

Interactive comment on “Mapping Congo Basin forest types from 300 m and 1 km multi-sensor time series for carbon stocks and forest areas estimation” by A. Verhegghen et al.

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The paper presents a land cover map of forest types for the Congo basin from MERIS and SPOT-VGT. From the map the authors derive a map of carbon stocks. Phenological curves for the land cover types are also presented. This manuscript addresses highly relevant scientific questions within the scope of the journal. Its classification approach through regional and seasonal stratification can be considered novel. The maps are of high relevance to the REDD+ initiative and the global change community.

The scientific methods and assumptions are generally valid. I would however raise the issue that the imagery is classified by a k-means algorithm before the classes are

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then assigned labels by an interpreter. k-means is a purely data-driven unsupervised algorithm. Its outcome is a map of classes that best separate the multi-dimensional signature space. In reality this means that the k-means algorithm describes a given dataset better than a supervised classification, which forces class labels to the data that may not be separable very well. The downside of the k-means algorithm is that the classes it comes up with are merely clusters of data points in hyperspace. They may not have a clearly defined meaning to a human interpreter and can consist of mixed variants of different land cover sub-types. How did the authors deal with this issue and how confident are they that the k-means algorithm identified meaningful land cover classes? The spatial stratification that the method adopts has the advantage that it allows the compositing of images from different years with similar seasonal phenological stages. A question I would raise though is what effect this stratification has on the boundaries between different strata. The authors should show a zoom image of such boundary areas to convince the reader that they are not creating artificial 'crisp' land cover differences between neighbouring pixels purely because of the different strata.

The validation was done with 100 points. For 20 classes this is a very limited number of points. As a consequence of this, the confusion matrix or correspondence matrix (the authors call it 'contingency matrix', which is not the best term) contains several classes for which the producers accuracy is 0. This is because only one point of that class was in the 100 sample points and this did not correspond to the image classification. Ideally, a larger number of points should be collected, perhaps by using high-resolution aerial photographs or GoogleEarth type imagery as surrogate ground data.

In addition, the kappa coefficient should be stated, it compensates for expected chance agreement and is more meaningful than percentage accuracy.

The results of the paper are generally sufficiently justified to support the conclusions.

The uncertainty of the carbon map is presumably largely caused by the scarcity of data on carbon stocks of different forest types. Can the authors write something about the

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uncertainty in that assumption? The classified map has an accuracy figure, but the average carbon densities are just taken at face value.

The title clearly reflects the contents of the paper. The abstract provides a good summary, but it should mention the carbon stock results.

The overall structure of the paper is clear. The role of the phenology curves could be better explained. What are the main conclusions from that work and how does it fit in? The number and quality of references given is very good and I am not aware of any important omissions.

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