

Interactive comment on “Dryland vegetation functional response to altered rainfall amounts and variability derived from satellite time series data” by Gregor Ratzmann et al.

Anonymous Referee #1

Received and published: 21 April 2016

This research attempts to understand the impact of rainfall variability on vegetation productivity across two dryland vegetated regions in Africa. For that the authors used an extensive time-series (1980s-2010s) of concurrent rainfall estimates and NDVI values and shifting linear regression models were selected to obtain the estimates of vegetation response as a function of rainfall data.

Overall, I find the manuscript to be well written, although missing some necessary details (see specific comments below). The objectives are clearly stated and the research aims to address them. However, I have a major concern that is related to the way they addressed (or not) the issue of temporal autocorrelation when modelling vegetation productivity proxy as a function of rainfall estimates. I only see a reference to ordinary

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least squares being used, with no reference to using a method allowing for errors to be correlated. Also, in terms of the phenological parameterization model that was used, can the authors provide an indication of its applicability, that is, was it compared with reference data?

Finally, although English is not my native language, I believe the grammar in the Supplementary Information could be improved.

Find below some specific comments:

Page 3, line 22. "...higher interannual length of the wet season variability..." sounds confusing, please replace by "...higher interannual variability of wet season length..."

Page 3, lines 33-36. Maybe I'm missing something but I failed to understand why dimensionality is an issue here.

Page 3, line 39. GIMMS was not defined previously. Also, please include the period of the ARC2 climatology (concurrent with the NDVI product?) and its spatial resolution.

Page 4, line 7. Initially β was defined as "vegetation response", so it's odd to see it here as " β response". Maybe keep it just β throughout the manuscript.

Page 4, lines 8-9. Please provide reference(s) or the rationale to justify this rainfall threshold.

Page 4, line 12. Please provide the period of the land cover map (2001-2012?). The correct designation of this MODIS product is MCD12C1. Also, please mention the land cover type (legend) that was used. MCD12C1 is currently released with three land cover legends.

Page 4, line 14. What's "sub-pixel land cover frequency"?

Page 4, lines 20-29. The authors should describe how they accounted for temporal autocorrelation in the regression. Further, an indication of the magnitude of pixels with negative slopes would be helpful.

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Supplement Information, page 6. "The principle of the SLR is depicted by Fig. S2 1". Please correct as should be pointing to Figure S3.

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Page 4, lines 32-33. I'm assuming the comparison between MAPi and MAP is done on a pixel basis, but that was not completely clear to me.

Interactive comment

Page 4, lines 33-34. How were the coefficients binned? Averaged? Please clarify and include also this information in all relevant figure captions.

Page 4, lines 35-36. Please provide additional information regarding choosing this particular type of model and what error distribution and link function was chosen and why.

Page 4, lines 37-40. I found this description hard to follow so perhaps the authors should rephrase it. E.g., there's no need to say upper 90th percentile, 90th percentile is enough.

Table 1. It would help the reader if the order of the regions on this table mimics that from Figure 3, 4 and 5.

Page 5, lines 1-2. I would suggest using a non-parametric test to confirm the results of the ANOVA, as the latter assumes a given (Gaussian) error distribution for the p-values to be meaningful.

Page 5, lines 16-17. For W=7, there's an increase of the response in wet periods for MAP values > 750 mm and this is not showing for the other pre-selected W values. Is this significant and has any meaning?

Page 5, lines 27-28. This should take into account the fact that in WA β_{max} for W=21 (dry and wet) is higher than the corresponding values for W=11 and 15.

Page 5, lines 28-29. Even in WA for W=7?

Page 5, lines 34-35. It would be helpful to have a list of non-significant pairwise combinations.

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Interactive comment

Page 6, line 14. Maybe I'm missing something, but the vegetation response IN ALL CLASSES is consistently lower in WA when compared to SWA.

Page 7, lines 3-6. In this study the authors tested only for the impact of rainfall on vegetation response, so other determinants can only be offered as hypothesis, so I would suggest rephrasing to "... might become relatively stronger factors."

Page 7, lines 8-9. Absolute values of what? Also, from Figure 3 but I don't see how in WA the region's response to hydroclimatic period is altered with respect to W. Did you mean unaltered?

Page 7, lines 14-15. The effect of W increases in terms of what?

Page 7, line 16. Can you identify those deviations in Figure 3?

Page 7, line 18. Please clarify what you mean with "local differentiation in land use" and what evidence you have to support this interpretation.

Page 7, line 39. Please keep the same convention and substitute "upper 10th percentile" by "90th percentile".

Page 8, line 2. CVP is given in Fig. 2c.

Page 8, line 5. CVS at peak β is not that different between SWA and WA. Maybe you meant CVP at peak β ?

Page 8, lines 18-19. Could the relationship between peak β and MAP be dependent on climate AND soil?

Figure S1 and S2 (and where appropriate), the authors need to include the year or period associated to the MODIS land cover data.

Supplementary SLR analysis, page 7. ANPP was not defined previously.

Supplementary SLR analysis, page 7. What's Fig. S3 2 and Fig S3 3? This part going from the bottom of page 7 and beginning of page 8 is hard to follow. Also, there's no

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reference to Figures S5, S6, S7.

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