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Interactive comment on "Primary productivity and its correlation with rainfall on Aldabra Atoll" *by* J. Shekeine et al.

Anonymous Referee #1

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1 Summary

Shekeine et. al. present a study where they study the relation between rainfall and primary productivity (represented by NDVI, a satellite derived proxy for vegetation greenness and indirectly productivity) on the Aldabra Atoll. First they study the long term (1969-2012) trend in rainfall and conclude that it is downwards by about 6mm/yr. Nevertheless it is clear that this trend is very small relative to the interannual variability (400-500mm). Second, they study the correlation between rainfall and NDVI for the 2001-2012 period, by analyzing the i) time series correlation and ii) correlation between phenology metrics derived from the TIMESAT software. Based on these correlations they draw conclusions about the future of the endemic species of the Atoll. Additionally,

the study present an analysis of tree rings which appears useless to draw conclusions about the long term productivity.

2 General comment

Although the subject of this study is very interesting and merits large scientific/societal interest, the analyses are poor (see my specific comments) and never support any causal relationship between changes in productivity due to changes in rainfall. Moreover, the study draws several conclusions that are not supported by the results. Finally, I believe that the study is little innovative (e.g. Vicente-Serrano et. al. (2013) have done something very similar but with a global focus and not zooming in on the atoll). Consequently, I recommend not to accept the paper. Only when the authors manage to adapt their paper, which basically would require to rewrite it completely with several additional analyses, I think it could be published eventually.

3 Comments

Title: "Primary productivity" is the main word in the title, whereas the paper never mentions primary productivity in the main text (except when discussing the work of others in the introduction (p983,L24) or to introduce the tree ring results (p984,L20), which later appear useless to study productivity trends). I believe the use of the MODIS GPP product would provide much more insight in the primary productivity than the NDVI, which is only a proxy of greenness.

Abstract: L4 Not only the trends are important. The variability is much more important from an ecological perspective (see later)

Introduction: p983,L1-p984,L9: I think this part of the introduction is way too extensive

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relative to the other parts of the paper. It gives an introduction of the importance, but this could strongly be condensed to half of the length. Additionally, I think the introduction is lacking information to put the study into context.

* For example, conclusion, methods about other studies that relate vegetation response to drought are never mentioned (although they are often much more advanced than the approach used in this paper: e.g. De Keersmaecker et. al. (2015) http://dx.doi.org/doi: 10.1111/geb.12279 or Vicente-Serrano et. al. (2013) https://www.pnas.org/cgi/doi/10.1073/pnas.1207068110).

* Also, lack of rainfall is not water stress. Therefore, rainfall data should be to drought and water stress (e.g. Vicente-Serrano et. al. (2010), http://dx.doi.org/doi:10.1175/2009JCLI2909.1). All this context is however missing in the introduction.

p984,L16-22: I think that by putting so much focus on the aim to use tree ring data, which later are useless, the paper gives a false impression to the reader. I would put all tree ring analysis information into the suppl. material and mention the limited information from tree rings in the results only. This will not give the reader the impression that tree ring analysis will reveal something about productivity trends, which it will not.

Section 2.3 See previous comment

p988, L16-21: Uncertainties in the SOS and EOS can have large consquences for the later analyses. Was any sensitivity analysis performed to assess the effect of different TIMESAT setting on later analyses? I think it is important to analyse and mention the uncertainties as a result of this.

p988L21: What is the best result? Was there any objective criterion for that? Or was it just visual inspection? Moreover, I am not completely convinced about the absolute accuracy of the method. E.g.in Fig4 the minimum albedo in winter 2006-2007 seems largely overestimated as the downward 'outliers' do not seem clouds (in comparison

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with the other NDVI values). I realize that it is not easy to have a good TIMESAT fit if there is only a very short 'dry period' (in combination with the dual peak in rainfall). Therefore it is of utmost importance to take the uncertainties due to TIMESAT along in the further analyses.

p988L21: the effect of noise can have strong effects on the later cross-correlation analyses (e.g. Lhermit et. al. http://dx.doi.org/10.1016/j.rse.2011.06.020.) Understanding the noise is therefore also important to understand the uncertainties and its effect should be included as an uncertainty in the analyses

p988L26: Was TIMESAT fit on all individual pixels? Or was it fit on the mean of all pixels? The paper should do the former as otherwise the final NDVI is just mixing all different ecosystems dynamics into one signal.

p989L12-17: Does it make sense to fit a smoothed curve on the rainfall series? I am not convinced and the reason for this is also clearly visible in Fig.4, where the EOS was determined wrongly for 2 of 12 years (!). This makes me doubt severely about the accuracy of the length of rainfall season analysis for all subsequent analyses.

p989L20: Was the correlation calculated between the raw NDVI and rainfall series? Or between the TIMESAT smoothed curves? I would strongly recommend the former as otherwise the TIMESAT uncertainties will be taken along.

Section 2.5 By calculating the cross-correlation on the complete time series it is implicitly assumed that the vegetation response to rainfall in the beginning of the growing season is similar to the response at the end of the growing season. This is, however, clearly not the case. For example in Fig.4 it is clear that the green-up after the first rains is much quicker and direct than the senescence phase at the end of the rain season. This difference in response, however, is never take this into account, but this will have strong consequences for the interpretation of the response of vegetation to rainfall.

Section 2.5 By correlation anaylisis between NDVI and rainfall, it is assumed that rainfall

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in the wet season has a similar effect as rainfall in the dry season. This is however not the case as small rainfall amounts in the beginning of the growing season, when there is still some 'water stess', will have a much stronger effect than the same small amounts in the middle of the growing season when there is not water stress at all. Therefore, the paper should work with rainfall anomalies and not rainfall amounts (e.g. De Keersmaecker et. al., http://dx.doi.org/doi:doi: 10.1111/geb.12279) and rainfall should have been converted to drought indicators or any other measure of water stress (e.g.Vicente-Serrano et. al. (2010) http://journals.ametsoc.org/doi/abs/10.1175/2009JCLI2909.1).

Section 3.1 The downward trend of -6mm/yr is very small relative to the interannual variability, which seems in the order of 400-500mm/yr. Therefore, I have my severe doubts that the slow trend will be determining the future of the endemic species. I don't think that species response is related mean rainfall. They will be much more affected by extreme dry years or the sequence of dry/wet years than by a relatively small trend. The paper, however, does not discuss or analyze any of these variability effects and bases all its conclusions on a mean trend. To overcome this issue, much more complex studies should be performed that account for both the trend and the interannual differences (e.g. Guan http://dx.doi.org/doi:10.1016/j.agrformet.2014.03.010 which use EEMD as a method to overcome this issue) and the effect of changes in variability should be included (e.g. Piao et. al. http://dx.doi.org/doi:10.1038/ncomms6018.).

p992L7: these minima are very sensitive to the accuracy of the TIMESAT method. See my earlier comment, where I don't trust the minima in the winter 2006-2007 in Fig.4

p992L11-19: I think the interpretation of these lags is very doubtful if there is no discrimination between the greening response (which is much faster) and the browning response (which is slower)

p992L22: 'and their sensitivity to water stress'. I don't agree. Rainfall is not water stress!

p992L25: 'suggesting the trotoise turf are particularly sensitive to changes in rainfall'.

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I don't agree. The NDVI of tortoise turf is more strongly correlated to rainfall, which is logical as it is a more deciduous. There is however no clear sign of a causal relationship (i.e. still low R2). Moreover, other vegetation types may be more sensitive to rainfall, but perhaps it is not shown by their NDVI as a large NDVI response is not necessarily an indicator of sensitivity.

p993: excluding 2007 seems a lot of cherry picking to me. Moreover, based on this logic the paper should also exclude 2006 as 2006 is also bimodal in rainfall. Moreover, I think the methodology is far from sound if two of 12 years need to be excluded because of methodological issues.

p993: In general I think the correlations are small in combination with the large methodological uncertainties. Therefore, I think it is difficult to draw any conclusion from them.

p994: How accurate are the mininum NDVI analyses if uncertainties are already apparent in Fig.4?

p994L5: given the low correlations and lack of any proof of causal relationship, I think this evidence is very small.

p994L20-p995L9: I agree that there must be concern, but I really doubt that a small trend in mean rainfall will determine the future of the tortoise population. Several other factors (interannual variability, etc.) are much more important, but the paper fails to address all of these. This also shown by Bourn et. al. who mention that two consecutive dry years are strongly responsible for the decline in tortoise population. This is something completely different than a small decline in mean rainfall! Therefore, I do not agree with any of the papers conclusion on the future of the tortoise population.

p995-23-p996L11: I think most of this information is not relevant in relation to the paper's analyses and highlights the very limited contribution of this paper to understanding the effective response of vegetation on the Atoll to water stress (rainfall, not drought; raw data, no anomalies; all year data, no difference between start/end growing

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season).

p996L15-21: The ‘strong’ correlation does not explain anything about causal relationships (certainly not if 1 of 12 years was removed and the other one should have been removed as well; or both not). Moreover, this correlation is not that strong (only significant at one of the four study areas).

p997L6: strong coupling. I don’t see this strong coupling. By looking at mean and maximum NDVI the paper does not assess at the coupling of dry periods, but only at wet periods, whereas Bourn et. al. have highlighted the importance of consecutive dry periods.

p997L14: tortoise are not sensitive to mean annual rainfall . To understand the vulnerability the paper should investigate the importance of changes in extremes and variability.

Interactive comment on Biogeosciences Discuss., 12, 981, 2015.