Response to Review 2nd Stage from RC2 and Associate Editor

About the format

My previous response followed the Associate Editor's instruction of the email on 8 January, including the point-by-point responses.

In this response, it has included both (a) line numbers (**as Line xxx**) of their comments (from the initial version), and (2) line numbers of changes in the revised document (**as Lxxx**), including insertion, rephrasing or removal

Response to Reviewer's comments (RC2)

CX: Reviewer comment number X

RX: Author response to reviewer comment X

Line x was referred to the previous response. Lx is referred to this response.

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 "currently" was added to the first sentence.

C2. Line 33 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. The last sentence in the abstract was deleted in Line 33-34.

Introduction

C3. Line 48 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 48 to 49: we now use "Slowdown" with the reference of Fyfe et al. 2016.

C4. Lines 52 to 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 warming signal at the period analyzed in the paper.

R4. We now added **L51 to L54** to point on the hemispherical difference and the appropriate reference in the list.

C5. Line 57 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 57** We meant here the climatic variables used for calculating the FWI. We now added this information **L55**.

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. The reference of "Sato and Nakamura, 2019" was added in **L59-L60** with a full sentence explain that point.

C7. Line 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. This sentence was actually moved to **L107 to L110** to justify our study period and our method. The impact of climate, land use and humans on fire activity were described in the new version of the modeling discussion section **L369-L382**.

Methodology. Mapping burned areas

C8. Line 88 and 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. We now specify that fire detection depends on vegetation type and that the validation of burned areas (Hao et al., 2012) did not depend on the land cover maps used (**L100-L103**). We also quantify more precisely the uncertainty of our dataset compared to reference data (**L00-L103**). The changes in the text are as follows:

L103-L105 Crown and surface fires have different signals on fire detection. The algorithms depend on vegetation type (Chuvieco et al. 2019).

L100 -L103: 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 (**L100-L103**).

C9. Lines 90 to 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. We removed L 154-L162 and rephrased now from **L112 to L114.** It was rewritten as: "For this present 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. We now give more information **L114-L115** "Current versions of land cover maps were not available for present and previous studies".

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 most important for this project, especially for studying trends. The MODIS land cover 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. We now specify this point **L112-L115**.

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. 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. We referred to **Fig. 2 L169**, **Fig 5, 6 L177** and **Fig. 3 L181**. The Statistical analysis paragraph was rewritten **L168 - L196**.

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

R13. Lines 249 to 253 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 (Bickel and Doksum, 2015). 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. Bickel and Doksum, 2015). Use of the data-appropriate distribution provides for more accurate estimates and confidence bounds. Now in lines **L194-L196**.

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 to 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. 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 targeted 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 (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).

We then modified **L67-L72 , L362-366** to better present this point of view and account for the reviewer concern.

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. 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 explore 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