

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.

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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. Mathew Williams: mat.williams@ed.ac.uk Received and published: 3 February 2020

This is a very interesting and ambitious paper that links remote sensing and demographic modelling to understand forest disturbance and regrowth in the Amazon. The role of disturbance in forest biomass dynamics and C storage is an important area of research which is challenging to study due to the timescales involved. I think the paper is a valuable contribution but I have some queries about the approach and conclusions.

C1

1. The study is undertaken for one area of the Amazon – are the results (e.g. Fig 3) likely to be extensible across the Amazon, and to other equatorial forests?

Response: One of the main reasons in choosing the Central Amazon was due to the available data from windthrows regrowth (that we have published in several manuscripts) and the BDFFP research area, where research on forest regrowth after clearcut and clearcut+burning spans more than 30 years. As mentioned in our discussion “the predominance of *Cecropia*, after clearcut, and *Vismia*, after clearcut+burning, have also been found in the Western (Gorchov et al., 1993; Saldarriaga et al., 1986) and the Southern (Rocha et al., 2016) Amazon suggesting that our findings are applicable to other regions. However, an Amazon-wide study is beyond the scope of our work which is to explore the sensitivity of Landsat to most recurrent disturbance types in the Amazon. This is emphasized in our Discussion section, paragraph 5 (last two sentences).

2. There are challenges in using LandSat data for tracking forest disturbance and clearance in the Amazon, which lead to biases for smaller magnitude impacts, i.e degradation losses (Milodowksi et al. 2017). These biases are likely to impact the monitoring of forest recovery also. So I suggest extreme caution in interpreting the LandSat time series used here for sensing subtle phenomena like canopy closure and biomass growth. In the results, the statement “The similarity of spectral signatures for the control forests previous to the disturbances suggests comparable structure and species composition” may not be valid. One could equally well conclude that the sensitivity of NIRv is not enough to detect any differences that likely do exist between control old-growth forests. It would help if independent data could show the comparable structure and species composition of the old-growth sites to resolve this issue.

Response: In order to address these comments we have included (in red color) the following in our manuscript.

[Section 2.2, Paragraph 3, Sentence 5] Landsat is not sensitive to clusters of downed

C2

trees comprising fewer than 8 trees (Negrón-Juárez et al., 2011) or small disturbances (Milodowski et al., 2017). [Section 3.1, First paragraph, last sentence] The similarity of spectral signatures for the control forests previous to the disturbances suggests comparable structure. While Landsat spectral signature alone may not be sensitive to fine differences in species composition, previous research indicated a relatively high floristic similarity between the old-growth forests at our study sites (Negrón-Juárez et al., 2017; De Oliveira and Mori, 1999; Negrón-Juárez et al., 2018; Magnabosco Marra et al., 2018).

3. The abstract notes that “Statistical methods predict that NIR will return to pre-disturbance values in about 39 years (consistent with observational data of biomass regrowth following windthrows)”. I don’t find these observational data within the text. It would be very helpful to link the remote sensing directly to ecological time series, so we understand what the NIRv is responding to. I find it hard to understand what “regrowth to old-growth” means in table 3. I think more argumentation is needed to justify the conclusion that “NIR may be used as a proxy in modeling studies aimed at addressing forest regrowth after disturbances.” I suggest that more metrics are required to pinpoint ‘old-growth’ versus ‘disturbed’ status. Specific ecological metrics would include those that describe biomass stem size distribution, and 3D leaf area density distribution. LiDAR is an obvious candidate for providing such information.

Response: Based on these comments we have made the following changes: [Abstract] Statistical methods predict that NIR will return to pre-disturbance values in about 39 years, a value consistent with our previous observational study of biomass regrowth following windthrows. [Table 3, Title] Time of regrowth to old-growth forest characteristics (years). (In section 3.3 we have explained the term “Regrowth to old-growth”: the changes a forest undergoes while it grows from disturbance until it has canopy attributes of an old-growth forest).

[Section 4, paragraph 5] Due to the agreement in recovery timespans for observed NIR trajectories and regrowth, and being the best wavelength for inferring biomass, we sug-

C3

gest that NIR may be used as a proxy in modeling studies aimed at addressing forest regrowth after disturbances. Vegetation characteristics such as tree height, leaf distribution, etc. (currently obtained from Light Detection and Ranging - LiDAR) can improve understanding of regrowth (Almeida et al., 2019), provided the chronosequence of that data encompasses several decades.

4. It seems to me that the model simulated quicker LAI recovery and slower biomass recovery to steady state than the remote sensing. The transient response of the model in Fig 7a seems to show overshoot of biomass compared to the ‘old-growth’ baseline – so when is steady state achieved? LAI (fig 7c) seems to equilibrate (within old growth range) after 15-20 years, much shorter than the NIRv estimate of ~40 years. It would be useful to discuss how model transient behaviours can be validated against independent time series, and how robust the comparisons shown here are.

Response. The reviewer is correct that the biomass recovery predicted by the model does overshoot the ‘old-growth’ baseline of AGB. The recovery simulations in ELM-FATES were run for 100 years, and we found AGB reached an equilibrium point starting around simulation year 75 after both disturbances. The biomass equilibrated at 163 MgC ha⁻¹ +/- 1.0 after the clearcut and 163 MgC ha⁻¹ +/- 1.6 after the windthrow, so a little more variance resulted from the windthrow disturbance. We would like to emphasize that AGB is variable across the Central Amazon, and the model stabilizing at ~163 MgC ha⁻¹ is within the observed AGB range (150 MgC ha⁻¹). In section 2.3 of the manuscript, we state that the baseline simulation was spun-up for 400 years and until stable biomass was reached. It was an expected result that LAI would recover and equilibrate faster than NIR (Fig. 7c). We opted to use modeled “canopy-coverage” as a better model comparison to NIR. Canopy-coverage is defined on lines 342 – 343 as the average of crown area, stem density, and LAI since these three variables influence reflectance.

We have updated the text accordingly in Section 3.3:

C4

[Section 3.3, Paragraph 1, Sentence 3] Modeled biomass returned to modeled old-growth forest values quicker after windthrows (37 years, range 21 to 83 years) compared to clearcuts (42 years, range 27 to 80 years). Here the baseline old-growth forest is characterized as when biomass prior to the applied disturbances (ran for 400 years) reached an equilibrium, and with values similar to observed old-growth biomass (~ 150 MgC ha⁻¹). Interestingly, biomass accumulation from regrowth surpassed the baseline old-growth biomass (108 MgC ha⁻¹), reaching an equilibrium point around 75 years at ~163 MgC ha⁻¹, for both disturbances, and more similar to observed values. The rate of change of biomass regrowth over 50 years switched and was faster in the clearcut simulation (2.5 Mg ha⁻¹ yr⁻¹) than the windthrow simulations (2.0 Mg ha⁻¹ yr⁻¹), which is likely due to the near-zero initial biomass and proportionally greater contribution of fast-growing pioneer species.

5. For the evaluation of the FATES model it would help to have direct independent comparison to ecological data. Table 3 could be enhanced with observations for comparison against FATES. It's good to see some model-data comparison to data in fig 9, but how does this size distribution mis-match reflect on the modelling of recovery from disturbance?

Response: We have compared FATES to independent field data, and have clarified this point in the revised manuscript. In section 3.3 we compare observed biomass (150 MgC ha⁻¹) to the modeled baseline biomass (108 MgC ha⁻¹), and post-disturbance recovery biomass once stable (163 MgC ha⁻¹). In the same section we also compare stem density, stating that FATES simulates low stem density (~200 stems ha⁻¹) compared to measurements. Since this study is answering hypotheses about remote sensing capabilities over a tropical forest, we think that comparing the model to these measurements and remote sensing observations is adequate. In Section 2.3 we have added text to refer readers to the Holm et al. (2020) study that thoroughly analyzed demography sensitivity and compared a wide range FATES outputs (meteorological, forest attributes, carbon allocation, biomass accumulation) to field data in the Central

C5

Amazon, close to the BDFFP site used here.

We have included the following:

[Section 3.3, Paragraph 3, first sentence] The LAI of the modeled old-growth forest (4.0 m² m⁻²), prior to disturbances, was close to the observed LAI (4.7 m² m⁻²) measured near our site (Chambers et al., 2004). Due to disturbance, the initial modeled LAI (Figure 7c) and total crown area (Figure 7d) decreased, as expected.

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C6

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C7

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C8