**Biogeosciences
Discussions**

Interactive
Comment

Interactive comment on **"Disentangling the effects of climate and people on Sahel vegetation dynamics"** *by* **J. W. Seaquist et al.**

J. W. Seaquist et al.

We are most grateful to the reviewers for contributing their insights and therefore helping us sharpen our interpretations. In our response below, we treat first those comments that the reviewers have in common. We then respond to each comment received from each reviewer in chronological order.

From Eric Lambin: I wonder why the authors selected the peak NDVI and LAI values to compare remote sensing and model-based greenness. I believe that integrated NDVI and LAI values over the growing season would better represent the potential impact of land use on vegetation cover. NDVIMAX and LAIMAX are likely to be strongly influenced by rainfall distribution at the scale of a couple of weeks. Integrated NDVI and LAI values are more likely to reflect the influence of land use on vegetation as this metric registers the cumulative impact of land management over the entire growing

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season. It is thus less sensitive to variations in the seasonal distribution of rainfall.

and

From Ning Zeng: It seems rather unusual to use the maximum NDVI and modeled LAI for correlation analysis. Although the seasonal cycle may be smooth enough so that the max values would likely reflect overall vegetation growth, but one can not exclude influence from major short-term fluctuations. A more popular approach is to integrate the variables over the whole growing season, or annaul mean which would be the simplest thing to do. If the authors choose to stay with max values, it needs to be demonstrated to give similar results as growing season or annual mean.

Authors' Response: The NDVI integral is often assumed to be a proxy for net primary production. If we were to present the integral, it would then be logical to choose LPJ net primary production (rather than leaf area index) for comparison purposes. However, we don't feel that the NDVI integral is the best metric in this case because of the uncoupling between the amount of photosynthetically active radiation absorbed by the vegetation canopy and plant growth. This would undermine the comparability between observed and modelled vegetation. Furthermore, the computation of NPP by LPJ requires a number of additional assumptions. For reasons of parsimony, we thought it best to settle with relatively simple, yet comparable measures of vegetation status – peak LAI and peak NDVI. Finally, this paper can be thought of as an extension of the study of Hickler et al. (2005). In that study, the same variables were used for comparison. We did repeat the methodology with both the yearly NDVI integral and yearly LPJ-derived net primary production but the same story emerges. In our revised manuscript, we will add a stronger rationale for choosing peak growing season NDVI and peak growing LAI in our study.

Eric Lambin's comments:

(1) Whilst vegetation data are dynamic, the data on cropping and pasture intensity are static and correspond to the end of the study period. It is not cultivation per se that

is expected to cause land degradation but a shortening of the fallow cycle, a deficit in fertilizer use, inappropriate land management practices, etc. For grazing, it is a lack of mobility of herds in case of drought that might be associated with land degradation. The maps on cropping and pasture intensity poorly capture these dynamic land use processes.

Though we already mentioned that the data on cropping and pasture intensity are static in our manuscript, we did not expand on this. We will add to our discussion by taking into consideration these observations.

(3) The use of least-square regression to measure vegetation cover trends only picks linear trends. Clearly, vegetation dynamics in drylands rarely conform to a linear pattern as it displays fluctuations. These fluctuations sometimes result in a net increase or decrease, but not necessarily in an incremental way.

Most of the interpretations of Sahel greening (at the continental-scale, at least) are grounded on studies implementing temporal least-squares trends so it may be something that the community familiar with such work can relate to. We agree it would be highly interesting to study various properties of change not addressed in these studies, for example persistence, abruptness, etc. This work is of course beyond the scope of the present paper. However, we think this is a good comment will include some discussion of the limitations of looking at linear trends in our revised manuscript.

(4) The suggestion (page 3053, lines 5-6) that massive food aid in parts of Chad and Sudan might explain a strong positive trend in NDVI in these regions is highly speculative and not supported by any detailed analysis. This is a complex issue that would require a very careful, more local-scale study. This sounds as a rather ad hoc explanation, which could very well be reversed (i.e., droughts and land degradation are causing conflicts and population displacements).

We agree that this comment is speculative. We will remove it in the revised version.

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(5) I would avoid attributing causality based on this purely statistical study. Thus, page 3054 line 19, rather than "livestock grazing is generally not a driver of vegetation greenness", I would write " livestock grazing is generally not associated with spatial (or interannual?) variations in vegetation greenness".

We agree and will rephrase the sentence.

(6) Differences in soil type, which are not represented in this study, could explain the low data-model agreement. I don't know how soils are represented in the LPJ-DGVM model but this may deserve a short discussion as soil attributes and their impacts on hydrology tend to be ignored in this type of study.

Soils in the LPJ-DGVG model derive from FAO's soil map of the world. The soils are divided into two layers of constant depth (0-500 and 500-1500 mm), and textures assigned according to Zobler (1986). For a complete description of the hydrology, see Sitch et al. (2003) – see manuscript for references. The soil map of the world is a highly generalized product and the data model used for mapping soil attributes are vector polygons. Therefore the true heterogeneity of soil attributes is not captured. We might expect the absolute values of some of the hydrological output (and therefore associated vegetation predictions) to be inaccurate, but would expect the fluctuations in such variables to be more accurate. As we have applied a parametric measure of data-model agreement in the temporal domain (that would the effect of non-linearity, for example) we expect to minimize this error. We therefore feel that low data-model agreement has as much to do with other factors already mentioned. A brief discussion of this aspect will be included in the revised version of our manuscript.

Ning Zeng's Comments:

a) All the analysis here is based on the trend from 1982-2002 when increased precipitation in the Sahel was apparently the main driver of change. However, this does not exclude possible strong human influence on longer timescales when whole landscape may be transformed by human activities which could have larger impact on climate.

The conclusions thus should include caveats that put the analysis in broader context.

We agree with this statement and will modify the text to include this in the final manuscript.

c) Fig.5a caption states that the x-axis is 'NDVI trend correlation coefficient', while the text says it is 'NDVI trend'. I suspect it is the latter because Fig.5b is the percent change, and the former is not discussed at all. Please clarify.

Fig. 5a denotes the correlation coefficient associated with the temporal trends of NDVImax. We will clarify this in the text in our revised submission.

d) The authors "identify a weak, positive correlation between data-model agreement and pasture intensity at the Sahel-wide level". Because the area with good 'data-model agreement' tends to be where it has been greening in response to increased precipitation over the last 20 years, I wonder if this could be simply due to the possibility that grazing is just following the greening as nomads slowly moved with the rain over the period? Just a conjecture, but it may be possible to test with your data.

We think this is a possibility and we can include this as a potential interpretation in our revised manuscript. Any additional analysis along these lines would be highly interesting for future work.

e) We will discuss Taylor et al's study in our revised manuscript – it is certainly very relevant.

Interactive comment on Biogeosciences Discuss., 5, 3045, 2008.