

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

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### Dedicated Responses to Anonymous Referee #2

We would like to thank Anonymous Referee #2 for reviewing the manuscript. Although we regret the general negative nature of this review we appreciate several constructive points of critique which will help improving the readability of the manuscript. Please find following specific Responses to each point raised by the referee.

Anonymous Referee #2 (AR #2): I read through this paper many times since the day I was asked to review it, but unfortunately I cannot understand how their conclusions are supported by their analyses.

C1

Response: We regret that Anonymous Referee #2 does not find support in the results of the study for the conclusions made. However, given the general nature of this statement and the lack of further details on which conclusions are not supported by which results, we regret to note that we are not able to follow the argument.

AR #2: To me, the paper is poor written and the conclusions are NOT supported by their data and analyses overall.

Response: We regret the judgment of the Referee on the quality of the paper. We understand that some sections of the manuscript were written in partly complicated and confusing language. We have thus revised the entire manuscript to improve readability. For a response to the second point raised in this sentence the same applies as stated in our prior response; given the lack of details on which conclusions are not supported by which results it is difficult to engage in a constructive review process in relation to this stated claim.

AR #2: For example, in Abstract, the authors claim “higher rainfall amount variability enhances regional-scale vegetation response to rainfall plasticity and thus dryland ecosystem resilience to dry periods” (lines 26-27 Page 1). I don't find any evidence in this paper showing that, because only NDVI and rainfall data are in this paper and there is nothing that can show it. This is just authors' speculation.

Response: We disagree with this point of critique and if the manuscript is read rigorously it should be clear why: There are several passages throughout the manuscript in support of the statement that “higher rainfall amount variability enhances regional-scale vegetation response to rainfall plasticity” (cf. e.g. page 5, lines 13-14, lines 23-25, lines 27-28; page 6, lines 23-24). Results presented in Figures 3-5 all show exactly that. We are unfortunately not able to follow in which way the fact that NDVI and rainfall data were used should impair our conclusions. We agree that concluding on something always involves a certain degree of speculation which should not, nevertheless, weaken a conclusion soundly based on observations.

C2

AR #2: In the last sentence of abstract, the phrase “recovering from drought” is misleading and over-interpret their results.

Response: We believe that this judgement of the Anonymous Referee is based on a misunderstanding. The fact that the Sahel region has been recovering from a severe drought period during the 1970s and 80s is not part of our interpretation of results presented in this study but scientific evidence (Anyamba and Tucker, 2005; Brandt et al., 2014; Dardel et al., 2014; Herrmann et al., 2005; Hutchinson et al., 2005). This statement puts our results in a broader context. In short it states that although the Sahel is currently recovering, future dry periods might have again severe consequences on affected ecosystems (where only the prediction after the comma is based on our results).

AR #2: Line 11, page 1: “Vegetation net productivity” what is “net productivity”?

Response: “Net productivity” refers to net production normalized by time. In a physiological sense it refers to gross productivity (which is the energy fixed during photosynthesis minus day respiration) minus dark respiration and thus the increment in plant biomass over the normalization time period.

AR #2: Line 27, page 1: what is “rainfall plasticity”?

Response: Thank you for pointing this out. Plasticity does not refer to rainfall but to vegetation response to rainfall. However, as this seems to introduce major ambiguities we decided to now include the acronym  $\beta$  already in the abstract and consequently replacing vegetation response to rainfall plasticity by  $\beta$  plasticity.

AR #2: Line 30, page 3: what is “cyclic part”?

Response: The cyclic part of a temporally continuously measured vegetation index (such as the NDVI) is the part which is measured during a growing season and consequently deviates from the base values which constitute the signal during vegetation dormancy. See Gangkofner et al. (2015) for a detailed description of the procedure

C3

used to derive this cyclic part.

AR #2: Line 22, page 4: “temporal window W”. I would like to add the unit of W here as “temporal window W (years)”.

Response: This was taken care of.

AR #2: Line 27, page 4: I would add “years” after “7, 11,15, and 21”.

Response: This was taken care of.

AR #2: Line 8, page 5: “response function”. It should be “response curves”, rather than “functions”.

Response: Mathematically speaking the term “response function” comprises the term “response curve”, thus we decided to keep the original formulation.

AR #2: Line 23, page 5: “43

Response: Unfortunately, we are not able to understand this comment.

AR #2: Lines 27 29, page 6: the sentence “We have shown that a shifting linear regression model can successfully . . .”. I don’t see this from the results.

Response: As we successfully applied SLRs and the presented results correspond to our predictions we believe that this sentence is well supported by our results.

AR #2: Lines 34-35, page 6: the sentence “ Moreover, SWA shows . . .” needs to be reworded. And “hydroclimatic periods” is too ambiguous. I’d rather use “dry vs. wet periods” directly.

Response: We rewrote this sentence. It reads now: “Moreover, response functions in SWA show a stronger difference between hydroclimatic periods (dry vs. wet), have clearer unimodal shapes along the MAP gradient and possess higher spatial variability”. We decided to keep the term “hydroclimatic period” as it is introduced on page 3, lines 1-2 and used throughout the manuscript.

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AR #2: And, there are many ambiguous terms throughout this paper, for example, “beta plasticity” in the next line. What is it?

Response: We believe that the term “plasticity” is part of the common language found in numerous scientific textbooks and articles throughout all relevant fields (ecology, earth sciences, remote sensing and so on). Thus, we omit an explicit definition in the manuscript. In general, plasticity refers to the variability in any response variable as a function of different environmental conditions. This can also comprise interactions as described here. For example, if differences in a response variable (here  $\beta$ ) between two conditions (here, dry and wet) are different between two locations (here SWA and WA) this suggests differences in the plasticity of the response between the locations with respect to those conditions.

AR #2: Lines 8 15, page 7. This paragraph is to explain why the slope beta changes with the temporal window (W). But I still cannot understand it after reading it.

The authors should explain it clearly because the major results of this paper relate to the temporal window. I’m confused at it when reading this paper because of the temporal window.

Response: This paragraph explains the technical background of the effect of different temporal SRL window sizes on the results. But, as we conclude at the end of the paragraph, those effects are rather small and do not have an specific ecological meaning. In plain terms, the diminishment of differences between wet and dry in the response functions for increasing W in SWA is a product of the SRL procedure. Assume that  $\beta_i$  is derived from an SRL computed over period i and  $\beta_j$  is derived over period j (and  $j=i+1$ , thus with only one year difference in the input time series). Let us further assume that i is assigned the class “wet” and j is assigned the class “dry”. Then – given a difference between  $\beta_i$  and  $\beta_j$  – this difference will decrease with the amount of years feeding into each SRL (and consequently  $\beta$ ). This is simply due to the fact that the importance of the one-year difference decreases with increasing W. This is what is stated in this

C5

paragraph in an admittedly condensed way. The second statement made by Referee #2 at this point appears to be a misunderstanding. The only way in which the temporal window is related to our major results is the fact that it hardly affects them. Thus, we conclude that this paragraph is necessary to explain the effects of W but (given their minor dimension) not to over-emphasize on this point.

AR #2: Line 42, page 7: “hydroclimatic control” What is it? If it is rainfall, just say “rainfall”.

Response: We have rewritten this sentence. It reads now: “This suggest a high sensitivity of absolute  $\beta$  values to above and below average rainfall conditions in SWA.”

AR #2: Line 1, page 8: what is “systematic response” here? I don’t think the authors have done anything related to “systematic”.

Response: We have changed this part of the sentence for clarification. It reads now: “The systematic response of SWA  $\beta$  response curves to hydroclimatic conditions, contrasted with the absence of a similar pattern in the WA case, . . .”.

AR #2: Lines 1 3, page 8: this whole sentence needs to be reworded.

Response: Please see our previous Response. Further, we are unfortunately not able to see in which way Anonymous Referee #2 suggests to rewrite this sentence.

AR #2: Lines 4 15, page 8: I cannot understand this paragraph.

Response: This paragraph explains the differences of peak  $\beta$  with respect to the position on the MAP gradient, the rainfall amount variability gradient and the season length variability gradient. It moreover comments on the small differences in this position for the season length variability gradient compared to the other two positions (MAP and rainfall amount variability). Although we believe that this paragraph is exactly reporting what we have rephrased here and we consequently decided to keep it as is in the manuscript we spotted a typo in this paragraph (line 7) which has been corrected.

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AR #2: Line 23, page 8: the phrase “grass and crop type vegetation” is weird. I prefer “grasses and crops”.

Response: We unfortunately do not fully understand the term “weird” in this context. The term “grass and crop type vegetation” refers to the classification scheme of the MODIS MCD12C1 product. We consequently decided not to change this term.

AR #2: Lines 26, page 8: this claim “. . . further support the finding that . . .” is not supported by any data in this paper.

Response: We believe that this entire sentence is well in line with our hypotheses and findings reported earlier in the manuscript (cf. Figures 3 and 4 and corresponding result sections). It is, however, unfortunately not entirely clear which part of this sentence the Anonymous Referee #2 deems not supported by the results.

AR #2: Lines 28 42, page 8: this paragraph should be re-written and the conclusion in this paragraph is not supported by their results.

Response: It is unfortunately not entirely evident from this statement in which way the referee suggests to rewrite the paragraph. Moreover, Referee #2 does not provide details concerning the statement that our interpretation is not supported by our results. Given the relatively detailed nature of this paragraph, we are unfortunately not able to provide any argument in further support of the points provided in the manuscript.

AR #2: Lines 8 9, page 9: This sentence describes vegetation differences in these two regions. And I expect to see the explanations of how different vegetations affect NDVI responses to rainfall. But I didn't see it. So, it doesn't explain anything.

Response: There appears to be a misunderstanding. This sentence is not of explanatory nature with respect to the vegetation specific  $\beta$ . It provides a potential explanation how differences in rainfall variability may affect the relative abundance of different vegetation types (and thus differences in number of data points in Figure 5).

AR #2: Line 26, page 9: “recovering from severe drought periods of . . .” is misleading,

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because it implies a role of previous drought and the processes of vegetation recovery. But these points are not discussed before and they should not be in the “Conclusion” section.

Response: Thanks, we have added a sentence in the discussion which addresses this issue. We agree that this fact should have been mentioned before (besides being mentioned in the Supplementary Material).

AR #2: Lines 28 29, page 9: the claim “less susceptible to changes in water availability given its widespread relatively high beta values”. Why? “widespread beta values” can be a proof of high sensitivity.

Response: Indeed,  $\beta$  can be interpreted as vegetation “sensitivity” to rainfall as well. Yet, whether  $\beta$  is named sensitivity or response, high values during dry periods indicate a relatively low susceptibility to temporally decreased water availability.

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