

Reviewer Comments (RC) are presented in black italics.

Author Responses (AR) are presented in blue.

All line numbers cited in this response refer to the tracked-change manuscript file [Main_texts_tracked_change.pdf](#).

General Comments

RC: *The authors have significantly improved this paper. I think it is now close to being ready for publication and supplies both significant datasets representing forest disturbance and a methodology that could be applied to other AGB datasets. It represents a substantial body of work and the authors have attempted to be comprehensive in assessing the quality of the parameter inversion. My principal concern about the paper is that the authors do not give proper weight to the intrinsic properties of the L-band data and the way it's been processed, so there is some ambiguity in parts of the paper about whether an observed effect is really due to forest processes or instead due to the data processing, or some combination of the two.*

The writing tends to be over-wordy but providing all the instances of this word be onerous. Several examples are noted below but many more of extraneous words and phrase are not commented on.

The comments below are sequential through the paper and mix substantial points with linguistic ones.

AR: We sincerely thank the Reviewer for the positive assessment of our revised manuscript and for the constructive comments. We revised the relevant discussion to clarify how spatial patterns in EO-derived AGB may reflect both actual forest biomass organization and spatial dependence introduced by the retrieval and processing chain, including associated assumptions, constraints, and noise. We have also revised the manuscript throughout to reduce wordiness, remove unnecessary or potentially misleading expressions, and improve clarity. Detailed point-by-point responses to all comments are provided below.

Specific Comments

RC1: *L56, 59: above ground; aboveground? Should be Efforts*

AR1: Corrected at Line 63 and 66. "Aboveground biomass" is now used consistently.

RC2: *L.60-61 This is not a sentence*

AR2: Corrected at Lines 67. The revised text reads:

"The individual event-detection method applies a continuous change detection algorithm to the time series of satellite imagery to identify and map individual disturbance events..."

RC3: *L.74 equifinality is likely to be unfamiliar to many readers and needs to be defined. It's sort of indicated by the earlier part of the sentence but not properly connected to it. It's not clear to me that the use of this term helps here. The 1st 2 occurrences are related to its definition, the 3rd (l.128) talks about*

“this equifinality” which is unrelated to the previous text, and the 4th (L.401) really isn’t helpful in explaining what is observed.

AR3: Revised at all four occurrences. We now directly describe the underlying issue where appropriate and remove the term where it does not improve clarity. The revised text reads:

Line 100: *“This limitation to identifiability, or equifinality, is more likely to occur when using univariate, or single feature approaches, or when relying on coarse-resolution data.”*

Line 160: *“Inferring disturbance parameters from static biomass maps is challenging because different disturbance combinations can produce similar mean biomass outcomes (Williams et al., 2013). Our framework overcomes this by using second-order spatial texture features rather than solely first-order statistics.”*

Line 169: *“To distinguish among disturbance regimes, the framework characterizes each observed satellite biomass map using first-order distribution statistics, Shannon entropy, and second-order texture metrics derived from a gray-level co-occurrence matrix (GLCM).”*

At the final occurrence, Line 604 *“leading to equifinality”* was replaced with *“reducing parameter identifiability.”*

RC4: 76 The reference should be given as Le Toan in the references (Le is a name, not the French le)

AR4: Corrected at Line 104 and in the reference list.

RC5: 129 GLCM not defined

AR5: Corrected at Line 170. The revised text reads:

“To distinguish among disturbance regimes, the framework characterizes each observed satellite biomass map using first-order distribution statistics, Shannon entropy, and second-order texture metrics derived from a gray-level co-occurrence matrix (GLCM).”

RC6: 130 why “direct”? As in many places, the text uses an unnecessary and sometimes misleading adjective.

AR6: Revised at Line 170 by removing “direct”. The revised text reads:

“These features are complementary in characterizing the distribution and the spatial patterns in the biomass maps.”

RC7: 131-132: This statement about the implications of high spatial variance is stated as fact rather than as a possible interpretation

AR7: Revised at Lines 173 to present this as a possible interpretation. The revised text reads:

“Conversely, high spatial variance or GLCM contrast may be consistent with a multi-aged, structurally complex forest shaped by frequent, small-scale gap dynamics.”

RC8: 139 What does structurally consistent mean?

AR8: Revised at Line 205 by removing “structurally.” The revised text reads:

“...the European Space Agency Climate Change Initiative (ESA CCI) Land Cover dataset (ESA, 2017), to provide a temporally dynamic forest mask.”

RC9: 145 At 25 m the GlobBiomass data are very noisy and it is misleading to imply that structure can be inferred at this scale; noise would dominate many of the measures proposed. As the paper shows, the actual resolution used is significantly coarser than 25 m.

AR9: Revised at Line 215 to distinguish the native input resolution from the effective resolution used for feature extraction. The revised text reads:

“The native 25 m map served as the input to our processing workflow; however, the spatial features used for the final inversion were calculated only after 10×10 block-mean aggregation to an effective resolution of approximately 250 m, as described in Section 2.2. This aggregation reduces the influence of pixel-scale retrieval noise and aligns the feature distributions of the observed and simulated biomass maps.”

RC10: 153 and many other places: The authors unhelpfully use the words tile, landscape, landscape tile to mean the same thing right through the paper. It is based on 25 km tiles; why not use this term consistently. By all means say somewhere that this scale is meant to be representative of a landscape but avoid using multiple terms for the same thing (particularly as in some places it’s not clear what is meant)

AR10: Revised at Line 220 and throughout the manuscript. We now use “tile” for the 25×25 km analysis unit and reserve “landscape” for its ecological interpretation. The revised text reads:

“We first established a global grid of non-overlapping, true-to-area 25×25 km tiles, each representing a landscape-scale analysis unit.”

RC11: 164 why on earth call it an informative feature (whatever that means) rather than Shannon entropy?? (and in Table 1)

AR11: Revised at Line 230 “Informative feature” was replaced with “Shannon entropy,”

RC12: 165 why does using the 4 adjacent pixels ensure “ecological integrity”? As in many other examples, this is a use of a phrase that adds nothing and is opaque in meaning.

AR12: Revised at Lines 232 and the revised text reads:

“To avoid texture artifacts at forest-mask boundaries, the GLCM was constructed exclusively from

adjacent pairs of valid forest pixels in four directions. Pixel pairs involving non-forest or missing pixels were excluded.”

RC13: 170. *If there are 106 pixels, as stated above, then 100 pixels is 0.01% of the landscape (whatever the landscape is; is it the same as the pixel grid refer to above?)*

AR13: Revised at Line 236 to clarify that the threshold refers to the aggregated grid. The revised text reads:

“For the final feature extraction at the selected aggregation scale, where each 25×25 km tile comprised a 100×100 pixel grid, GLCM texture features were computed only for tiles containing at least 100 valid forest pixels, corresponding to 1% of the aggregated grid.”

RC14: 179: *why is strictly written here?*

AR14: Revised at Line 283 by removing “strictly.” The revised text reads:

“This standardized resampling protocol improves the comparability of the derived spatial distribution and GLCM texture statistics across EO biomass products with different native resolutions and temporal configurations.”

RC15: Table 1 *Many of these features will be highly correlated; which are really significant?*

AR15: Indeed, some correlations are observed between features. In Wang et al (2024) we analyzed the contribution of the full feature set to DRP prediction using Random Forest feature importance. Rather than selecting a subset solely based on pairwise correlations, we retained the complete feature set because the features characterize complementary aspects of biomass distributions and spatial texture, and their contributions differ among the DRPs. Specifically, μ was mainly informed by biomass-distribution statistics and GLCM correlation, α by texture features, and β predominantly by the coefficient of variation, with GPP also contributing.

RC16: 212 *What are these spatial gradients? Nothing above is about gradients.*

AR16: Revised at Line 325 by replacing “spatial gradients” with “spatial patterns.” The revised text reads:

“In order to quantify how much a GPP estimate based on a single year could change the spatial patterns of the different DRPs, a comparison between the DRPs from the 2001-2010 mean is contrasted with a prediction based on the GPP of 2010”

RC17: 231 *CV is not ACF-related; it is a 1st order statistic*

AR17: Corrected at Line 365.

RC18: 250 What's WOR?

AR18: Defined at its first occurrence at Line 373. The revised text reads:

"...We quantified this divergence using the weighted overlap ratio (WOR; see details in Section S2.2 in the supplement)."

RC19: Section 2.2 The authors treat the spatial correlation in the EO product as a property of the real world but do not consider the fact that the EO AGB inversion algorithm is likely to generate spatial correlation. This needs a more careful description and discussion

AR19: We agree that the previous text attributed the strong spatial correlation in the EO AGB product too directly to real-world ecological processes. We have added texts in Section 2.2 to clarify that the observed spatial correlation may arise from a combination of actual forest biomass organization and spatial dependence introduced by the AGB retrieval and processing chain, including spatial filtering and resampling.

RC20: Figure 3 caption. As I said in my 1st report, the 25 x 25 km tiles are typically comparable to or bigger than the 0.25° grid so how can they be nested within the grid cells?

AR20: We apologize for the typo in the Figure 3 caption. "0.25° × 0.25°" has been corrected to "2.5° × 2.5°" at Line 466. The revised text reads:

"The inset in each panel provides a magnified view of a 2.5° × 2.5° region, illustrating the spatial arrangement of multiple 25 × 25 km tiles."

RC21: 335 does this mean that any tile that touches a grid cell contributes to the overall average, rather than in an area-weighted sense, for example? This does not seem right.

AR21: We recalculated the gridded product using the area-weighted mean, with each overlapping tile weighted by its area of overlap with the corresponding 0.25° grid cell. The recalculated results show no substantial differences in the overall spatial patterns compared with the previous version, as shown below. We have accordingly replaced Figure 4 and updated the gridded dataset.

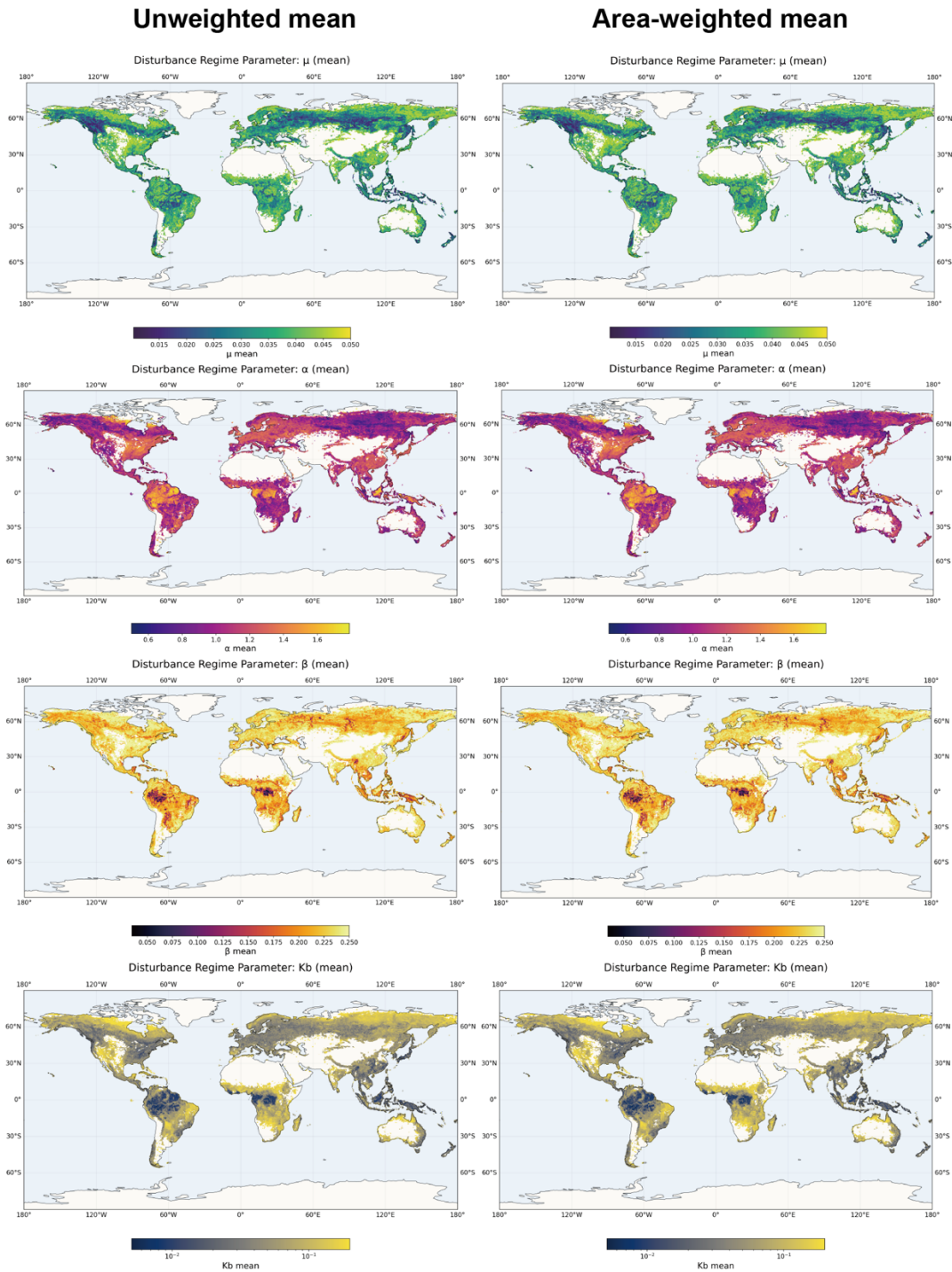


Figure R1. Comparison of disturbance regime parameters derived using unweighted and area-weighted aggregation. For each 0.25° grid cell, the spherical overlap area with every intersecting 25×25 km tile was calculated and normalized to provide the weights used to average tile-level predictions; the revised results show only minor local differences and no substantial change in the overall spatial patterns.

RC22: 370 delete anomaly

AR22: Corrected.

RC23: 375 “uncertainty” after parameter

AR23: Corrected.

RC24: 441 insert “sites” before are

AR24: Corrected.

RC25: 443 replace dimensional by order, insert “there is” before “a”

AR25: Corrected.

RC26: 452 centrally

AR26: Corrected at Line 695. The revised text reads:

“In contrast, the parameter α is a unimodal distribution, approximately symmetric, with a global mean value of 1.25.”

RC27: 455 these are not high-dimensional stats, just normal 2nd order ones

AR27: Corrected at Line 700. The revised text reads:

“Furthermore, it highlighted the value of using richer feature sets, including second-order spatial statistics, to derive these parameters, as illustrated by the low- and high- β landscapes.”

RC28: Around 480: this relates to my earlier point. What the authors have actually done is ensure their spatial correlation properties are similar to those seen in the EO-base dataset, which is likely to be a combination of true AGB correlation on the ground and correlation induced by the AGB inversion algorithm. There is no information in the paper indicating how to partition the correlation between these two.

AR28: Revised at Lines 737 to clarify that the spatial patterns in the EO AGB product may reflect both true forest structure and spatial dependence introduced by retrieval and processing, which cannot be separated using the available data. The revised text reads:

“Consequently, our framework focuses on characterizing the spatial attributes represented in the EO AGB product, namely the size, form, and distribution of mapped biomass loss, rather than simulating the mechanistic temporal spread of disturbance processes (i.e., spatial contagion). These mapped spatial attributes may reflect both the true spatial organization of forest AGB, but also a spatial dependence in

the dataset introduced by the AGB retrieval and processing chain, which cannot be partitioned using the available data.”

RC29: 485 in what sense are they strict or specific? Are the authors saying that the products intrinsically exhibit correlation because of the inversion algorithms? Or are these just irrelevant meaningless words?

AR29: Revised at Line 744 by removing “strict” and “specific.” The revised text reads:

“While the synthetic grids have no predefined physical resolution, EO biomass products are mapped at defined native spatial resolutions.”

RC30: 487 Despite the last sentence in this paragraph, and as stated in their own text (section 2.2), the lack of spatial correlation in the original dataset IS a model deficiency which is “corrected” (in the sense of matched to the specific correlation properties of the observational dataset) by spatial averaging

AR30: We agree that the lack of spatial dependence in the synthetic disturbance generator represents a model simplification. We clarify that spatial aggregation mitigates this mismatch by aligning the spatial statistical properties of the simulations more closely with those of the EO product but does not mechanistically reproduce disturbance spread. The revised text at Line 745 reads:

“To bridge this fundamental scale mismatch and align the discrete simulated events with the continuous patterns of satellite data, spatial aggregation is utilized as a necessary scaling step to anchor the spatial statistics to the observational scale, aligning the spatial statistical properties of the simulations more closely with those of the EO product.”

RC31: 550 et seq. There are any number of studies showing that L-band data lose sensitivity and saturate as AGB increases, it is not “apparent”. If the GlobBiomass AGB estimates do not saturate it must therefore be that the information to get past the saturation limit has to come from somewhere else.

AR31: Our use of the term “*apparent saturation*” referred specifically to the flattening or negative bias observed in comparisons between mapped and reference AGB, which is a property of the complete biomass retrieval system and cannot necessarily be attributed to SAR signal saturation alone. We make this clear in the next sentence. To avoid ambiguities, we adjust the sentence and remove “*resulting on an apparent saturation effect*”.

RC32: 552 resulting in (not on)

AR32: Corrected.

RC33: 554 associated with (not to)

AR33: Corrected.

RC34: 555 delete “to”

AR34: Corrected.

RC35: 565 *What is rigorous about the process? Is this landscape sample a tile?*

AR35: Revised at Line 851 by removing “rigorous” and clarifying that the samples are 25 × 25 km forest tiles. The revised text reads:

“For the sensitivity analysis, we randomly selected 300 high-biomass 25 × 25 km forest tiles across major biomes globally (forest cover > 50% and AGB > 220 Mg ha⁻¹) to capture a continuous macro-ecological gradient.”

RC36: 569 et seq. *Why a gradient etc? Why not “we considered a range of”? k and S not defined*

AR36: Revised at Lines 856 by referring to a range of thresholds and defining the parameters k and S. The revised text reads:

“We considered a range of high-biomass thresholds ($T \in \{150, 200, 250, 300\}$ Mg ha⁻¹). We further applied two transformations to AGB values above each threshold: (1) a linear transformation controlled by the scaling parameter k ($k \in [0.1, 1.5]$); and (2) an asymptotic inverse-log transformation controlled by the scale parameter S ($S \in [300, 2000]$ Mg ha⁻¹), with smaller S values producing stronger increases in AGB.”

RC37: 586. *these Figs don’t exist*

AR37: This resulted from a mismatch between different versions of the Supplementary Information. The revised manuscript now cites the latest Supplementary Information, in which the figures are numbered consistently from Figure S1 to Figure S15.

RC38: 587-8: *What it does support is that the AGB estimates are very noisy and this noise can produce spurious high values of AGB under the algorithm used.*

AR38: Revised at Lines 896 to clarify that the broad upper tail does not necessarily represent true high-biomass estimates and may partly result from retrieval-model assumptions, constraints, or noise. The text now emphasizes that these values should therefore be interpreted with caution.

“The large fraction of mapped AGB values above the selected high-biomass thresholds in both tropical and extratropical regions (Figure S12) reflect the presence of a broad upper tail, indicating that the biomass product does not impose a universal hard truncation at these values. However, it is not clear to which extent these distributions reflect true high biomass estimates from variations cause by the retrieval-model assumptions, constraints or noise, and should therefore be interpreted with caution.”

RC39: 600 et seq. *The language used in this section is not ideal. The fact is that L-band has reduced sensitivity to AGB as AGB increases, which inevitably reduces variability and is likely to make much of the variability due to noise. The underlying premise in this paragraph is that if we didn’t have this sensitivity and*

the real forest behaves differently, we would get different disturbance parameters except for gap size. Is it really true that this is only the case for extreme and unlikely cases (so can effectively be ignored), or instead that it is an intrinsic limitation of the inferences from this dataset?

AR39: We rewrote this paragraph to clarify that the sensitivity of DRPs represents an intrinsic limitation of the framework. The revised text at Lines 915 reads:

“Overall, these experiments illustrate how the different DRPs respond to compression of the upper end of the biomass distribution. The stability of α across all scenarios indicates that the inferred spatial clustering frequency of disturbances is comparatively robust to upper-tail dampening. In contrast, μ , β , and K_b are sensitive to reductions in spatial variance and to underestimation in high-biomass forests. This sensitivity represents an intrinsic limitation of the framework because suppressed biomass variability, together with retrieval noise, can propagate into the inferred disturbance parameters. As such, the magnitude of the resulting deviations increases with both the fraction of affected pixels and the strength of the imposed reconstruction. Despite differences in spatial resolution and temporal configuration, the spatial harmonization protocols produced consistent sensitivity bounds across all evaluated products. To further overcome the dampened retrievals in high-biomass forests, the emergence of data streams from novel missions, such as the P-band BIOMASS, may play a significant role in diagnosing global disturbance regime parameters.”

RC40: 624. What does “metrics” refer to?

AR40: Revised at Line 960 by replacing “metrics” with “uncertainty measures.” The revised text reads:

“Together, these uncertainty measures indicate reliable predictions across approximately 90% of global forest regions.”

RC41: 630 What is meant by boundary conditions? Is this inherent limitations?

AR41: Revised at Line 966 by replacing “boundary conditions” with “challenges.” The revised text reads:

“Nevertheless, several challenges remain, including the reduced sensitivity of spaceborne radar signals to local spatial variance in dense forests, the model assumption of pixel independence that omits explicit transient contagion dynamics, and the current lack of extensive, independent global ground-truth validation.”