbance vegetation management. Fire as a disturbance agent within forests will produce various levels of fire severity (e.g., Alves et al. 2018), and thus tree mortality, whereas postdisturbance fire is likely used to benefit forage species and remove woody vegetation, not kill trees in the forest canopy. The latter point is implied by the use of these sites as pasture for several years. It would be more appropriate to refer to these as “cut+burn” or “cut/burn” or something like that. The fire and/or grazing seems to have impacted species composition of the regrowing forest, but attributing the dynamics observed solely to fire seems inappropriate. Furthermore, since no burn simulations were used, is there any reason to have the burned sites without referring to the harvesting history as well? It is likely more appropriate and more interesting to reframe them as cut and burned, which is what they are based on the text. Then, the authors have two types of harvesting, which perhaps provides a more robust assessment of the ELM-FATES model and the complexity of management activities that should be incorporated in the modeling. Even with these concerns, I found the paper to be an interesting attempt to leverage remote sensing in the testing of ecosystem models. Similar applications could lay the groundwork for new developments in Earth system modeling, especially in regions that are traditionally data poor, such as the Amazon.

Response. In the submitted manuscript we defined cleared and burned areas as 'burning areas' for simplicity (line 102). However, for clarity, and in agreement with the reviewer, we will use the whole name: cleared and burned (clear + burn) in the revised manuscript. In our study we emphasized that different disturbances produce different pathways of regrowth that NIR was able to capture.

Specific and Technical Comments

Line 16: Enumerate which sensors (TM and ETM+) or missions (Landsat 5 and 7) provided the data.

Response. We have included these details in the revised version on Line 16:

[Abstract, second sentence] We used chronosequence of Landsat (Landsat 5 Thematic Mapper and Landsat 7 Enhanced Thematic Mapper Plus) satellite imagery to determine the sensitivity of surface reflectance from all spectral bands ....

Line 42: Forest or successional pathways need to be defined early. Are the authors referring to succession of structural or compositional characteristics?

Response. In the Introduction section, second last sentence of the submitted manuscript we defined successional pathway.

Lines 114-115: Why the different areas? What distinguishes them? I see them defined in lines 211-213. Perhaps the figure caption could reference the distance to edge component of these areas?

Response. For clearcut and burned sites we used areas with different distances from the disturbance edge to determine whether distance is a factor affecting the regrowth. To address this reviewer question, we have included the following in the caption of Figure 1:

[Caption Figure 1] For the pathways of forest regrowth after clearcutting and burning sites we analyzed areas with different distances from the disturbance edge (A1, A2 and A3 in yellow).

Lines 159-162: I think that there may be a few missing words in here.

Response. Yes, we agree. We have revised these lines as follows:

[Section 2.2, first paragraph, second last sentence] We used LEDAPS since has a long time series of data, is promptly available, have high spectral performance comparable to Moderate Resolution Imaging Spectroradiometer (MODIS) (Claverie 160 et al., 2015) ~~and several datasets (Vuolo et al., 2015;Nazeer et al., 2014)~~ and it is suitable for ecological studies in the Amazon (van Doninck and Tuomisto, 2018;Valencia et al., 2016).

Lines 178-179: Was each pixel in the 3x3 pixel box treated as independent (n=9) or averaged (n=1)?

Response. We used the average of the 3x3 pixel box. We have included the following text to clarify this point:

[Section 2.2, third paragraph, second sentence] The spectral characteristics of old-growth forests and their changes after disturbances were investigated using several boxes of 3x3 pixels (Figure 1 b,c,d) as shown in Figures 3a-c. The average of each box was used in our analysis.

Line 179: Figure 3 is cited earlier in the text than figure 2. Consider swapping them or delete the reference to Figure 3 (which I don't think is necessary)

Response. We have remote the citation of Figure 3 in this line.

Lines 193-196: This paragraph seems out of place. As I understand it, the gap-filled data is used in the analysis of the forest regrowth timing described later (lines 213-221). It would be easier if the description of the gap-filling methods appeared just before the analysis of those data.

Response. The methodology of gap filling is very important in the manuscript and was applied to all our analysis of time series of remote sensing data. As suggested by the reviewer, we have placed this paragraph before the analysis of the data in the revised manuscript.

Lines 208-209: I was confused by this sentence. Should it read "The comparison : : : disturbances that was conducted was possible due: : :". Still, I don't agree fully with the sentiment that controlling for those know environmental gradients makes the analysis possible. Perhaps the authors mean that by controlling for these other factors, they make the assessment of forest successional pathways following disturbance more robust.

Response. To address this concern, we have made the following changes:

[Section 2.2, sixth paragraph, first sentence] A direct comparison of successional pathways of forest regrowth from studied disturbances was conducted that is feasible due to the similar conditions of climate, soils, and structure and composition of the old-growth forests.

Lines 209-211: Do the authors mean that greater magnitude wind disturbances tend to have longer recovery times?

Response: We meant that areas with higher disturbance take the longest time to recover.

Lines 254-256: Considering the fact that forest recovery is a major focus on this manuscript, I suggest adding some more detail. Specifically, how does the model represent distance from intact forest for regrowth, since distance within the harvested and harvested/burned areas was used in the assessment? Maybe distance from intact forest is not included in ELM-FATES and the authors are just trying to represent the variability in dynamics. The lack of clarity lead me to wonder how important the sampling design was for the overall study.

Response. We agree with the reviewer that we should provide more of an explanation in the model's representation of disturbed land and distance to intact forest, as this is important for forest recovery. The reviewer's intuition is correct, in that the distance from intact forests was not included in the ELM-FATES simulations used here. A surrounding fully matured forest did not exist around our idealized recovery plot. To help clarify disturbance and recovery dynamics we have added a description to the revised manuscript of seed supply and creation of naturally disturbed patches in ELM-FATES as the forest is recovering from a harvest or windthrow:

[Section 2.3, second paragraph, last sentences] Simulations of disturbance and subsequent vegetation regrowth ~~after disturbance~~ were initiated from this old-growth forest state. The model design used here only allows for simulating intact forests with natural disturbances (e.g., gap dynamics or windthrows) or harvested forests, but not both at the same time or in adjacent patches. Accounting for distance to intact forests was excluded due to the current limited understanding of seed dispersal mechanisms (i.e., spatial variability, dispersal limitation, etc.) in tropical forests (Terborgh et al., 2019). We use a more general form of seed production, such that the individual cohorts in ELM-FATES use a targeted fraction of net primary production (NPP) during the carbon allocation process (after accounting for tissue turnover and storage demands), which adds to the site-level seed pool for recruitment of new cohorts. Field data was not used to simulate or calibrate the modeled forest recovery post disturbance.

Line 270: I found Table 1 and Figure 2 to be quite useful in understanding the sensitivity/ uncertainty analysis proposed by the authors.

Response. We thank the reviewer for this comment.

Line 290: I assume NIR was selected because the results indicate that NIR is most sensitive, correct? If that is the case, perhaps rewording portions of this paragraph to state that the most sensitive spectral band was compared with the ELM-FATES output and save the identification of that band for the results section. At a minimum, stating that NIR is compared because it is most sensitive (referencing results below) is needed.

Response. We have added the following improvements to address this comment:

[Section 2.3, fourth paragraph, first sentence] In order to evaluate ELM-FATES performance during forest regrowth we compared NIR, the most sensitive band to regrowth (see results), with ELM-FATES outputs of aboveground biomass (AGB, Mg ha^{-1}), total stem density of trees ≥10 cm DBH (stems ha^{-1}), leaf area index (LAI, one-sided green leaf area per unit ground surface area, m^2 m^{-2}), and total crown area (m^2 m^{-2}) since these variables directly influence the surface reflectance …

Lines 344-346: To strengthen the connections between Table 2 and Figure 3, it might help to mark in some way the portion of each time series used to test the sensitivity of the metrics to regrowth.

Response. We have made the following changes to the revised manuscript to address this comment:

Table 2. Test of the significance for the slopes of the time series of six bands from L5 (LEDAPS SR Landsat 5) for the windthrow (period 1991-2011), clearcutting (period 1987-2011), and burning (period 1990-2011) cases in Central Amazonia shown in Figures 3d-f. The critical values (t0.975,8 and t0.975,12) for the *t* distribution were obtained from statistical tables. Bolt represents H1.

Figures 4-5: The colors and legend text don't always make sense in these figures. For example, in Figure 6 it looks like the color coding is mixed up for A1 and A2 as they switch colors each year for each time-series. Please check your symbology and the figure captions carefully.

Response. We noticed a few simple typos in the script plotting the figures. The typos were corrected and figures replotted. There are no changes to our results from these changes. For instance, new Figure 6.

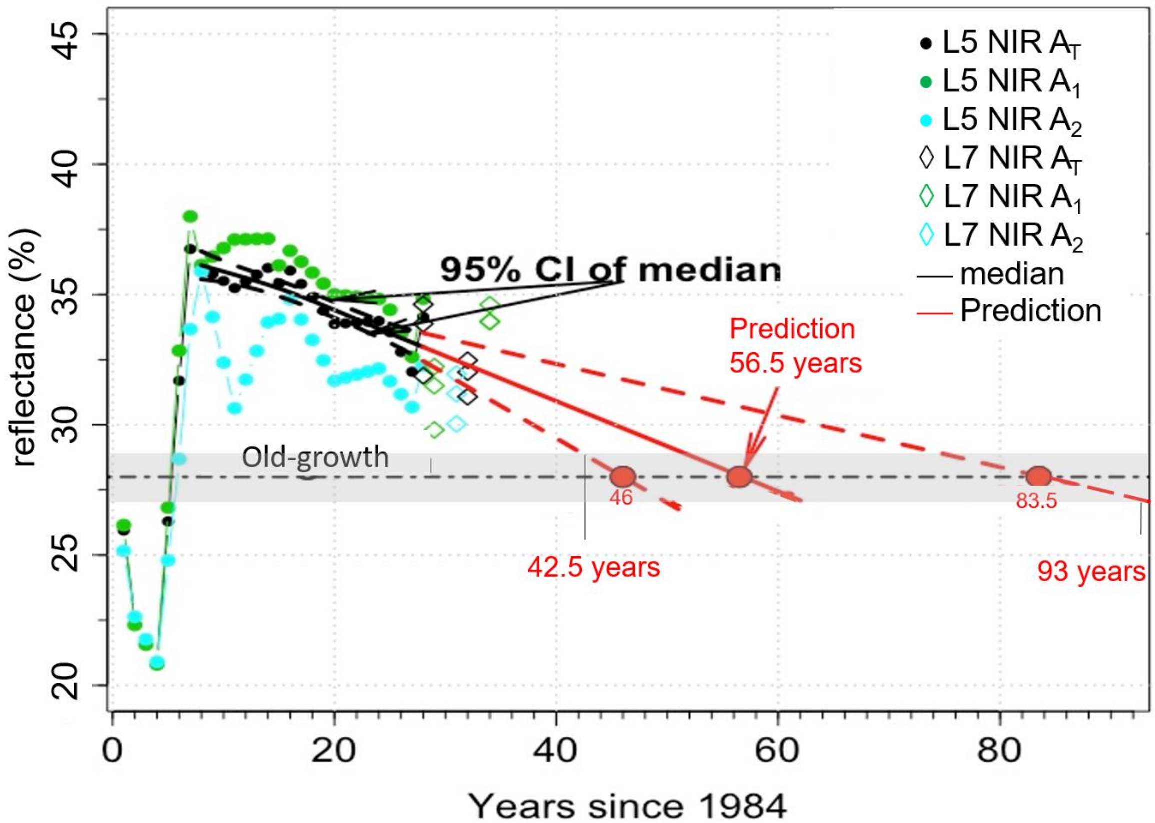

Figure 6: Changes in NIR for cut+burn site in areas A_1, A_2 and $A_T = A_1 + A_2$ showed in Figure 1) and prediction of NIR to pre-disturbance values (blue). The linear fit (solid liner) and the 95% CI (dashed line) are shown. Grey bar represents the control (old-growth forests) NIR of 28±1% and the black horizontal dashed line is 28%.

Lines 472-478: This paragraph starts off referring to regrowth, but it appears that the results being discussed are Figure 3a-c, which pertain to the initial disturbance effects. The regrowth is more complicated than that, and is really explored in later paragraphs. This makes me wonder whether it would be better to frame this portion of the discussion in terms of sensitivity of spectral bands for detecting short-term (0-5 year) effects rather than re-growth (6-25 years).

Response. This paragraph refers to the initial changes following the disturbance. To clarify, we have made the following changes:

[Section 4, first paragraph, first sentence] Our results show that Landsat reflectance observations were sensitive to the initial changes of vegetation ~~regrowth~~ following windthrows, clearcut, and burning, three common disturbances in the Amazon.

Line 522: Replace "Thought" with "Though"

Response. Replaced. Thanks.

Lines 555-558 (and elsewhere): By "higher disturbance", do the authors mean higher disturbance magnitude (i.e., tree mortality)? Also, is the comparison meant to highlight a difference between modeled mortality and actual mortality or some sort of comparison between the windthrow and the clearcut. This portion of the paragraph, which is attempting to explain differences between NIR and ELM-FATES results, was confusing to me.

Response. We agree with the reviewer that "higher disturbance" needs to be more clearly defined. By "higher disturbance" we are referring to either loss of biomass or more openness in the canopy coverage depending on the context of the sentence. We will clarify "higher disturbance" throughout the text. During this section of the discussion we are comparing between the windthrow and the clearcut. We modified these sentences in the revised manuscript:

[Section 4, sixth paragraph, fourth last sentence] ELM-FATES predicted ~~that~~ the timing of peak canopy-coverage was marginally sooner after windthrows compared to clearcuts, opposite to the NIR pattern. This discrepancy may be related to more biomass loss and open canopy coverage, followed by a lack of rapid colonization ~~the higher disturbance~~ in the modeled clearcut.

Line 557" Replace "that" with "than"

Response. Replaced.

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