

Response to Interactive Comments, Dec 2020

CX: Reviewer comment number X

RX: Author response to reviewer comment X

(All the lines are referred to the revised manuscript.)

C1 The manuscript by Hao et al. relates a data set of burned areas to potential driving forces like the development of livestock population and droughts. It is an interesting topic for Biogeosciences and beyond. However, I have the impression that it just scratches at the surface of the topic in a very general manner and that the whole analysis is oversimplified. E.g., when describing the burned area data set, the authors refer to previous work without briefly explaining the underlying method with few sentences.

Similarly, for the drought index and biomass data sets the underlying methodology is not explained or even mentioned. This is absolutely necessary to evaluate the results of the analysis. The authors lay more emphasis on mentioning where the data can be downloaded and which R packages are used than describing the origins and the functions itself. Also the statistics is oversimplified. The discussion replies well-known facts rather than really going into the details of the selected data sets and their links and feedbacks. One example: “In our study, we showed the strong impact of political events (here the collapse of the political regime) on grazing intensity and the subsequent effect on fire activity.” The study period was 2002-2016, the political changes they refer to occurred in 1990/1991. In the conclusions just bullet points with statistics are given rather than a real interpretation of the results and maybe an outlook. From this general evaluation I recommend rejecting the manuscript. More detailed comments can be given as soon as the main issues are solved.

R1 It is not correct to say the drought index and biomass data sets were not mentioned. We state: “The Palmer Drought Severity Index (PDSI) from the TerraClimate site (<http://www.climatologylab.org/>) was used...”.

To further clarify we added the citation for the **climate data** developed on the TerraClimate site. If readers are concerned about how the climate data are created they should investigate this publication. It is my feeling that going into details about the formulation of climate data is tangential to the thrust of the present publication, especially since the reader can examine all the details of the formulations in the reference. The reference is:

Abatzoglou, J. T., Dobrowski, S. Z., Parks, S. A., and Hegewisch, K. C.: Terraclimate, a high-resolution global dataset of monthly climate and climatic water balance from 1958-2015, *Sci Data*, 5, 170191, <https://doi.org/10.1038/sdata.2017.191>, 2018.

Lines 140 to 157 Regarding to **biomass**: We rewrote this section to be clearer and added a more detailed formulation and a citation.

We estimated the annual biomass production within the grassland domain of the study area (Fig. 2) using the production subroutine of the Rangeland Vegetation Simulator model (RVS) (Reeves

2016) which applied the methods of Reeves et al. (in press). The RVS, which was originally developed for simulating rangeland vegetation dynamics in the continental United States, models annual production based on MODIS normalized difference vegetation index (NDVI) at a 250 m spatial resolution (MOD13Q1). The MOD13Q1 NDVI data are composited on a bi-weekly basis and are available at a spatial resolution of 250 m. The QA/QC flags were used to isolate only the best quality NDVI pixels. At each pixel, the highest quality maximum value composite on an annual basis was retained for further analysis. The relationships between ANPP estimates and maximum NDVI were divided into two groups to enable different models to be fit to the lower and upper end of production given as

$$y = 240.31 * e^{3.6684 x} \quad (1)$$

where y is the estimated ANPP in kg ha⁻¹ of dry weight and x is the NDVI for the upper range (x ≥ 0.46) and

$$y = 971.1 * \ln x + 1976 \quad (2)$$

where y is the estimated ANPP in kg ha⁻¹ and x is the NDVI for the lower range (x < 0.46). The partition into 2 groups was done, in part, because of the asymptotic nature or “saturation” feature (Santin-Janin et al., 2009) of NDVI with respect to ANPP.

Lines 172-174 and 180-182 The R packages and methodology were rewritten.