

Interactive comment on “Fire and vegetation dynamics in North-West Siberia during the last 60 years based on high-resolution remote sensing” by Oleg Sizov et al.

Anonymous Referee #1

Received and published: 22 July 2020

Dear authors,

Your manuscript addresses an important topic, highlighted again this early summer with a heat wave and related extensive fires spreading well into tundra areas across Siberia, with high estimated amounts of carbon released to the atmosphere. The topic and scientific questions formulated in the objectives are highly relevant and in the scope of BG. While the topic is important, and it is obvious that a lot of work was performed for this study, the manuscript unfortunately fails in presenting especially methods in a reproducible way. Also, the manuscript is not coherent, it looks as if different work was pieced together, but not integrated from beginning to the end.

[Printer-friendly version](#)

[Discussion paper](#)



A) The objectives as formulated in the introduction are not followed by a corresponding structure and sequence in the methods and results section. This makes the overall manuscript very hard to follow as a reader, as one needs to search for the corresponding information. For example, there is not a dedicated methods section that explains how the first objective (to quantify burned surface areas and assess frequency and causes of wildfires) was addressed and the sequence is changing between methods and results. Apart from structural problems, some of the objectives are not directly followed at all or in a qualitative way only. B) Methods: The manuscript contains tables with data sets, but it remains unclear which data sets were used for which objectives/results specifically, how the imagery was preprocessed given so many different data sets of highly varying spectral, spatial resolution and quality were used. Also, details on the processing of data (esp. remote sensing data, e.g. atmospheric correction) are largely missing (indicating a software without even the version or parameters used for the algorithm is not sufficient for reproducible methods). Further, there is little to no information about validation of the classification results or reference to uncertainties of results. C) Overall the presentation of the manuscript is really not sufficient – as indicated in more detail below, graphs are poor (missing legends (esp. Fig 1 & 2), scale, missing reference of Figure in main text). Consider a more rigorous selection of graphs and information displayed in tables. Also, thorough revision of language (esp. articles) and checking of consistency are needed to make this manuscript more accessible. With this list of co-authors I expected a higher quality of the overall presentation.

The following more detailed comments on suggestions for methodological improvements are based on the structure of the results section. Please note that these comments refer only to examples, a thorough overall revision of all sections is needed, esp. also to make them coherent (also applies to discussion and conclusion sections which are not covered here explicitly as changes rely on reworked methods and results):

1. Temp., precip, climatic indices - Methods for GDD5 (line 144) – provide reference for this formula. - Line 158 – what is the 3° increase based on – a trend fitted to the climate

[Printer-friendly version](#)[Discussion paper](#)

data in Fig. 4? If yes, show the trend and related statistical information. - Data from 3 meteorological stations are presented. It remains unclear how these are linked to the 3 selected study sites as the stations are located outside of the study sites and not in obvious pairing to the 3 sites. For example on line 180 it is stated that based on the climate data analysis, the vegetation class in Novy Port changed from forest-tundra to dark needled northern taiga – how are the climate data linked to vegetation classes, and the station data to the study sites?

2. Qualitative assessment of vegetation dynamics Overall this section is not convincing as it is largely missing a corresponding reproducible methods section. Quantitative results are hard to reach based on Corona as reference data set. But even if only qualitatively assessed, methods need to be clearly outlined. - How were these transitions qualitatively assessed? Some information can be found in section 2.1, some in the field sites general description, but nowhere is clearly formulated how the transitions were visually/qualitatively assessed, what classes were followed. Also, it remains unclear how the topographic map was used for this (does it contain forested area? Burned area?). The graphs that are mentioned to highlight how this was done are not conclusive (e.g. Fig2 misses a color legend, also it is not clear from this graph which of the layers show the most reliable forest cover, Fig SB3 – without clear indication in the imagery it is hard to understand where the active afforestation mentioned in the figure title is located – this is certainly due to the very different quality of the Corona versus Yandex map layers, but as presented does not convince the reader that this active afforestation has happened). Also, how many sites (burned and background sites) were assessed in total? Are the different conditions statistically balanced (for several of the assessed transitions only a single reference site is mentioned, does it mean that this condition was only observed once)? What was the exact sampling design? - What is active afforestation? Define in the related methods section - L. 191- fig 5 is wrongly referenced (Fig 5 displays potential evaporation, not tundra after wildfire) - L 204 – removal of vegetation cover – define in related methods section.

[Printer-friendly version](#)[Discussion paper](#)

3. Dynamics of fires - Methods: classification to identify burned areas: how was the initial state determined in the Corona images? In table 2 you also list Sentinel, Modis and VIIRS data – how were these data used (you only mention Corona and Landsat in the fire methods section)

4. Dynamics of vegetation and fires - NDVI is NOT the normalized digital vegetation index - Which imagery was used to calculate NDVI? Any preprocessing performed? Georegistration issues discovered? Explanations on remote sensing data in methods are insufficient. - Fig 8 – what is displayed here exactly? This remains unclear based on the corresponding methods section and figure title. Is the standard deviation based on spatial variation for the background sites? How is temporal variation in NDVI of the background sites accounted for? Are the different years and the background areas statistically balanced for their size?

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2020-174>, 2020.

Printer-friendly version

Discussion paper

