Response to Reviewer's comments (RC1)

CX: Reviewer comment number X

RX: Author response to reviewer comment X

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

Abstract

C1. This is a very good paper that elucidates interactive effects of climate, vegetation, and land use in northern Eurasia, which as a significant impact on human health and global change impacts. Contrasts in responses in Kazakhstan relative to other regional responses help elucidate the complex interactions.

C2. The final sentence indicates large potential impact of the findings in this paper which are not fully developed in the discussion. The need to incorporate the human-management influence in global models is important but the paper doesn't really elucidate how this would be accomplished.

R2. The final sentence was deleted. It is not the main objective of the manuscript to predict future fire dynamics and emissions in northern Eurasia.

1. Introduction

C3. This paragraph needs to make a clearer statement of the core objective(s) of the paper. There are many methodological details here that are suited to the next section where they are explained more clearly. The last two sentences in this paragraph are more suited to results and discussion sections.

"To disentangle keystone variables affecting fire activity, we examined the trends of the spatial and temporal distribution of the area burned from 2002 to 2016 across different land cover types and geographic regions of northern Eurasia. Daily NASA MODIS (Moderate Resolution Imaging Spectroradiometer) dataset at a 500 m × 500 m resolution was used. The burned area data were analyzed at multiple spatial and temporal scales using frequentist statistical methods to identify the regional trends. We identified the geographic region with the largest declining trend and explore the influence of the confounding factors of climate and human activity on burned area in this region. Assessing burned area changes in northern Eurasia over this time period benefits from the lack of fire suppression in this region (Goldammer et al., 2013), so the impact of climate and land use on fire activity can be better understood."

R3. The objectives were specified in:

(1) Lines 59-61 We will investigate trends in the spatial and temporal distribution of area burned from 2002 to 2016 across different land cover types and geographic regions of northern Eurasia, a region highly sensitive to climate change;

(2) Lines 64-66 to evaluate the decline in burned area as a function of variable fuel conditions (Krawchuk and Moritz 2005), land use and relative moisture conditions (Pausas and Ribeiro 2013), and abrupt changes such as grazing;

(3) Lines 76-78 We closely examine the interactions of climate, fire, grazing and fuel availability in Kazakhstan, the country of northern Eurasia with the largest decline in burned area during 2002–2016.

2. Methodology

Lines 96-100 Description of MODIS dataset and its analysis was moved to section 2.2 Methodology – Mapping burned areas. We used daily NASA MODIS (Moderate Resolution Imaging Spectroradiometer) dataset at a 500 m × 500 m resolution. Our MODIS-derived burned area algorithm was validated in eastern Siberia with the Landsat derived burned area (30 m × 30 m) (Hao et al., 2012). The ratio of these two satellite derived burned areas was 1.0 with a standard deviation of 0.5 % over 18,754 grid cells.

Lines 104-107 Lack of fire suppression was moved to section 2.2 Methodology – Mapping burned areas. Assessing burned area changes in northern Eurasia over this time period benefits from the lack of fire suppression in this region (Goldammer et al., 2013), so the impact of climate and land use on fire activity can be better understood.

2. Methodology

C4. It would be helpful to start the methodology section with a description of the study area and why contrasts between Kazakhstan and other parts of the region offer such a unique opportunity to study the interactions of interest. Figure 2 could be discussed at this point. A brief description of the growing season and common management of grazing systems could be provided here to set the stage for later discussion.

R4. Lines 86-90 To understand the forces driving the decline of burned area, we focus on the effects of drought and grazing in Kazakhstan. From 2002 to 2016, Kazakhstan had the highest rate of decline in burned area in northern Eurasia (see Figs. 1 and 2). In Kazakhstan, grassland is the dominant ecosystem and grazing is the major agricultural activity (Food and Agriculture Organization FAO Live Animals Database, 2016).

C5. The data sets selected and statistical approaches seem appropriate to the objectives of the authors.

R5. We agree with the reviewer.

C6. The results section, figures, and tables, including the supplemental section, clearly present the results of the analysis.

R6. We agree with the reviewer.

4. Discussion

C7. Overall the discussion provides insight into the results. The discussion of modeling needs to be clarified as discussed in comment below.

Grassland fires and grazing

C8. Some information in this section could go into a description of the study area suggested for Methods section. For instance, that the fires are intentionally caused as part of grazing management and the perhaps the impact of the Soviet collapse on policy and agricultural systems as it influenced conditions during the period of analysis.

R8. The information presented in this section was the results of analysis. It should belong to here, not in the study area. It follows the logic and flows better in the manuscript. We studied Kazakhstan initially because it had the largest decline of burned areas, not because of Soviet collapse and its agricultural systems (e.g. grazing). These two events were the explanations of the decline of burned areas

C9. Annotate Figure 7 to show some of the key points discussed here - e.g., economic collapse/livestock reduction in 2000

R9. Figure 7 We added the major political breakpoints and livestock changes.

Modeling

C10. The manuscript makes a case for the importance of fire in many ecological processes important in the global system and the gaps in the models. The last sentence of abstract implies the findings may improve modeling of the interactive effects but this discussion indicates the difficulty of modeling the stochastic effects. Would modeling based on alternative scenarios related to policy and management be the best way to capture this?

R10. The last sentence of the abstract was deleted. Our intention was mostly to provide to the reader the demonstration that grazing (a) can be highly variable as a fast response or abrupt change in agricultural policies or political regime, and (b) these abrupt changes can have a significant impact on fire activity. When DGVMs try to benchmark their outputs on observed burned areas, they might be aware of some key processes (here grazing) that might have affected burned area variation, and figure if their models account for this process or not, so that any potential mismatch might be due to this missing process. We then specify that grazing is partly inserted in FIRE-DGVMs and suggest that should be better integrated regarding the significant impact on burned areas. We however acknowledge the difficulty in predicting political collapse in future IPCC scenarios, as well as wars or conflicts that might hardly affect socio ecosystems. We did not intend to solve this problem but warn the DGVM community to try and capture this important process.

Lines 391-396 Our study demonstrates that grazing can be highly variable as a fast response or abrupt change in agricultural policies or political regime. These abrupt changes can have a significant impact on fire activity. Better integration of human process on grazing activities in DGVMs, even as stochastic events, would capture this important process to account for probable political collapse/agricultural policies, societal decisions or widespread animal diseases. These improbable factors could affect future global carbon budget.

C11. Delineate the regions summarized in Table 1 on this Figure 2 map.

R11. Figure 2 is very busy and the region in Table 1 may not show well in the map. However, a sentence is added in the caption, “The border of Kazakhstan is also illustrated in Figure 1.”

C12. The difference in scales make it visually appear similar. Maybe note in the legend that the scales differ by order of magnitude or plot both on single graph with exponential y-axis.

R12. A sentence is added to illustrate the scale of two figures. "Note: the scale of burned area (y-axis) in Kazakhstan (a) is 10 times greater than that in Mongolia (b)."

<u>Response to Reviewer's comments (RC2)</u>

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Abstract

C1. **Line 23** First sentence needs an adjective like "current" unless the authors want to refer to a much more general assessment than the response of Northern Eurasia to current climate change.

R1. Line 23 "<u>currently</u>" was added to the first sentence.

C2. Line 32 I would not use the verb "may" in the abstract: either use "can" and develop this assessment in the paper or skip this assessment in the abstract.

R2. Line 32 Change "may" to "can". **Line 33** The last sentence in the abstract was <u>deleted</u>.

Introduction

C3. Lines 46-56 I think that the terminology "warming hiatus", although coming from IPCC, might be confusing for readers that are not in the topic. Several authors prefer the term slowdown, for example references [7, 2, 3]. Unless that the authors have a given opinion on this, in line 50 they could better indicate as hiatus/slowdown and, possibly, they could include relevant references that used the term slowdown.

R3. Lines 47-49 <u>Slowdown</u>" is used with the addition of the reference, Fyfe et al. 2016.

C4. Lines 46-56 Here also is interesting for potential readers to comment about hemispherical the differences on this "warming hiatus/slowdown", as it seems important in the context of the paper. The last the version of the dataset Had-Crut (see figure 1 here included) highlights these aspects and the global mean increase of temperature is tempered by the Southern Hemisphere but the Northern Hemisphere has a more clear warning signal at the period analyzed in the paper.

R4. "<u>Slowdown</u>" is used as R3.

C5. Line 53 I would write here something "geographical components" if the authors are referring to this. Otherwise, the sentence may indicate divergence of variable inputs to calculate the FWI (that might be or not also the case).

R5. **Line 52** "<u>Geographical</u>" component was inserted.

C6. To remark about the sensitiveness to current climate change of Northern Eurasia, here is a good point to add recent references to this aspect [6], or any other that the authors consider descriptive. It would support the first assessment of the abstract.

R6. Lines 55-57 The reference of "Sato and Nakamura, 2019" was added on the current climate change in Northern Eurasia.

C7. Lines 82, 83 This is a key fact in the context of last sentence of the abstract and could be mentioned on the discussion about modelling.

R7. Lines 364-396 The impact of climate, land use and humans on fire activity was described in the modeling section.

Methodology. Mapping burned areas

C8. Lines 88-89 I would recommend a better link of this aspect about uncertainties with the validation done by (). Also note that it seems that there is an improvement in the use land cover from [5] to this manuscript. However, is the validation method conditioned by the differences in land cover datasets used?

R8. Lines 97-100 Regarding to **uncertainty**, our previous work is added: Our MODIS-derived burned area algorithm was validated in eastern Siberia with the Landsat derived burned area (30 m × 30 m) (Hao et al., 2012). The ratio of these two satellite derived burned areas was 1.0 with a standard deviation of 0.5 % over 18,754 grid cells.

Lines 101-102 Surface and crown fires generate significantly different spectral signals, so that the fire detection algorithm depends on vegetation type classification (Chuvieco et al., 2019).

Validation of burned areas (Hao et al., 2012) did not depend on the land cover maps used.

C9. Lines 90-95 I consider a bit confusing these sentences. In particular if "This study used" are referring to the previous [5, 4] studies or the current manuscript under review. I recommend rewriting these sentences and being more clear "This study uses : : :" "That study used" or directly "[5] used: : :" to be sure that the reader is not lost.

R9. Lines 108-112 It was rewritten: "For this study, an up-to-date land cover product was used for 2002–2013 and the 2013 land cover map was used for 2014–2016 because current versions were not available for present and previous studies. For the study of Hao et al. (2016a, 2016b), the MCD12 land cover map of 2015 was used for 2002–2016.

C10. Line 95 This dataset no longer available. Is that used from previous studies [5,4] or also for this one?

R10. Line 110 "Current versions of land cover maps were not available for present and previous studies" is added.

Data sources. Land Cover

C11. Maybe add a comment about consistency in the products of Land Cover here mentioned (MOD12) and those of section 2.1. I understood well, that they are from same sensor but with different retrieval algorithm? Is it important a degree of consistency?

R11. Data consistency is important for studying trends. The MODIS product was updated from time to time with updated retrieval algorithm. The original dataset from 2002 to present was reprocessed with updated algorithm. Therefore, the entire dataset was consistent for different years of 2002 – present but may vary from different versions of the product. Reprocessing of the land cover product was described by Friedl et al. 2010 ten years ago. This publication was also referred in line 85. It is therefore **not** necessary to add these comments because the MODIS reprocessing scheme was well known in the MODIS community.

Statistical Analysis

C12. The M-estimation is often used to avoid that outliers condition the result.
Was this a preventive decision or actually the dataset has outliers? Probably here the authors can refer already to the Figures when describing methods: annual trends Figure 2, and rank correlations Figures 5 and 6. Here also when it is indicated the validation of the estimation of burned areas, the authors may add also that it is shown in Figure 3. This helps readers.

R12. Lines 167-172 Our objective was to present consistent grid cell trends in the presence of within-cell variation. We chose to use M-estimation to mitigate the effect of large within-cell variation due to a relatively small within-cell sample such that the map presents a consistent surface. If computed using ordinary least squares (OLS) estimates, such large within-cell variation could result in some cells with inconsistent or "outlier" trends compared to their neighbors.

C13. Line 159 Any particular reason for gamma distributed response or previous studies that used this hypothesis?

R13. Lines 179-182 We applied the correct distribution to the data instead of a normal approximation. A theoretical gamma distribution is defined as having support for $y>0$ and often skewed (Mood et al., 1974). The gamma distribution is therefore characteristic of the burned area data. Use of the data-appropriate distribution provides for more accurate estimates and confidence bound.

C14. Line 169 Any particular reason for beta distributed or previous studies that used this hypothesis?

R14. Again, we applied the correct distribution to the data instead of a normal approximation. A theoretical beta distribution is defined as having support for $0 < y < 1$ which is characteristic of the proportion burned area data (ref. Mood et al. 1974). Use of the data-appropriate distribution provides for more accurate estimates and confidence bounds

Results

C15. For **Figure S1.1** a reduced vertical range from 0 to 2 may help to visualize differences. Although I understand that the authors considered a common range for all the possible effects from figures **S1.1** to **S1.4**

R15. We believe it would be better to keep the response range consistent across the plots S1.1-S1.4 because they show the same response across the range of covariates and thus easier to visually compare between plots. Even though S1.1 is more in the lower range of 0-2, the trends and confidence limits are still easily discernible.

C16. Lines 332-353. The authors highlight the role of human-related factors and how they affect the predictability of Dynamic Global Vegetation Models. I found the figures S2 and S3 interesting for the discussion. Note, however, that Kazakhstan has been in the Russian Federation until 1991, so I understand that figures are trying to link the grazing intensity with this aspect. But without any specific reference, it may be a reasonable/possible link but anyway soft link. At this point I don't know if other factors in Kazakhstan could affect equally (or at least contribute to) the grazing intensity implied by Figures **S2** and **S3**. For example, the human population decreased in the 90's and increased during the 2000's.

R16. Lines 64-66 and Lines 365-396 We based our study on the varying constraint hypothesis (Krawchuk and Moritz 2011) stating that globally, fire regime is linked to fuel biomass status, and how climate might affect its amount availabaility in fuel limited ecosystems or its moisture content in drought limited ecosystems (Pausas and Ribeiro 2013). Beside these climate variables, abrupt changes have been observed globally on long term (Marlon et al. 2008) or recent fire history (Pausas and Keeley 2014), with among other targetted processes, grazing (by livestock or megaherbivores) and humans (fire prevention). For our study area, namely the Asian steppes, and for the recent period, we hypothesized that the impact of grazing on fire might be the main contribution (based on what was observed in Africa by Holdo et al 2009) following the political history of the region. In turn, this study aims at providing an additional study case in central Asia to ascertain this hypothesis so that the grazing/fire interactions might be tightly accounted for in fire DGVM interactions. The list of other possible factors associated to the change in political regime might be long (population density, farming practices, firefighting capacities (e.g. decrease in fire fighting expenditures mentionned for post Soviet period in Mouillot and Field (2005). In Kazahkstan, population decrease was around 10% and would technically lead to less fire settings. So we tested the two major fire-related hypothesis observed in African grasslands: grazing (Holdo et al. 2009) and land cover change (Andela et al.2017).

C17. It is possible that the journal required an increase in resolution of several Figures to ensure good printing quality.

R17. It is a balance between the resolution and size of the figures. We will work with the journal to maximize the resolution with reasonable size.

C18. In caption Figure 4, I would add write Northern Eurasia (including Kazakhstan) for non-linear readers of the paper.

R18. Done as suggested.

C19. Also in Figure 4. Did the authors find any reason for differences between even years than in odd years? It seems to be a close to systematic pattern: burned area in even years is larger than odd years.

R19. Lines 218-219 The trends of wave-like burned areas are typical for burned area trends in the world (e.g. Andela et al., 2017). We do not study the pattern. It is a study itself.

Typos
C20. There is a typo in the reference here [1] of the paper (ORCGIDEE but it should be ORCHIDEE)

R20. Corrected

<u>Response to Interactive Comments</u>

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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 <u>added the citation for the **climate data**</u> 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 139-157 Regarding to **<u>biomass</u>**: We <u>rewrote this section to be clearer and added a more detailed formulation and a citation</u>.

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 ($x \geq 0.46$) 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 179-182 The R packages and methodology were rewritten in these lines.