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Comment

Interactive comment on “Global changes in dryland vegetation dynamics (1988–2008) assessed by satellite remote sensing: combining a new passive microwave vegetation density record with reflective greenness data” by N. Andela

Anonymous Referee #2

Received and published: 29 May 2013

This paper discusses the combined use of VOD and NDVI time series to assess global changes in drylands vegetation. This topic is definitely of interest for the readers of Biogeosciences and the authors tried to carefully address the issues of changes in woody (persistent) and herbaceous (recurrent) covers in drylands, as well as to discuss potential drivers for the observed changes. Unfortunately the manuscript is far too long and difficult to follow. A clear effort of conciseness and simplification is needed before considering it for publication. The manus notably contains too many figures (12 + 1 in annexe). The authors should only select the most relevant ones. The text would

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definitely gain in clarity if the authors could better synthesize their results. I also found Sections 2 and 4 too long and in many aspects confusing.

My recommendation is that this paper should only be considered for publication after addressing the previous points on the clarity of the text as well as the following major points below:

(1) In Section 2, the authors described quite extensively their conceptual framework and related hypotheses (“expectations”) on the significance of changes in VOD and/or NDVI in terms of changes in woody, herbaceous and/or total above-ground biomasses. To my opinion, these hypotheses are too restrictive and wrongly formulated. I would rather avoid formulating them in such a ‘simplistic’ way before discussing the results. Moreover results that are supposed to bring light on these aspects (Section 5.1) are limited to the analysis of spatially and temporally averaged profiles for 3 different lands covers in South Africa, with no mention of any spatial and/or temporal variability (adding a measure of standard deviation could be an option). Based on these, ecological interpretations of changes in VOD and NDVI are realised at global scale (Fig. 12). It sounds to me that the authors generalised their findings and drew conclusions at global scale too quickly. For this part of the research, I would really recommend to realise a global scale analysis and to simplify what they called their conceptual framework. I doubt it needs a section on its own. It could easily be included in the section related to data description and methods.

(2) The method section related to the rainfall-driven models needs to be written again. In the current version, the description is spread over Sections 4.2 and 4.3, which makes it confusing. I would suggest writing again section 4.2 with a focus on the model and not on the vegetation/precipitation anomalies; these are just input data to the model. Basically the major information on methods related to the estimation of global vegetation trends are in the 1st and the last paragraph of this section. The other paragraphs concern the modelling part.

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(3) The results in Sections 5.3 are interesting but there is no need to describe all 3 figures (observed, predicted and residuals). Example: p17 line 5 the authors say “If observed and expected trends had the same direction this resulted in weakened residual NDVI trends”. Obviously as residuals = (observed – predicted), residual trends are not weakened, they are simply smaller. This remark applies to the entire paragraph.

(4) The discussion is too long and contains too many repetitions and/or reformulations of the results (or other sections). Examples: P21 L23-27 ; P24 L11-19. Moreover this section contains new results. Example: P20 L7-17. If regarded as important for the discussion, Fig. A1 should be included in the paper as normal figure and should be described in the results section. The authors really need to decide what they want to highlight as main original findings for this paper.

(5) Abstract and conclusions need to be written again and adapted based on new results and other changes in the manus. In line with point (1), the authors should be really careful with drawing conclusions for global drylands on the basis of results limited to South Africa and Australia (examples: ecological interpretation of co-variation between NDVI and VOD ; impact of changes in fire regimes).

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Minor points:

P10 L2: which VOD datasets?

P11 L19: which land covers types?

P18 L6: ‘annual time-step minimum NDVI’ could be simplified by ‘minimum NDVI’ (with mention that it relates to the permanent component in Fig. 9a).

P18 L21 - Trends in burned area: Are they related to changes in occurrence (i.e. number of burned area per year) or to changes in affected areas (i.e. percentage cover of burned areas) or something else?

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Fig 1a and b: not described in the results.

Interactive comment on Biogeosciences Discuss., 10, 8749, 2013.

BGD

10, C2344–C2347, 2013

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C2347

