

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.

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

L.60-61 This is not a sentence

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.

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

129 GLCM not defined

130 why "direct"? As in many places, the text uses an unnecessary and sometimes misleading adjective.

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

139 What does structurally consistent mean?

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.

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)

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

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.

170. If there are 10^6 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?)

179: why is strictly written here?

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

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

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

250 What’s WOR?

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

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?

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.

370 delete anomaly

375 “uncertainty” after parameter

441 insert “sites” before are

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

452 centrally

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

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.

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?

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

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.

552 resulting in (not on)

554 associated with (not to)

555 delete “to”

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

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

586. these Figs don’t exist

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.

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?

624. What does “metrics” refer to?

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