

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 #3

Received and published: 9 May 2016

This paper explores the response of vegetation to rainfall and hence water availability across a gradient of precipitation in two sites in west and south-west Africa using a time series of NDVI and rainfall data.

The analytical approach involves: 1). shifting time series analyses run per grid cell with a proxy of vegetation productivity as the dependent variable, and rainfall as the independent variable, assessed over a series of time-windows of length 7-21 years. (The authors also explored models accounting for the previous year's rainfall; plus the interaction effects between the previous year's rainfall and following year's rainfall. However ultimately the most parsimonious model with rainfall of the given year is ever chosen, based on authors' assessments of AIC.) This analyses produces a time series of beta

[Printer-friendly version](#)

[Discussion paper](#)



values/productivity response to rainfall per grid cell, across different time windows. 2). The beta values per grid are then binned, and used as the dependent variable in the second part of the analysis that uses mean annual precipitation as the independent variable: essentially asking how does the vegetation response to rainfall depend on rainfall?

The authors also assess the peak vegetation responses to precipitation and variation in rainfall and the wet season length; and finally exploring vegetation-specific differences in responses to rainfall. The paper has applied significance in terms of understanding the responses of dryland vegetation under future climate scenarios, in addition to furthering more pure science objectives. However I perceive some problems with the OLS analysis as outlined below, which I think should be addressed as a priority, since this may affect the conclusions of the paper. In addition, the conclusions of the paper should make further consideration of the anthropogenic factors in each of the study sites, at least in terms of better explaining how the patterns observed (if the modelling is robust to the potential autocorrelation problems) might also be moderate by human behaviours, particularly in areas with many crops.

Overall with respect to the writing, whilst the aims of the paper are quite clear, the readability of the paper is hampered by a) unnecessarily convoluted and confusing language and sentence constructions b) some undefined terms and c) the use of different terms to describe the same parameter. This unfortunately detracts from the science undertaken. (I appreciate that the authors may not be native English speakers, so I hope this comment is understood as it is intended; as a constructive means to improve the paper, rather than a criticism of their efforts thus far). Specific examples are:

1. The overall variable of interest, beta. Initially this is defined as vegetation response to rainfall, in other places it is described as the 'beta response'. This must be standardised throughout the paper, e.g. with the use of a subscript.
2. Abstract L26: 'we conclude that higher. . .'. This sentence is confusing. 'Rainfall

[Printer-friendly version](#)[Discussion paper](#)

plasticity' doesn't really make much sense, especially when the paper later on contains precise terms concerning measures of precipitation. As such I think some alternative terms would be better in this paper overall, and particularly the abstract to help the reader.

3. Abstract L23: 'interannual rainfall amount variability' – vs L31 'rainfall variability'. Then on Page 3. L.21, 'absolute rainfall amounts' used. So we have a series of different terms i.e. 'rainfall', 'rainfall amount', 'precipitation' and 'absolute rainfall amount' which I think are all describing the same physical parameter. Better to choose one precise term such as 'total precipitation (mm)' and be consistent throughout, modifying it as necessary e.g. coefficient of variation of precipitation. In another case, on page 7, L8, you have 'some beta sensitivity to W for absolute values'. However THE parameter you are discussing the absolute values of is not stated.

4. Page 4. L27. W (window) and WA (west Africa). It would make the reader's life easier to differentiate these two abbreviations. For the Window parameter, the units (years) should also be added (e.g. W7years)

5. Page 3 L38. GIMMS is not defined before being introduced in the text.

6. 'Sub-pixel land cover frequency' : I think there is a sub-pixel land cover distribution as a result of the resampling procedure. Is this correct?

7. Other points on language that should be addressed involve more careful checking of the text e.g.: Abstract L17. 'as explanatory variable' –change to as an/the explanatory variable

8. 'Hydroclimate period' – probably easier to use this term once and thereafter say 'wet' and 'dry' seasons. Keep the language as simple as possible, allowing the reader to focus on content.

*****ANALYSIS AND INTERPRETATION****

Page 4, L9. A month is wet season if >20mm precipitation. Is this a recognised thresh-

Printer-friendly version

Discussion paper



old in the literature ? Please cite a reference. This is an important threshold and analytical step because on L34 the data is partitioned into binary classes of wet and dry seasons- changing the threshold will therefore affect the partitioning.

Page 4. L20. With respect to the analyses conducted, the principle tool used is ordinary least squares regression. However, given that the regression analyses are conducted over time and space, the analyst should immediately flag the risks of temporal and spatial autocorrelation. If present, such autocorrelation will violate model assumptions of error independence, and hence may cause problems in the interpretation of the results. Apologies if I have missed this somewhere in the SI, but I do not see any noting of either of the autocorrelative problems being acknowledged. If it is the case, it would be a significant omission in the consideration of the analysis, and I think is the —major analytical issue— to be addressed following review. If error correlation over space and time ultimately do not represent an analytical challenge, then the analysis leading to this conclusion should be included (e.g. by presenting the results of a Moran's I analysis).

Page 4. L35. Authors bin the beta values – was this using a mean function?

Page 7. L6. On a separate point, in the discussion the text states: “higher GAM R2 scores in SWA indicate an overall stronger effect of MAP on shaping beta compared to WA”. *Sensu strictu* statistically: the coefficient of determination tells you how much of the variation in the dependent variable is explained by the independent variable; whereas, the effect size is the magnitude of the coefficient on MAP.

Page 7. L15. The W parameter: the purpose of the inclusion of the different W sizes should be better explained, especially given the authors' conclusion that effects of W tell you about the statistical impact of averaging over different time spans, and losing differences between wet and dry periods, rather than any ecological significance. To reveal this as being a statistical artefact in the discussion seemed to undermine the inclusion of this aspect of the analysis. A more positive way to describe this result would

[Printer-friendly version](#)[Discussion paper](#)

probably be that it highlights the importance of partitioning the analysis of responses into dry and wet-season responses.

Page 7. Line 18. The authors mention here local variations in land use. This is an important factor in explaining vegetation patterns across the globe i.e. anthropogenic disturbance. It should at least be acknowledged that there may also be differing disturbance regimes in the two sites, which may be dependent upon human density and predominant modes of agricultural production and management. For instance high human population density combined with high levels of fuel-wood extraction seasonal burning may restrict the growth of perennials and development of grassland into savannah in WA whereas such anthropogenic constraints are fewer in SWA. CIESIN has gridded population data you could check: <http://sedac.ciesin.columbia.edu/data/collection/gp-w3>.

**THE CREATION OF A PROXY VARIABLE FOR VEGETATION PRODUCTIVITY **

Understanding this component of the work is essential to the reader since the derived cyclical fraction constitutes the proxy for vegetation productivity. The concept of measuring values as the integral of vegetation values above a baseline of productivity is straightforward. However, the text in the SI on the details of the work undertaken is quite confusing: “To determine the onset and the end of the CFR of any given year, a baseline is derived, which constitutes the mean upper limit of the dry (or cold) season values between two vegetation peaks. Values above this baseline are part of the CFR. The baseline is calculated using the amplitude between the mean of the four lowest values (“low level mean”) between two peaks and the average of these peaks” (SI pages 4-5). Perhaps a diagram as provided in figure s3 would help the reader here.

Moreover, given the central importance of this step in establishing the dependent variable upon which the analysis depends, I would like to see some more justification of the approach used, and its appropriateness in this instance. I appreciate this is difficult given that the main citation is an article in press. I wonder whether it is possible to

BGD

Interactive
comment

Printer-friendly version

Discussion paper



get an author's draft to circulate amongst reviewers? For instance, given that the central question of the paper is examining responses to rainfall variability, are the authors not concerned that the linear interpolation of outliers is removing some real variability in the vegetation responses? That is, removal of outliers may be employed as a statistical sub-procedure to remove bias from parameter estimates caused by errors in data collection or data entry by researchers. However, such outlying data points are often real measurements that should be included in analyses. What is the basis for interpolation in this case?

Assorted minor points

Page.2. L18. 'arid-most parts': define with respect to rainfall as is done for the semi-arid regions on the following lines.

Page 3. l23. 'characterised by high inter-annual length of the wet-season variability' : re-order sentence

Supplementary information Figure S6: 'shidting linear...' Spelling. Error also in S7

Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2016-48, 2016.

Printer-friendly version

Discussion paper

