Biogeosciences
Discussions

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Interactive
comment

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

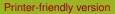**Gregor Ratzmann et al.**

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Dedicated response to Anonymous Referee #1

We would like to thank Anonymous Referee #1 for providing a constructive and helpful review of the manuscript. We believe that the comments helped improving the overall quality of the manuscript. Please find following dedicated answers to each point raised by the referee.

Anonymous Referee #1 (AR #1): 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 least squares being used, with no reference to using a method allowing for errors to be correlated.

Answer: We would like to stress at this point that the study does not involve time series analysis. Temporal autocorrelation by definition is a phenomenon which is limited to time series analyses (relying on parametric methods). Thus, we would like to underline here (and throughout this answer) that temporal autocorrelation does not affect our analyses at any point (see the specific answer to this point raised by Anonymous Referee #1).

AR #1: 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?

Answer: The phenological model used in this study has been tested and compared to modelled NPP Data throughout its development. For a detailed description please see Gangkofner et al. (2015) available on ResearchGate under http://bit.ly/1UfqE3v (we had to shorten the link because it did not fit into the PDF).

AR #1: Finally, although English is not my native language, I believe the grammar in the Supplementary Information could be improved.

Answer: Thank you for pointing this out. We have completely revised the Supplementary Information paying close attention to the grammar and possible other language issues.

AR #1: Page 3, line 22. ". . .higher interannual length of the wet season variability. . ." sounds confusing, please replace by ". . .higher interannual variability of wet season length. . ."

Answer: This has been replaced.

AR #1: Page 3, lines 33-36. Maybe I'm missing something but I failed to understand why dimensionality is an issue here.

Answer: Probably the term "dimensionality" is not clearly leading to the point of this statement. The information we want to convey here is that firstly the NDVI is a unitless variable and d(-)/d(mm) – what ïĄć is in essence – would not be too informative. Secondly, we believe that transforming all values to z-scores helps in comparing the two different regions. We have now rewritten this sentence accordingly and removed the reference to dimensionality.

AR #1: 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.

Answer: Thank you for pointing this out. The missing information has been added.

AR #1: 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.

Answer: This statement in each instance it appears (3 in total) refers to a response function of β to another variable (e.g. MAP). Thus, we decided to exchange the term "β response" by "β response function".

AR #1: Page 4, lines 8-9. Please provide reference(s) or the rationale to justify this rainfall threshold.

Answer: We have added an explanatory sentence for this.

AR #1: 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.

Answer: The product specification has been updated. For the type, please see the manuscript. We explicitly mention it (Type 3, which refers to the LAI/fAPAR classification).

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AR #1: Page 4, line 14. What's "sub-pixel land cover frequency"?

Answer: The sub-pixel land cover frequency is a result of the MODIS land cover classification procedure for MCD12C1 and provided along with the products. It reports the relative frequency of all present land cover classes within one pixel with the most frequent one being assigned to the pixel.

AR #1: 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.

Answer: We agree that temporal autocorrelation is a serious issue for time series analysis conducted using parametric methods. However, the present study does NOT do time series analysis. We compute temporally shifting linear models using OLS techniques, hence we are using parametric methods. Those models, however, use annual rainfall as independent variable and vegetation productivity proxies as dependent one. Thus, neither at the stage of computing those models nor at a later stage time is involved (as variable being used in modelling) in the methodological process of this study. Thus we conclude that temporal autocorrelation is not of concern at any of the analytical steps involved Regarding the number of negative slopes: We added this information to the manuscript.

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

Answer: Thanks, this has been changed accordingly.

AR #1: 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.

Answer: This information is contained in the sentence: "Thus, if MAPi > MAP for any given grid point that cell for time step i was assigned the class "wet", otherwise "dry"." Here, "grid point" and "cell" refer to "pixel".

AR #1: Page 4, lines 33-34. How were the coefficients binned? Averaged? Please clarify and include also this information in all relevant figure captions.

Answer: Thank you for pointing out this shortcoming. The missing information has been added where missing.

AR #1: 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.

Answer: Thank you for point to this missing information. The reason for choosing GAMs is, we were not interested in direct quantitative inference in the sense of deriving coefficients (such as slopes) from the model. Thus, selecting a semi-parametric model (such as a GAM) was deemed appropriate. We added information on error distribution and link function used as well as the specifics of the platform used.

AR #1: 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.

Answer: Thank you for pointing this out. We have rephrased this section.

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

Answer: Table 1 has been rearranged.

AR #1: 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.

Answer: Since ANOVA tests as presented here were intended rather to highlight the differences already observable from the boxplots we completely removed any references to it (p-values) and derived Tukey-test p-values. The corresponding sections in

the text have been updated accordingly.

AR #1: 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?

Answer: Thanks. We added two sentences in the discussion part of the revised manuscript explaining this effect. In essence, we believe that the generally higher values of β for MAP > 600 mm in wet periods are caused by the specific response of savanna vegetation (cf. Fig. 5 and Fig. S4). The relative differences for W = 7 may derive from the rather dampened β values around MAP = 600 (and consequently not from a subsequent increase). This is probably due to the transition region from savanna to grassland around this MAP (Fig. S4).

AR #1: Page 5, lines 27-28. This should take into account the fact that in WA βmax for W=21 (dry and wet) is higher than the corresponding values for W=11 and 15.

Answer: We rewrote the sentence stating that there is no systematic effect of W.

AR #1: Page 5, lines 28-29. Even in WA for W=7?

Answer: Yes, even for this one. However, as we decided to remove the reference to p-values, this significance refers to the interpretation of the boxplot now (cf. Krzywinski & Altman (2014) for a discussion on significance inference from boxplots). Nevertheless, this ANOVA (for WA, W=7, all parametric assumptions being met, including the error term distribution) yields a $p \ll 0.001$.

AR #1: It would be helpful to have a list of non-significant pairwise combinations.

Answer: We agree that this table could be helpful. However, as we decided to omit the Tukey-tests applied the table should not be of interest anymore.

AR #1: 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.

Answer: This sections describes the grass/crop specific response. The fact that all vegetation type specific responses are lower in WA compared to SWA is mentioned in the last sentence of the results part.

AR #1: 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."

Answer: We added "might" making the sentence more suggestive.

AR #1: 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?

Answer: Thank you for pointing to this sentence. We have rephrased it.

AR #1: Page 7, lines 14-15. The effect of W increases in terms of what?

Answer: We changed this sentence which was indeed hard to understand. It states that the stronger the effect of climate (dry/wet) the stronger the effect of W (when comparing the two regions).

AR #1: Page 7, line 16. Can you identify those deviations in Figure 3?

Answer: We have added a sentence mentioning two examples from Fig. 3.

AR #1: Page 7, line 18. Please clarify what you mean with "local differentiation in land use" and what evidence you have to support this interpretation.

Answer: As this point is admittedly rather short in the manuscript thus far, we added some information on the specific land use practices in both regions (for MAP > approx. 400-500 mm (semi-) nomadic livestock keeping in WA and farm-based livestock keeping in SWA and a mixture of crop farming and mainly communal livestock keeping in both regions above those values). We moreover note how this could affect results. We moreover added a sentence indicating the potential impact of different population

densities in the two regions.

AR #1: Page 7, line 39. Please keep the same convention and substitute "upper 10th per- centile" by "90th percentile".

Answer: This has been changed.

AR #1: Page 8, line 2. CVP is given in Fig. 2c.

Answer: The curve of CVP over MAP (which is referred to here) is shown in Fig. 2a.

AR #1: Page 8, line 5. CVS at peak β is not that different between SWA and WA. Maybe you meant CVP at peak β?

Answer: This refers to CVS (as laid out in the next sentence) in the way that peak β seems to converge at similar CVS values.

AR #1: Page 8, lines 18-19. Could the relationship between peak β and MAP be dependent on climate AND soil?

Answer: This is very likely to be the case and the entire paragraph was intended to state this. However, we agree that this point was somewhat hard to get from the original writing. The paragraph is therefore now completely rephrased focusing on precisely stating that peak β is shaped by both, climate and soils.

AR #1: Figure S1 and S2 (and where appropriate), the authors need to include the year or period associated to the MODIS land cover data.

Answers: Thank you for pointing this out, it has been now taken care of.

AR #1: Supplementary SLR analysis, page 7. ANPP was not defined previously.

Answer: This has been taken care of.

AR #1: 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 reference to Figures S5, S6, S7.

Answer: Thank you for pointing this out. We have completely revised this section and replaced misleading figure references in the text.

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