

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

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This is an interesting study that addresses the controversial issue of the role of human activity in vegetation cover change in the African Sahel. This study applies a methodology characterised by a higher level of sophistication compared to previous studies. Yet, the methodology adopted is well aligned with previous efforts and the results are perfectly consistent with most recent regional-scale analyses of this issue. I find the study convincing. The limitations are well discussed towards the end of the paper.

However, a couple of issues are of concern: (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

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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. (2) 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 season. It is thus less sensitive to variations in the seasonal distribution of rainfall. (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. (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). (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”. (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.

Minor comments: (1) page 3050, line 19: it should be “land cover” rather than “land use” (2) page 3055, line 27: replace “are” by “a” (3) page 3058, line 10: “population

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density and agricultural activity ”

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