

Interactive comment on “Assessing vegetation structure and ANPP dynamics in a grassland-shrubland Chihuahuan ecotone using NDVI-rainfall relationships” by M. Moreno-de las Heras et al.

Anonymous Referee #1

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This is an interesting study aiming to model the contribution of shrubby and grassy patches to the yearly time course of MODIS-NDVI in drylands and then to use remotely-sensed proxies to quantify the Aboveground Net Primary Productivity (ANPP) of each Plant Functional Type (PFTs) over the last 13 years and across a 18km² area in the Chihuahuan "desert". The context of the study is that of a well-documented conversion from grasslands to shrublands over the last century triggered by a combination of over-grazing, summer drought and modification of the fire regime, and that of the impact of these vegetation changes to ecosystem functioning. This is clearly stated in the paper.

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Overall, I find the analysis looks like a long and winding road. Essentially, authors have to solve an inverse problem. They assume that the MODIS-NDVI signature at a 230m resolution results from the growth response of a mosaic of two PFTs: shrubs and grasses. Given a growth response model for these two PFTs, the NDVI time series is decomposed in two parts and the contribution of each PFT is estimated. To solve this problem, I think a more straightforward approach could be used. First, I would have slightly refined the two differential equations (eq. 1 and 2) capturing the key ecohydrological processes of the system (see below). Second, I would have extracted the NDVI time series from a set of calibration sites where cover of each PFT is known (to be chosen among the 27 sites) and used these remotely-sensed data to optimize the few parameters governing the plant biomass dynamics (eq. 1). Third I would have assessed the performance of the model when applied to the entire area.

Now the main concerns I have with the approach followed by the authors are detailed below.

(i) Model structure. The low-dimensional model coupling plant biomass and soil moisture (eq. 1 and 2) falls a bit short to capture the key ecohydrological processes that control ecosystem response in these drylands. First, the lag between the pulse of resource (rainfall event) and the production of biomass is an important parameter to contrast shrub and grass response as thoroughly discussed in the paper. So I do not understand why this parameter is estimated in a second step - i.e. by correlating simulated biomass dynamics and observed rainfall data - and not included in the model structure. I am not convinced that the growth rate and the mortality rate are per se sufficient to properly estimate this lag. Second, rain use efficiency is very dependent on the timing of rainfalls as illustrated in the last part of the paper that distinguishes monsoonal and non-monsoonal rainfalls. The model structure neither accounts for this. Third, differences between shallow-rooted grasses and the complex rooting systems of shrubs lead to more or less flexibility in using different water sources. In this context, a one-layer soil model with similar threshold values for grass and shrub biomass production

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looks like a strong simplification. Finally, given the linear relationship between ANPP and integrated NDVI over the growing season (Fig. 5), equation 1 might be viewed as a prognostic model of NDVI. This should be clearly explained to connect this model with the rest of the paper.

If I understood well, this model was not calibrated with biomass data and so mortality and growth parameters were retrieved from published data. I wonder why authors did not use published values for the other parameters they do not include in the model (i.e. the delay effect).

(ii) There is some disconnect between the ecohydrological model (eq. 1 and 2) and the model of NDVI decomposition (eq. 3). Although the ecohydrological model highlights the key role of soil moisture dynamics the rest of the paper only focuses on rainfall data and on NDVI-rainfall relationship. Why? Soil moisture dynamics is like a low-pass filtering of rainfall data and is more informative to model biomass response compared to rainfalls. For example, winter-spring recharge is probably essential to explain the spring growth of shrubs. The key point here is that I am not convinced that the so-called "Arain" function captures adequately the growth response of individual PFT and its coupling with soil moisture dynamics. For example, in the creosotebush core sites, the adjusted NDVI-rainfall relationship is somewhat biphasic and seems to capture both grassland and shrub responses (Fig. 3B). I also wonder if the persistence of high NDVI in shrublands during the dry period is adequately captured by the model (it seems this is more pronounced in observed data (fig. 3A) compared to the simulated data fig. 1A). In short, I do not have the feeling that the NDVI decomposition based on these Arain empirical curves is the best option for the coherence of the paper.

(iii) The performance of the model decomposing NDVI time series should be clearly presented. How much of the observed NDVI variance is captured by model 3? In addition, an examination of the magnitude, spatial and temporal distribution of the residuals should be conducted. I also would like to know the sensitivity of model outputs to parameter uncertainty (growth rate, mortality rate, lag)?

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Other comments.

- Is there any significant change in the cover of shrubs over the examined period? Is the proposed method able to track these changes in areas where significant shrub encroachment has been reported over the last decade?

- another way of calibrating and/or validating model 3 is to use high resolution imagery and apply segmentation methods to precisely estimate the cover of shrubs. This could be discussed.

- a very simple and empirical way to classify land cover based on NDVI time series is to conduct an ordination or a partitioning of the matrix of correlation between NDVI time series. I would be curious to compare the outcome of this analysis with that shown in figure 4.

- is the change of ANPP along the ecotone consistent with the decrease of ANPP that has been associated with shrub encroachment in dry areas (Knapp & al. Global Change Biology 2008)?

- is the year-to-year variability in ANPP higher for grass-dominated sites? coefficient of variation in yearly ANPP along the ecotone could be reported.

- to complete figure 5, add panels showing the relative contribution of grass and shrubs to total ANPP. This figure could be divided into two.

- Figure 1 should present the soil moisture dynamics.

Typos.

Capital letters for panels in legend of figures

"Variables" in fig 4B

Figure 5C and 5B should show increasing values of ANPP from left to right in the color palette

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