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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

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Author response to referee comments 1 and 2,

Firstly we thank the two referees for their constructive and thoughtful comments, which have provided valuable suggestions to improve this manuscript. Comments of both referees were based on the manuscript submitted for access review and not the manuscript published in BGD. Comments of referee 2, provided before publication in BGD, were dealt with already before publication in BGD. Comments of referee 1, provided more recently, were used to improve the BGD manuscript. For completeness, below we offer detailed responses to both reviewers comments. Since both reviewers

comments were based on the initial submission, our responses will focus on the improvements between the revised BGD version and the initial submission (see supplement).

AUTHOR RESPONSE TO ANONYMOUS REFEREE 1

Comment 1. Based on the title "combining a new ..." and the theoretical discussion in Section I was expecting a more quantitative approach for combining the two observables. In contrast, in many places the work is rather suggestive and not backed up by hard facts, such as validation results. Many times wordings like "suggest", "may have been", "could" etc are used. I'd suggest to either strengthen the quantitative aspects or to ease a bit the expectations by modifying the title and the introduction.

Response: This is indeed an explorative study using a novel product. The exact relation between VOD and biomass is still matter of ongoing research and for that reason we cannot yet be that quantitative as we would like to be. We modified the title and introduction section accordingly. In the title, we now use "comparing" instead of "combining". In the Introduction section, we deleted about 10 lines text for conciseness and added the following text at the end of this section:
"Differences between NDVI and VOD are explored and their trends and co-trends are interpreted ecologically over the global drylands. A model is developed to estimate the dryland vegetation responses that can be explained by precipitation variation. Residual trends (i.e., the observed minus model-explained trends) and their potential drivers are discussed and attributed where possible, by comparison with independent datasets (e.g. burned area and grazing) and information from previous studies. "

Comment 2. Throughout the manuscript, NDVI and VOD are compared 1:1 (e.g. the discussion on page 17 stating that NDVI has less distinct trends than VOD; Fig. 8). Even though both indices commonly range between 0 and 1, it is not said that these can be directly compared as their relationship to vegetation density is not linear. Possibly the data first need to be standardized before comparison.

Response: During the revision, we removed the sentence "Box plots of the distribution of observed, expected and residual trends, stratified by land cover and humidity classes, illustrate that NDVI overall had less distinct trends than VOD" to avoid confusion.

In the revised version, we explicitly limit our theoretical framework to contrasting trends between NDVI and VOD, i.e. comparing the directions, rather than the magnitudes, of their individual trend. We understand that the exact physical relation between NDVI and VOD is still not fully resolved, therefore, we cannot yet fully interpret trends in NDVI and VOD that are for example both positive but of different strength. In our revised manuscript we interpret such a trend as “A long-term increase in both NDVI and VOD signifies an increase in the relative fraction of herbaceous AGB and/or an increase in total AGB”.

Comment 3. Both VOD and NDVI datasets are based on multiple input data sources with differing quality over time. The potential impact of the varying quality needs to be discussed.

Response: In section 3.1 (NDVI), we added: “Although data-sets are merged from several sensors and corrections were performed to create long-term time series (Tucker et al., 2005), they do not affect the calculated trends (Fensholt et al., 2012).”

In section 3.2 (VOD), we added: “Because of enhanced sensor characteristics (particularly the longer wavelength), the accuracy of VOD retrievals from AMSR-E is expected to be better than those from TMI and SSM/I. A comprehensive evaluation study by Liu et al. (2013a) demonstrated that the errors associated to sensor changes in the harmonized VOD dataset are small, however. The harmonized dataset captured long-term changes in total aboveground vegetation water content and biomass over different land cover types without sensor artefacts.”

Comment 4. Why is the analysis only performed until 2008, while both datasets are available until 2010? Including 2009+2010 would make the conclusions drawn from the analysis much more robust, as the strong La Nina event of 2010 may have flipped several of the trends observed until 2008.

Response: When the study of this manuscript was initially conducted in 2011, we focused on the period 1988-2008 as this study extends the study “Global vegetation biomass change (1988–2008) and attribution to environmental and human drivers” by Liu et al. (2013a) in Global Ecology and Biogeography. These two papers combined enable better understanding vegetation dynamics and the drivers behind these changes at the global scale, particularly over the dry land.

Comment 5. The manuscript is overly long with many repetitions. I expect that the text can be shortened with about 30% without losing any of its strength.

Response: We have made an effort to make the manuscript as concise as possible and managed to reduce the total length by 1200 words (15%) compared to the initial submission. This is the most we felt able possible without degrading the readability of the manuscript.

Minor issues:

Comment 6. P2.l7: The NDVI is not only sensitive to chlorophyll and canopy cover fraction but also to leaf area index, leaf angle distribution, and several other canopy characteristics. Even though NDVI and VOD are very valuable indicators to study dryland vegetation dynamics and bush encroachment, especially at the global scale, also hyperspectral data are widely used for this purpose (e.g. Oldeland et al, 2010). The readers should at least refer to the use of this technique in the introduction.

Response: In the introduction we review multi-decadal regional to global studies. Although Oldeland et al., 2010 provide an interesting analysis, long-term hyperspectral data are not available. We do now cite this paper in the discussion however.

Comment 7. P5.l6: “NDVI does not penetrate vegetation...”. This is not true. On the one hand the formulation is technically incorrect (it is not the NDVI that penetrates, but the radiance), but above all, the NDVI often does provide information of the substrate, otherwise the signal would always be more or less flat (i.e. close to 1) and not show any dynamics. It is true that the NDVI saturates for dense vegetation, but does the VOD never saturate? To my knowledge it does as well for very dense vegetation.

Response: In the revised manuscript, we removed this sentence as well as the sentences before and after this sentence for conciseness.

Comment 8. Section 2: the linear mixture concept is not new and has been used for more than 20 years in remote sensing research. Reference shall be provided. Also mention that the mixing model is a very simplified one because transmittance and multiple scattering is not accounted for.

Response: Added: “This is a somewhat simplified approach, as light reflecting from one component can affect reflectance of another component by multiple scattering and transmittance (Roberts et al., 1993).” And also cited “Roberts et al., 1998” as being an influential work using the linear mixture concept.

Comment 9. P7.l14ff: “Although there is....” reference shall be given for this statement.

Response: Added "Reich et al., 1997" as an appropriate reference.

Comment 10. P9: the four expectations are formulated in a confusing way. I suppose that 1) “an increase in the herbaceous biomass component” should be “an increase in the relative fraction of the herbaceous biomass component”? Similarly for 4) “a decrease in the relative fraction of the herbaceous biomass component”? Please clarify.

Response: We rephrased the four expectations as:

1. A long-term increase in both NDVI and VOD signifies an increase in the relative

fraction of herbaceous AGB and/or an increase in total AGB.

2. A long-term increase in NDVI combined with a decrease in VOD signifies an increase in the relative fraction of herbaceous AGB.

3. A long-term decrease in NDVI and an increase in VOD signifies an increase in the relative fraction of woody AGB.

4. A long-term decrease in both NDVI and VOD signifies a decrease in the relative fraction of herbaceous AGB and/or a decrease in total AGB.

Comment 11. P9.L21-23: “Sensor characteristics...” Sentence is out of place and can be removed.

Response: Done

Comment 12. P10.L9ff: also say something about the quality over time.

Response: Done, see also response on comment 3.

Comment 13. P10.L16-18: How realistic is it to assume homogeneous precipitation in a grid cell over areas that are strongly controlled by convective precipitation?

Response: We argue that this assumption is reasonable because we study monthly mean values. Although these regions are governed by convective precipitation, on a monthly basis we are not studying individual events but totals. For neighboring grid cells temporal precipitation patterns are generally very similar at this resolution (cited: Koenig et al., 2002).

Comment 14. P10.L20: you decided to use a static land cover map for a 20 year period? You should spend a few words on how this possibly affects the results of this study.

Response: Added to methods: “Only static livestock density and land cover data were available, but these could in some cases be combined with regional studies to interpret the influence on vegetation dynamics."

Added to discussion: "We used a land cover map of 2005 and for many regions agricultural extent increased during the study period. Therefore, trends may partly be explained by land cover conversion from natural land cover classes to cropland."

Comment 15. P12.L19 (and many other places): You use the term "expected" vegetation/anomalies versus "observed". Personally, I think that the term expected is not appropriately chosen in this context. What you actually mean is the variation explained by your API model. Therefore, I'd suggest to use another term (e.g., "explained", or "estimated")

Response: We changed all relevant instances of 'expected' to 'estimated'.

Comment 16. P13: First you explain why you should use anomalies and not the absolute values, while in the end you mention again that you also analysed the absolute values. This makes the reasoning unnecessarily complicated (also in the results (p16.L6ff) and discussion). Just mention that you analysed both and what different information both measures give you. Related to this, the one figure in the appendix can be moved to the main text.

Response: In the revised manuscript, we have chosen to provide the figure in the annex material to keep the manuscript as short as possible and because this figure is not central to our conclusions, but as raised by an earlier reviewer it is of general interest for the remote sensing community because several previous studies have analysed absolute values rather than anomalies. We have revised the description of the methods: "Here we develop models based on precipitation and vegetation anomalies (rather than the original vegetation indices) as all trends are present in the anomalies, and not in the seasonal pattern. So a model optimized for anomalies will give a better estimation of trends in the vegetation indices caused by precipitation. Although direct correlation between precipitation and vegetation indices will be higher than correlation between

vegetation index and precipitation anomalies, a model based on the original vegetation indices and precipitation would be more suitable to study the effect of precipitation on seasonal rather than interannual vegetation dynamics. However, to facilitate comparison with published studies, the analysis was also repeated using the original vegetation indices rather than anomalies (cf. Herrmann et al., 2005)."

Comment 17. P15: Explain how differences in temporal data coverage between pixels were accounted for in the slope estimation.

Response: Changed to "Data gaps were ignored in trend calculations yet are considered in their interpretation."

Comment 18. P17.L16: "... NDVI overall had less distinct trends..." Can this be said so easily? Are we really looking at the same units? Besides the differences seem to be very subtle and not significant (Fig. 8).

Response: During the revision, we removed the sentence "Box plots of the distribution of observed, expected and residual trends, stratified by land cover and humidity classes, illustrate that NDVI overall had less distinct trends than VOD" to avoid confusion.

Comment 19. Section 6.1: Not really a discussion, most of this has already been said before-> remove/condense.

Response: In the revised version, we rewrote section 6.1 to be more concise.

Comment 20. Section 6.3: Recently, a study was published on trends in remotely sensed soil moisture over almost the same period (1988-2010; Dorigo et al., 2010). Please discuss the results obtained in your study in the light of the results obtained by Dorigo et al. As vegetation dynamics are closely related to soil moisture availability.

Response: We have included the paper by Dorigo et al. (2010) in our discussion. The slightly different study period (1988-2010) might affect trends as compared to our manuscript (trends based on 1988-2008). Trends found by Dorigo et al. (2012) compare well to our precipitation driven model of expected/estimated trends in vegetation indices.

Comment 21. P22.L7: “In those regions....understory”. Do you have evidence for this? Literature?

Response: Woody encroachment into (arid) drylands is reported for many regions. In our manuscript, we refer to studies that help to test our theoretical framework (Fig. 2 and 3) and several papers of special relevance (Adamoli et a., 1990; Vegten, 1984; Oldeland et al., 2010; Fensham et al., 2005; Van Auken, 2000; and Briggs et al., 2005). For an extensive overview we refer to Archer et al., 2001 who provide a table of papers studying woody encroachment for many regions of the world.

Comment 22. P25.L17-19: The period 2001-2011 does not agree with your study period.

Response: Unfortunately data availability of possible drivers of global vegetation trends is limited. We do not suggest that trends over different periods can be directly compared. Rather we explore the possible effect of fire on vegetation indices (see e.g., supplement Fig. 11).

Comment 23. P28.L7: “Spatial distribution of trends...” This is very suggestive. Do you have evidence for this? And does CO2 really play an important role in areas that are strongly water-limited (reference)?

Response: The reasoning and references behind this conclusion are provided in the discussion (BGD-P8770L10 until P8771L13 and references therein; especially, Bond and Midgley, 2012; Higgins and Scheiter, 2012; Donohue et al., 2013; Franks et al., 2013; and Keenan et al., 2013).

Comment 24. P28.L21-22: Do you have evidence of advances in agricultural practices over these areas?

Response: In the revised version, we added the following paragraph to evidence the

advances in agricultural practices. "The strongest positive median NDVI and VOD trends were found in agricultural areas (Fig. 8). Increases of both indices over the world’s agricultural regions are explained by advances in agricultural practice including mechanization, irrigation and fertilization (Liu et al., 2013a). In India, Pakistan, Bangladesh, China, Ukraine, southwestern Russia, several European countries, the USA, and a number of other countries, substantial areas of agricultural land are irrigated (Wada et al., 2010). Evidence that increasing trends in NDVI are caused by irrigation and fertilization has been documented for India (Jeyaseelan et al., 2007) and the North China Plain (Piao et al., 2003), while Liu et al. (2013a) showed that positive VOD trends in southern Russia, China, India and the US are the result of increased agricultural production."

Comment 25. Fig.4: Negative significant correlations should not be masked but also shown as this provides valuable information.

Response: Negative correlation between VOD and precipitation is found for about 0.9% of the global drylands (significant positive correlation for about 61%). It seems caused by frost-related missing values and seasonal open water patterns in some pixels at high latitudes, altitudes or close to rivers. To keep this paper concise and to avoid over-interpretation where the negative correlation is caused by low data quality rather than vegetation changes, we decided to mask out the regions with negative correlation.

AUTHOR RESPONSE TO ANONYMOUS REFEREE 2

Comment 1. A clear effort of conciseness and simplification is needed before considering it for publication. The manus also contains too many figures (12 + 1 annexe!). The authors should only select the most relevant ones.

Response: Following your advice to better focus our manuscript, we deleted the methods, results and discussion of persistent and recurrent vegetation trends in our revised manuscript, resulting in a great simplification that has no effect on the key findings.

Moreover, we increased the conciseness of the paper by deleting/rephrasing several sentences of the various sections. Overall we reduced the text by approx 1200 words and replaced two figures by one new figure. Overall, the manuscript has been shortened by more than 15%.

Comment 2. 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 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.

Response: The conceptual framework and related hypotheses ("expectations" herein) are based on sound theoretical understanding. Given the fact that the VOD product is still relatively new, we felt it necessary to provide a theoretical framework at this point in the manuscript. We made an effort to reduce the length of this section but feel that shortening it too much would make it hard for readers to understand how we arrived at the hypotheses. We have expanded the sections on the relation between NDVI and VOD and now provide a figure with the mean (former Fig. 1) and standard deviation of both NDVI and VOD. This provides insight on the spatial distribution of results shown for southern Africa. We also refer to Liu et al. (2011a) who carried out an earlier

comparison between NDVI and VOD and showed that while NDVI saturates at denser vegetation VOD continues to be sensitive. A line referring to this has been added to the discussion (it was also mentioned in the introduction).

Comment 3. 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.

Response: We have updated section 4.2 and 4.3, now section 4.2 is focused on the 'precipitation model' alone, while section 4.3 deals with calculation of trends and their interpretation. Note that section 4.3 has shortened considerably, we deleted methods, results, discussion and conclusions about the persistent and recurrent vegetation components.

Comment 4. 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 residuals being (OBSERVED – PREDICTED), residual trends are not weakened, they are simply smaller. This remark applies to the entire paragraph.

Response: We shortened section 5.3 by reducing the amount of text of the second paragraph and deleting the third paragraph. Minor change: we changed 'weakened' by 'smaller'.

Comment 5. 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 be described in the results section.

Response: We have made an effort to make the discussion more concise and reduced its length by 450 words. We especially focused on removing repetition (for example: P21 L23-27; P24 L11-19). Note that Fig. A1 was already discussed in the results (second paragraph of section 5.2).

Comment 6. Abstract and conclusions need to be written again and adapted based on new results and other changes in the manus. In line with my 1st point, 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).

Response: Abstract and conclusions have been updated. A major change is that former conclusions iv and vii have been deleted. The first because the same point was made already in v and vi (for conciseness), the second because these results have been removed from the paper. In order to provide more global insight on the co-relationship between NDVI and VOD we added a figure showing standard deviation for both of them (see new Fig. 2). We would like to stress that the figure showing NDVI and VOD for three land cover types of southern Africa is meant as an illustration but not as any globally representative proof. The relationship between NDVI and VOD was already explored in the theoretical framework (and references therein), earlier literature (e.g., Liu et al., 2011a, Shi et al., 2008) and now also to a certain extend in the new figure 2. We agree that care should be taken generalizing the results of change in fire regimes and have tried to avoid this in the revised discussion: P26L3 "Outside the African savannas and woody savannas, the residual trends in NDVI are not easily attributed to changes in annual burned area, at least not using the data available to us".

Minor points:

Comment 7. P10 L2: which VOD datasets?

Response: We have updated the description of the VOD product in the data section.

Comment 8. P11 L19: which land covers types?

Response: Added: 'previously mentioned' deleted: 'selected'

Comment 9. 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).

Response: The text on 'annual minimum' and recurrent vegetation components has been deleted.

Comment 10. 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?

Response: Units are stated in the figure (% per year per 0.25° grid cell), so an increase of 0.1 indicates that 0.1 % of the 0.25° grid cell has been burned additionally.

Comment 11. Fig 1a and b: not described in the results.

Response: We have made some changes to figure 1 and figure 2 is new. Figure 2, containing the former Fig. 1a and b are now described in the results section 5.1.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/10/C4338/2013/bgd-10-C4338-2013-supplement.pdf

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