

***Interactive comment on* “Does Vegetation Parameterization from EO NDVI Data Capture Grazing induced Variations in Species Composition and Biomass in Semi-Arid Grassland Savanna?” by J. L. Olsen et al.**

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Dear reviewers. Please see below for answers to your comments and let us know if anything is not satisfactorily addressed. Note that figure 3 has been removed, and figure 2 and 6 (now 5) is altered. Figure 2 now includes GIMMS3g data (as requested) and the arrows in figure 5 (conceptual illustration) have been altered. The two altered figures are added here.

Attached is the manuscript with tracked changes (pdf)

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» Answer to Reviewers

We would like to thank the reviewers for the detailed and constructive reviews. We think that the review suggestions and the revisions as a consequence hereof have substantially improved sections of the discussion and results, as well as figures of the manuscript. We hope that the reviewers find the revised version of the manuscript more balanced in accordance with some of the critique points raised by reviewer#2.

» Reviewer #1

This paper presents an original approach to disentangle rainfall effects to other factors such as the grazing on the vegetation dynamics, and particularly the greening trends of the Sahel. It aims to analyze if NDVI data are able to capture variations observed in situ observations of vegetation caused by rainfall variability and differences in grazing regimes.

I noted that authors have taken into account most of the suggestions made during the first stage of the review process, so I have now just five comments:

1) For the definition of the SoS and EoS of the growing season, you used TIMESAT. You used the MODIS NDVI product, but it exists also the MODIS vegetation phenology product providing also estimates of the timing of vegetation phenology. So I wondered why you chosen TIMESAT rather than the existing product?

- Answer: The MODIS SoS and EoS products are in 500 m resolution and created mainly from 8-day EVI. We want to keep to the 250 m resolution data as this is the only MODIS resolution that is comparable with the smaller ungrazed plots.

2) To found the best parameter for monitoring biomass, you test here each parameter separately. Why you don't test the combination of parameters?

- Answer: This is a very valid point and multiple regression or PCA analysis is a natural next step. However we feel that this should be thoroughly treated in a separate study and perhaps include biomass sampling from more diverse locations and with less focus

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on grazing. For this study it is also a point of discussion to keep the NDVI metrics at level with the research gone into identifying the greening of the Sahel, i.e. using a single parameter.

3) You found for your site that the best parameter for monitoring biomass is the small integrated NDVI. Do you think that this will be the case for other Sahelian areas?

- Answer: For annual herbaceous vegetation, yes, it is reasonable to assume that focusing on the growing season activity represented by the small integrated NDVI should be the best parameter.

4) P11, L26 change 15 and 30% by 15% and 30%

- Answer: Corrected

5) Last comment, this paper benefits from an important database of field measurements, so it is quite “frustrating” that you used only 8 years of data. I understand the reasons why you don’t use AVHRR data but however I wondered if you plan to test also the possibility of using AVHRR dataset.

- Answer: This is a good point. The time series from the AVHRR GIMMS3g pixel covering the test site are now included together with site averaged biomass and precipitation. The results of the GIMMS3g comparison with in situ measured biomass have been discussed in the text also, however pointing out that a strict comparison is inadequate due to the spatial scales mismatch.

» Reviewer #2.

This paper addresses the impact of grazing intensity and enclosure on the relation between remote sensing data and standing biomass, considered a proxy of above groundnet primary productivity (ANPP). It is based on a very valuable long-term dataset, described and analysed in Miehe et al 2010, and MODIS data. It proposes a new hypothesis concerning the Sahelian NDVI trend.

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Three main results are shown:

i) Differences are found between exclosures and grazed pastures in terms of biomass, plant species and species traits. It is shown that end-of-season standing biomass is higher for exclosures than for grazed plots, mostly for years with cumulated rainfall larger than average.

ii) The best NDVI metric to correlate to end-of-season biomass, for both exclosure and grazed pasture, is the NDVI integrated over the growing season (iNDVI).

iii) It is shown that the higher end-of-season standing biomass of exclosures does not translate in a higher iNDVI.

The discussion then suggests that increasing grazing intensity in the Sahel over the last decades may have contributed to the 'regreening' trend observed by satellites. The paper reads well. References are up to date and relevant. Figures are clear (but see below, some figures should be added). It is well suited for Biogeosciences. This study addresses two very important points: first, the impact of free or managed grazing on biomass and productivity, second its impact on NDVI and the possible role of changing grazing intensity on the NDVI trend detected by satellites in the Sahel. It potentially provides an important contribution to the 'Sahel greening' debate and deserves publication. A number of issues have to be addressed before it can be published though.

- Answer: We would like to take the opportunity to thank the reviewer for the insightful, detailed and constructive comments and suggestions for an improved and more balanced manuscript. We agree with reviewer on more or less all issues raised and believe that the revised version of the manuscript has largely benefitted from the comments and suggestions.

General comment:

The paper is based on a very valuable dataset that was already analysed in Miehe et al. 2010. It is unnecessary to repeat what was done and found before. That should

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mostly go in the introduction, since it is known already (ex. difference in terms of biomass, effect of rainfall). The paper should focus on what is new: namely the link between field and remote sensing for enclosure and grazed land, and the possible differences. In the same vein, the different metrics have been compared before in the same area (M'Bow et al. 2013 among others). Again, the present paper should focus on what is new: the differences between enclosure and grazed land in terms of these metrics. Some trimming is necessary.

- Answer: Thanks; the suggested trimming has been done as suggested. Most of the biomass/precipitation differences are now merely described under the site description with reference to Mieke 2010 and a few sentences is provided in the introduction as well. Reviewer 1 suggested that the AVHRR GIMMS pixel values are shown together with biomass and rainfall. While this is not strictly necessary for the grazing issue, it is a good suggestion and will satisfy some curiosity amongst the readers involved in validation of long term dataset. It is however pointed out that a strict comparison is inadequate due to the mismatch of spatial scales between GIMMS pixel resolution and Wido ground observations. In summary, figure 2 has been kept and improved with the GIMMS3g iNDVI time series, while figure 3A & B are now left out.

The conclusion that changes in grazing intensity may play a role in the NDVI trend is largely unsubstantiated and largely extrapolated. This extrapolation ignores a number of interfering factors. I recommend to better substantiate the results and conclusions and to be more cautious before generalizing. Following are two issues to address so as to reach more solid conclusions.

- Answer: Going through the manuscript again, we fully agree with this important comment and for that reason we have chosen to substantially change the way the linkage between grazing and the Sahel greening is presented in the discussion since this hypothesis was in fact not supported by the data presented; changes to the revised manus are explained further below.

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1) Confusion between ANPP and end-of-season standing biomass. The measurements of end-of-season biomass is a proxy for ANPP. It is best suited for annual vegetation. However, as it is commented in other papers (by Miede among many others), several factors impact the ANPP/end of season standing biomass: herbivory (grazing, insects etc ...), phenology and measurement dates and frequency. For most studies, these factors are considered negligible, since, for example, they do not change a lot between sites or years. In this study, the very objective is to address the impact of differences in grazing intensity and species composition, therefore these factors have to be discussed.

- Answer: We thank the reviewer for this comment. We acknowledge that this is an important difference when the aim is to study differences in grazed and non-grazed biomass. We have chosen to use a consistent terminology throughout the manuscript by using the term “end of season standing biomass” (abbreviated as ESSB in the revised manus) since this reflects more accurately what has been measured at the site over the years studied. The difference between ANPP and ESSB has been explained more thoroughly in the “field data” section (ESSB being used as a proxy for ANPP) – furthermore the timing of ESSB sampling are discussed in the context of changes in species as pointed out by the reviewer (see below).

There are already some elements in the text (in the discussion section), which try to estimate ingestion by cattle. In my opinion, this deserves a full treatment, with a description of methods, data and results. I acknowledge it is a difficult task. However, it needs to be addressed to support the conclusions on the impact of grazing on ANPP (note also that ingestion is not the only effect. Trampling also occurs). Some words on the consequence of changing leaf lifespan on the date of peak biomass may be needed also. If enclosure plants have higher lifespan, does that mean that late september biomass data are closer to ANPP than for enclosure than for grazed pastures? Is there 2-year old plant matter (e.g. litter, straws) that should be removed from ANPP. All over the text, there is a need to distinguish ANPP from end-of-season aboveground biomass

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(do not use productivity or production when end-of-season biomass is the variable or provide corrected estimates of ANPP).

- Answer: Again, we acknowledge the reviewer comment on this. However, the full data required to address and adjust for the difference between “end of season standing biomass” and ANPP do not exist for this site. Unfortunately estimates of trampling and ingestion do not exist in more detail than already put into the discussion. We have chosen to include the information about livestock ingestion in the section of field data with a reference to the publication by Miede et al 2010 including this information. This information is used to show that the ESSB sampled in grazing exclosures are orders of magnitude higher than the value expected to be ingested by livestock from the current stocking densities. We believe that this is as far as we can go based on the available data, but the point that the difference between ESSB for grazed and non-grazed areas by far exceed the quantity expected to be ingested by livestock has been made more clear in abstract and conclusion.

2) More important: The possible impact of changes in grazing intensities on the NDVI Sahel trend is not really substantiated. The results nicely show that NDVI / biomass relationship is different for exclosure compared to two different grazing systems. One can conclude that there is no significant differences between the grazed plots (communal versus controlled), despite different grazing intensities. Then, if the (un-cultivated) Sahel as a whole is considered a 'grazed' area over the last decades, as opposed to a large exclosure in 1984 becoming progressively grazed, we can draw the opposite conclusion: 'Changing grazing intensity is NOT responsible for NDVI trends in the Sahel'.

- Answer: This is a crucial point and again we fully acknowledge the critique by reviewer that the conclusion was in fact not supported by empirical evidence. We have discussed this amongst authors and a large amount of text has been changed in the revised manuscript (in particular the abstract, discussion and conclusion) to make it more clear what can be concluded from the Widou data presented and what is merely hypotheses that can/should be studied further with the availability of appropriate datasets.

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I recommend to revise the discussion/conclusion of the paper, based on more substantiated findings, and I recommend to have a much more balanced conclusion.

- Answer: We have done so and hope that reviewer agrees that the revised manuscript appears more balanced in the sense that we do not claim anymore that the data presented here supports that grazing will cause a greener Sahel. What can be concluded from the data presented is nicely summarized by reviewer in the beginning of the reviewer comments and the discussion of the impact on EO greening of Widou/Sahelian rangelands from changes in grazing, is kept as discussion items not claimed to be supported by the ground observations.

Minor comments:

1) The influence of grazing intensity on NPP estimates found by other authors is not always in line with what is reported here (decrease of NPP due to grazing). For instance, see Hiernaux et al. 2009, J. Hydrology for similar ecosystems. Please moderate/change your statement.

- Answer: Thank you for pointing this out. The statement has been modified to include points from this publication: Hiernaux, P. Mougou, E., Diarra, L., Soumaguel, N., Lavenue, F., Tracol, Y. and Diawara, M., 2009, Sahelian rangeland response to changes in rainfall over two decades in the Gourma region, Mali: Journal of Hydrology, v. 375, p. 114-127.

2) Enclosure is a very atypical situation in the Sahel. The nature of such enclosures deserve some comments. In my opinion, a cattle-free Sahel would not look like (fire-free)enclosures. The difference between communal and controlled grazing makes probably more sense.

- Answer: This is of course a very valid point, and it should already have been mentioned in the text. It has been added to the discussion.

3) The differences in biomass / iNDVI for the different plots is really interesting. A

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figure with ANPP / iNDVI would be nice (in addition to end-of-season-biomass/iNDVI), which estimates of grazing and phenology influences to correct ANPP as much as possible. I was wondering if the differences would still be significant when such corrections are accounted for. Also, it seems that one year is largely driving the correlation for enclosure (do the results still hold without that year, in terms of different relationships between the plots, after correction for herbivory/trampling effects ?) Consider also including figures with average annual cycle of NDVI for the plots, as an illustration of the differences (or the absence of).

- Answer: This is a valid point as well, but unfortunately apart from the estimation of ingestion already mentioned in the discussion, we have no solid data/methods available to convert standing crop into ANPP. Therefore the wording of the text has been changed to reflect this by writing “ESSB” instead.

4) Are there any data or literature results on the optical properties of the enclosure plots ? That would feed a nice discussion. Differences in canopy architecture? LAI ? leaf optical properties ? Nitrogen content?

- Answer: Yes this is a very important point. Unfortunately we do not have detailed information on this and without this information there is no empirical support (in the Widou data presented) for the linkage between species composition and the impact on seasonally integrated NDVI. For this reason we have based a part of the discussion on the results reported by Mbou et al. (2013) where such data is available from the Dahra site.

5) A number of statements have to be down-toned or reformulated. For instance ‘It is beyond doubts that the increasing population in Sahel and the widespread practice of pastoralism has caused a significant increase in livestock over the recent decades (Ickowicz et al. 2012)’. I would be much more cautious, as real figures for livestock are extremely local or often inaccurate, to say the least (except maybe in some places with sedentary cattle, like the Ferlo ?). Also, in agropastoral Sahel, increase in population

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is not always accompanied by increase in livestock, you may have less land available for grazing.

The last two sentences of the discussions are questionable also. In some places, NDVI trend has already been shown to correspond to herbaceous biomass and ANPP trends (long term field and satellite studies, papers by Dardel et al., among others for recent studies).

- Answer: Yes agreed, the statements mentioned have been toned down or changed (see also answer to 2. major point of review). The study of Dardel 2014 was already included but has been additionally used in the text.

Beside, I don't see why the Olsen et al. paper is in line with the paper on trees in the Sahel. In region where tree cover is less than 5%, iNDVI has not been shown to depend on tree cover changes (neither changes in magnitude, nor in specific composition), as far as I know. Please explain. The term 'improvement' needs to be defined in a scientific way, if to be used in a discussion like this (Biodiversity? Productivity? People income? Livestock?).

- Answer: Ok this part has now been removed from the paper, but as reviewer is aware (we believe) new research from Brandt et al. (some published – some in prep) is looking into these interesting issues in relation to the greening debate.

6) A technical question, of minor importance. There may be a pixel-size issue, when large view angle MODIS data are plotted against field data. The authors may want to consider only near-nadir data. That may even improve their results. Perhaps this has been looked at already.

- Answer: Yes, this is a question that is/have been studied amongst the co-authors and reviewer is right that the angular effect is of importance when calculating seasonal metrics from MODIS index data in the Sahel. In this study we have chosen to be on the "safe side" by using fitting algorithms based on the MODIS standard product that does

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already to some degree incorporate selection of low view angles in the compositing scheme (the one observation with the lowest view angle out of the two highest NDVI observations in a given composite period is selected). The MODIS BRDF corrected product could as well have been selected for the current study – however it is expected that the curve-fitting performed when using the TIMESAT software for calculating the NDVI seasonal metrics does mitigate this issue of off-nadir measurements.

For these reasons, I recommend 'major revisions' to the manuscript.

Please also note the supplement to this comment:

<http://www.biogeosciences-discuss.net/11/C9315/2015/bgd-11-C9315-2015-supplement.pdf>

Interactive comment on Biogeosciences Discuss., 11, 16309, 2014.

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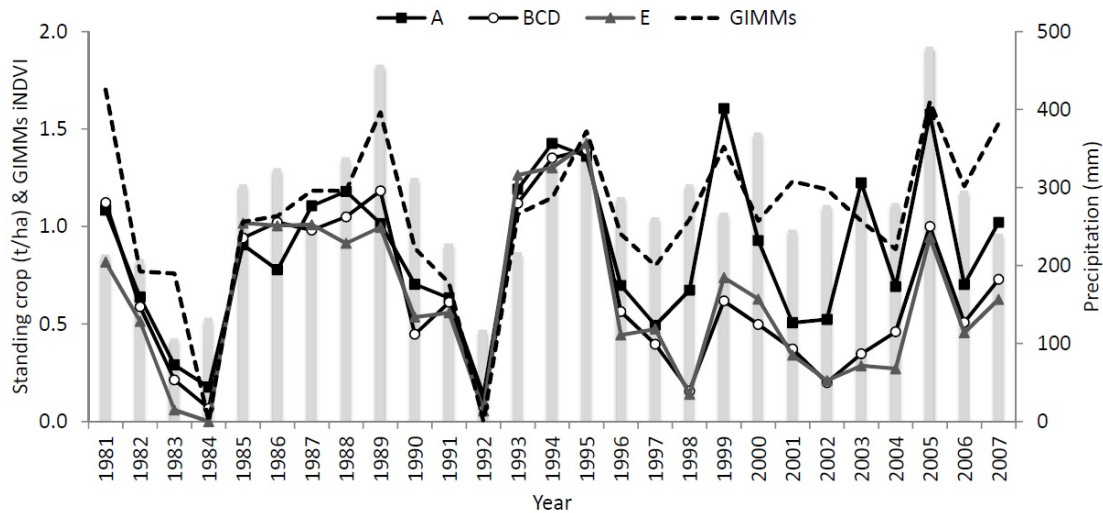


Fig. 1. New fig 2

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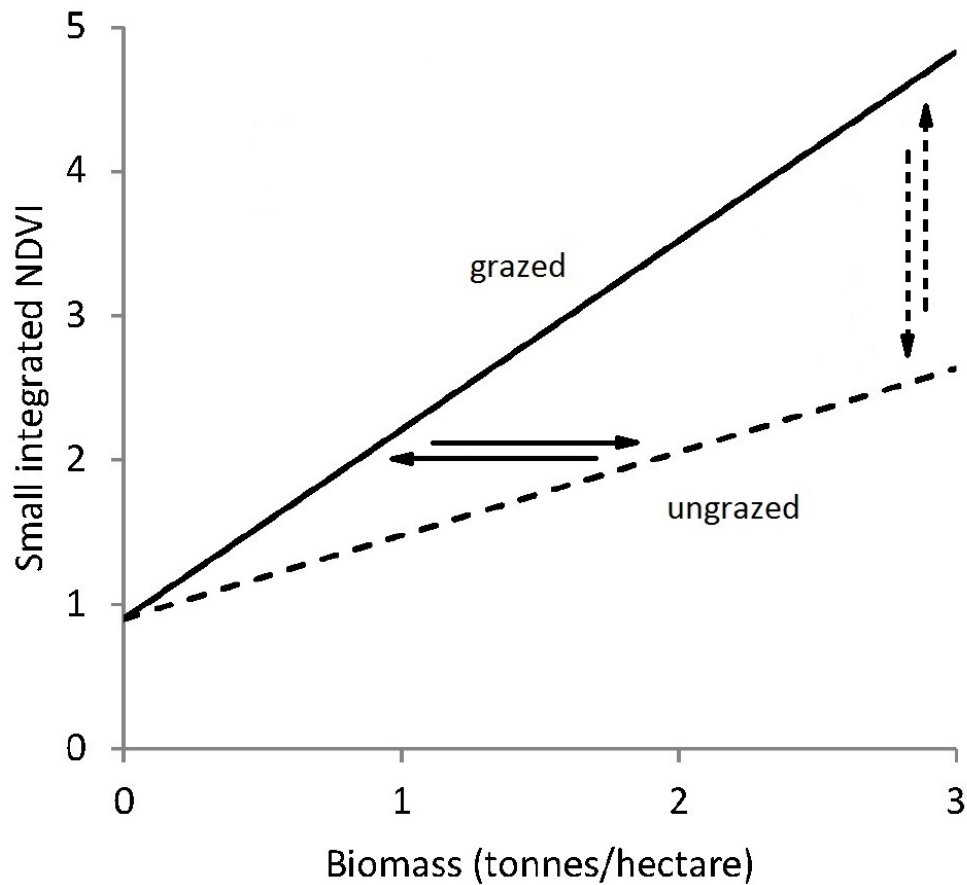


Fig. 2. New fig 5 (previously 6)

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