Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2019-28-AC2, 2019

Biogeosciences Discussions
Open Access
EGU

Interactive comment on "Mapping trends in woody cover throughout Namibian savanna with MODIS seasonal phenological metrics and field inventory data" *by* Vladimir R. Wingate et al.

Vladimir R. Wingate et al.

vladimir.wingate@unibas.ch

Received and published: 9 May 2019

Interactive comment on "Mapping trends in woody cover throughout Namibian savanna with MODIS seasonal phenological metrics and field inventory data" by Vladimir R. Wingate et al.

Anonymous Referee #2

Received and published: 24 April 2019 Review of "Mapping trends in woody cover throughout Namibian savanna with MODIS seasonal phenological metrics and field inventory data" The study aims to map, describe and explain trends in woody vegeta-

Printer-friendly version

Discussion paper

tion cover in Namibia. In the current shape of the manuscript, I cannot recommend a publication in Biogeosciences.

The methods and results are not described/presented in a way that allows to understand the results. The methods need to provide more details about how phenological metrics are defined and how they were computed. In addition, the description of random forest setup need to be substantially improved.

The authors have now substantially altered the methods and results as per the reviews recommendations.

Specifically, 1) sentences which describe how phenological metrics are defined and computed have now been made clearer; 2) a description of the random forest models and how they were used has been updated, and 3) the structure of the methods has been altered as proposed by the reviewer (please see below for details).

From the Methods section, I assumed that one random forest model was trained for all field observations but then on page 13 line 4, a “2007 model” is stated. So did you train random forest models for each year separately? In addition, it is not clear which predictors were included in the random forest models.

Random forest models were trained for each year using one single set of field data, and the five corresponding phenological metrics values for each year (2001-2016). This point has now been clarified as per the reviewer’s suggestions.

L19P11 now reads: “Models were created by taking plot measurements of percent woody cover, with the coincident pixel values of each of the five metrics (Table 1), for every year (2001-2016)”.

Table 1 lists DSI, MeanDS, MaxWS and DSINT but then other predictors (partly year-specific) are mentioned in Figure 3. Please replace Table 3 with an overview Table about all random forest models that were trained and which predictors were included in each model.

Table 1 includes a description of each of the phenological metrics used.

All predictors where used in training the models.

L18P11 now reads: “Models were created by taking plot measurements of percent woody cover, with the coincident pixel values of each of the five metrics (Table 1), for every year (2001-2016).”

Rather than include a large table with all the individual predictor layers, we now indicate that the year is appended to the abbreviated form.

Table 1 now reads: “Table 1. Phenological metrics used in this study, their abbreviation and concise description. Each metric was computed for every years of the study (2001-2016); the resulting short form is then labelled as “DSI2016”, where the year is appended to the short form.”

I find it also confusing that different results for different years are shown. For example, Figure 3 shows the importance of predictors for 2008 but Figure 4 shows the random forest fits (? - axis labels are missing!) for 2016. Is there any reason why you selected these different years or why you are not showing results for all years?

Only the most recent model outputs are now included. The remaining model output figure are included in the supplementary material, since including them all would take too much place, and results in a very condensed figure. Instead, the text describing these results is included.

L6P14 now reads: “Predictor variable importance for the most recent output (2016) is plotted in (Figure 3). Plots for the remaining years (2001-2015) are provided in the supplementary material (Figure S1).”

Axis labels have now been included in Figure 4, and a 95% confidence interval has been added.

Relation to previous studies: The authors refer to Brandt et al. (2017) and Song et al.

(2018). These two studies show for Namibia an increase in woody vegetation cover and in short vegetation, respectively, which is contrary to the results of this study. A more direct comparison and discussion of these results is necessary.

We directly compare and evaluate the results from Song et al. (2018) for each biome in this study: Section 4.1.1, L15P23, as per the reviews recommendation. L15P23, now reads: "When evaluating the results from Song et al. (2018) for Namibia only, an overall greening trend from 1982 to 2016 can be identified. On average for each FAO biome, a decrease in bare ground and a simultaneous increase and short vegetation can be noted, while a gain in tree canopy is seen across the tropical dry forest biome."

Both Brandt 2017 and Song et al. 2018 use different datasets to evaluate changes in vegetation cover: specifically, these datasets a of a longer temporal resolution, and coarser spatial resolution, as well as different spectral resoltions. These specifics are now discussed in detail in Results Section 4.1: In addition, the results of Song et al. 2018 and Brandt et al. 2017 are compared to our results in the Results Section 4.1.

Specifically, with Reference to Brandt et al. (2017): Since the results from Brandt et al. (2017) show only a greening trend across Namibia, this study only attempts to discuss the potential causes of the differing results, and goes on to describe how they are similar across biomes.

Lastly, a sentence describing the different datasets used by Brandt et al. (2017) has been appended:

L7P24 now reads: "However, in contrast to the results of this study, they find a greening trend predominating across Namibia; this discrepancy can most likely be explained by the different temporal, spatial and spectral resolutions of the datasets used. Here, Brandt et al. (2017) employed the 0.25° spatial resolution 1992-2011 vegetation optical density dataset, derived from satellite passive microwave measurements."

Specific comments

Section 2 “Material and methods”: This section misses currently a logical structure because it jumps back and forth between data description, methods, different types of datasets etc. I suggest revising the structure as following:

The current section 2.2 “Study region” (including Fig. 1) can be easily merged with the section 1.2 of the introduction. Then the new structure could be: Section 2.2 has now been merged with Section 1.2, and is now entitled Section 1.3 study region. 2.1 Method overview (= 2.1 Approach + reference to Figure 2)

Section 2.1 is now equal to 2.1 Approach, and a reference to Figure 2 has been included.

2.2 Datasets 2.2.1 Field data (= 2.4 + 2.5)

2.2.1 Satellite and ancillary data (= 2.3 + 2.6 + 2.7) The structure of the methods has now been adjusted as per the reviewers recommendations: 2.3 Estimation of phenological metrics - includes smoothing filter description from 2.3 and a substantially improved description of the phenological metrics (2.8) and how their were estimated)

Smoothing filter description was carried out as part of the initial MODIS processing, as per standard protocol (please see “TIMESATâĂŤa program for analyzing time-series of satellite sensor data”); hence, the authors have decided to leave the smoothing paragraph in its current section.

A description of the phenological metrics has now been enhanced as per the reviewer’s suggestions (Section 2.3).

Specifically, this section clarifies that these metrics were computed annually.

For a complete description of how certain of the metrics were computed, namely, the DSI, DSINT, and SINT, the authors continue refer the reviewer and readers to the relevant literature, in order to keep the length of the text to a minimum. For the remaining metrics, their estimation is included on the text.

2.4 Estimation of woody vegetation cover (= 2.8 without the description of phenological metrics + 2.9 + 2.11)

These sections have now been altered as per the reviewers suggestions.

2.5 Trend analysis Sections 3 and 4: Both section have very similar sub-sections (e.g. 3.3. and 4.1 Trends in relation to biomes). Hence, the entire text is very lengthy and repetitive. I suggest to combine sections 3 and 4 into "Results and discussions". Please also assess if there are four different sub-section on "Trends in relation to : : :" needed.

This manuscript had originally been in the "Results and Discussion" format, however, an earlier reviewer suggested that this be changed to two distinct Results and Discussion headings. As a consequence, the authors have decided to keep the present format, in light of the effort which went into separating these headings previously.

In addition, to make the text more comprehensible, a single section "4.1 Trend evaluation", now includes sections 4.1.1 Biomes, 4.1.2 Land-use and population, 4.1.3 Multi-temporal imagery assessment, and 4.1.4 Precipitation. These have been renamed so as to remove any repetition.

Finally, Results Section 3.3 has been renamed to "3.3 Trend evaluation in relation to biomes, land-use and population", in order to correspond more closely with heading 4.1

P 1 L 20-25: Please indicate for which periods trends were computed.

L23P1: The time period has now been included (2001-2016)

This section now reads: "An overall decrease in woody cover was identified over the period from 2001-2016, with the most pronounced decreases found in urban and densely populated areas."

P 9 L 1-5: Please clarify if the average percentage woody cover is representative for the variability in a field plot.

Printer-friendly version

Discussion paper

A single value of percentage woody cover was derived per plot using the methods proposed by Herrick et al., 2013. The within field plot variability was not available for this study, only percentage woody cover per plot.

P8L17 now reads: “Here, the woody cover per plot represents the % of points covered by either trees or shrubs or both, where each site/plot had 100-160 points.”

In addition, P9L12 reads: “In this study, the field plots were not scaled to the resolution of MODIS (250 $\times$ 250 m) using spatial averaging; instead, instead, the percentage woody cover computed using the methods proposed by Herrick et al. (2013) within the 50 $\times$ 50 m field plot was compared with the corresponding MODIS pixel values”

P 9 section 2.8: This section describes both the random forest modelling approach and the phenological metrics. I suggest to split these into two sections because the description of phenological metrics is currently not understandable.

This section has now been split as per the reviewer’s suggestion and are now refered to as Sections 2.4, 2.4.1, 2.4.2).

P 9 L 30 – P 10 L 3: I do not understand what you are trying to say here. Please revise the paragraph.

This paragraph has been revised and shortened and now reads: “

L15P11: “The Random Forest algorithm was selected since it is effective at estimating predictor variable importance, integrating multiple predictors variables with different predictive power, and not assuming normal statistical data distribution or any particular relation (i.e. exponential) between dependent and independent variables. Hence it has been used extensively in remote sensing studies to integrate a range of imagery and metrics (Breiman 2001; Moisen and Frescino 2002; Cutler et al. 2007; Prasad, Iverson, and Liaw 2006; V. Wingate et al. 2016).”

P 13 L 3-4: I would rather state first which variables are the most important ones. All figures: Please remove the black background in all figures. This is a waste of ink if

somebody one to print the paper.

A statement of which variables are the most important has now been included as suggested by the reviewer:

L3P14: This section now reads: “The evaluation of predictor variable importance yielded a clear pattern: the most important predictors were most often (but not in all cases) the mean dry season values (MeanDS), dry season index (DSI) and dry season integral (DSINT) (in variable order). The least important predictors were consistently the maximum annual wet season value (MaxWS) and annual small seasonal integral (SINT) (expect for the 2007 model, in which the DSINT is the weakest, potentially implying an anomalous year).”

Black background has now been removed from all figures as per the reviewer recommendation.

Fig. 2: It is confusing that the legend ranges from +0.8 to -0.2; please reverse. The inset map of Africa hides parts of the data; please change.

Figure 2: The legend has now been modified as per the reviewer recommendation: High NDVI values are shown in dark green, while low and negative values shown in beige. In addition, the inset map has been modified as per the reviewers suggestion.

Fig. 3: The names of the predictors do not correspond to the abbreviations listed in Table 1. Make sure to list the correct names of ALL predictor variables in Table 1.

A sentence clarifying that the phenological metrics (predictor variables) were computed for each year has now been included in the main text and within the table caption. Table 1 now reads: “Table 1. Phenological metrics used in this study, their abbreviation and concise description. Each metric was computed for every years of the study (2001-2016); the resulting short form is then labelled as “DSI2016”, where the year is appended to the short form.”

L21P10 now reads: “Phenology metrics for each year (2001-2016) were extracted us-

ing TIMESAT software, which has been extensively used for measuring seasonal land surface phenology in drylands"

Fig. 5: The intention of this figure is unclear to me. What do you want to show here? What do the error bars show? Why are you comparing 2016 woody cover with tree cover (which year?).

The aim of this section is to use an independent proxy variable (tree cover) and compare it to woody cover generated in this study, in order compute validation statistics.

Firstly, we compare tree cover and woody cover, measured as part of this study, and find they are closely correlated.

Subsequently, we use tree cover from the Bastin et al. (2017) dataset and compare it to the woody cover dataset generate in this study, and find a strong linear correlation. This correlation we use a validation statistic.

L6P12: This section now reads: "Finally, model predictions were compared to the recently published 4,684 sample calibration/validation dataset of percentage tree cover from Bastin et al. (2017) (all plots located in Namibia) (Bastin et al. 2017); each data point consists of a 0.5-ha plot, visually assessed for tree cover percentage using very high resolution imagery. Since we find observed tree cover percentage and observed woody cover percentage, sampled as part of this study, to be highly correlated (r=0.83), we assume the Bastin et al. (2017) dataset is assumed to act as a good proxy for percentage woody cover; it provides the latest estimate of tree cover in drylands and is based on the interpretation of contemporary high resolution imagery."

The caption of Figure 5 now reads: "Linear relationship between percentage woody cover (2016) at 5% increment classes, and percentage tree cover. Here, each annual estimate of percentage woody cover was compared to percentage tree cover derived from the Bastin et al. 2017 dataset. Errors bars show the standard deviation.."

Fig. 6: Their no units for the percentage changes.

Figure 6: Units have now been included

Fig. 7 + Fig 8: These figures could be smaller and combined in one figure.

Figures 7 and 8 have now been combined into a single figure as per the reviewers recommendation. Accordingly, the caption text has been revised.

Please also note the supplement to this comment:
https://www.biogeosciences-discuss.net/bg-2019-28/bg-2019-28-AC2-supplement.zip

---

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2019-28, 2019.

Printer-friendly version

Discussion paper