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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.

Anonymous Referee #2

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Review of Olsen et al. 2015, BGD.

This paper addresses the impact of grazing intensity and exclosure on the relation between remote sensing data and standing biomass, considered a proxy of above ground net 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.

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.

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 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 exclosure 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 exclosure and grazed land in terms of these metrics. Some trimming is necessary.

The conclusion that changes in grazing intensity may play a role in the NDVI trend is

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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 to reach more solid conclusions.

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 Miehe 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. 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 treament, 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 exclosure plants have higher lifespan, does that mean that late september biomass data are closer to ANPP than for exclosure 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 (do not use productivity or production when end-of-season biomass is the variable or provide corrected estimates of ANPP).

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 betyween the grazed plots (communal

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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'. 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.

Minor comments:

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

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

3) The differences in biomass / iNDVI for the different plots is really interesting. A figure with ANPP / iNDVI would be nice (in addition to endèof-season-biomass/iNDVI), whith estimates of grazing and phenology influences to correct ANPP as much as possible. I was wondering if the differences would still significant when such corrections are accounted for. Also, it seems that one year is largely driving the correlation for exclosure (do the results still hold wihtout 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).

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

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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 signifcant 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 unaccurate, to say the least (except maybe in some places with sedentary cattle, like the Ferlo ?). Also, in agropastoral Sahel, increase in population 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). 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 chnages 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 ?).

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 event improve their results. Perhaps this has been looked at already.

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

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