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Interactive comment on "Landsat NIR band and ELM-FATES sensitivity to forest disturbances and regrowth in the Central Amazon" *by* Robinson I. Negrón-Juárez et al.

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.

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.

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 post-disturbance 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.

Specific and Technical Comments

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

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

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?

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

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

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)

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.

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.

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

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 stufy

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

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.

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.

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.

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).

Line 522: Replace "Thought" with "Though"

Lines 555-558 (and elsewhere): By "higher disturbance", do the authors mean higher disturbance magnitude (i.e., tree mortality)? Also, this 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.

Line 557" Replace "that" with "than"

Literature Cited

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