Dear Editor,

We have revised our manuscript according to the comments of the reviewers and Copernicus Editorial Board. This letter contains a point-by-point reply to the comments, including links (line numbers) to the revised manuscript. We referred the line numbers of two revised manuscripts with and without revised parts. Line number of the manuscript with revised parts (marked–up) is shown in parentheses.

Besides, we added our own revisions for the ring width index (RWI), described in the end of the letter, because we added new data. After the update of the RWI dataset, there were no significant changes in the results of our study and their interpretations.

Reviewer #1.

We numbered all comments and replied to them.

(1) In the introduction, discussion and particularly the conclusion, the authors mostly discuss earlier findings from Spasskaya Pad, and hardly touch upon potential similarities or dissimilarities with other regions. This makes it very hard for the reader to assess to what extent the findings presented here may hold lessons for the other boreal forests on permafrost. In my view, your results hold important lessons for the potential impacts of increased precipitation variability in northern forests, also beyond Siberian larch forests! Precipitation variability is increasing rapidly in this region (see also https://doi.org/10.1016/j.jhydrol.2021.126865) so it is important to discuss what your findings imply for the future functioning of Siberian larch forests and potentially boreal forests in general. You also demonstrate a clear "legacy effect" that could be related to recent insights regarding duration of the impacts of extremes (see for instance https://onlinelibrary.wiley.com/doi/pdfdirect/10.1111/gcb.16078). You still find divergence in NDVI over ten years after an extreme event. This is a major legacy effect, that has important implications for knowledge on Arctic greening/browning and should be stressed more strongly in the conclusion and abstract!

Response: We added more explanations in the abstract, introduction, discussion section 4.4, and conclusions. In the abstract, the implication of our results was added as described in the reply to the comment (5). In the introduction, we explained the greening and browning trends over high-latitude regions and what factors can control them on L. 38-51 (L. 38-51). Browning was mainly observed in dry regions, including our study site. Besides, introduction was thematically restructured as described in the reply to the comment (8). We explained the long-term effect of the extreme wet event in our study site, which can be potentially observed in other dry regions, in the discussion on 520-529 (L. 475-484) and conclusions on L. 552-554 (L. 504-506).

(2) The described aim of the research is to assess how the local forest has changed over time, but throughout the methods you have decided a priori to split up the data into a pre-2007 and post-2007 segment based on an extreme event. Hence, it seems more appropriate to either first statistically evaluate and demonstrate whether there is a significant trend break. I do not doubt this would be the case if you would try it, but it would provide a back up for your methodological choice. Alternatively (maybe this is easier) you could reframe the research aim to explicitly investigate the effect of this wet event. This would make sense, since the subdivision of forest types within the transect seems to already be based on forest damage and regeneration stadia, and the introduction already extensively discussed observed effects of the 2007 wet period.

Response: Yes, we agree with the comment. Our study is not only the extreme wet event, and we would like to show the historical variation of the larch forest using NDVI. The larch forest at our study and also northeastern Siberian taiga site have been suffered from drought and recently wet event. We would like to show how drought and wet event affect the NDVI. But for the most visible and impacted change in the correlation between NDVI and ecosystem parameter (especially soil moisture) was the wet event. It is not possible to change the statistical analysis at this moment, therefore we reframed the research aim to investigate the effect of wet event as described by the reviewer on L. 103-108 (L. 83-88).

(3) The ecophysiological meaning of the d15N, d13C and C/N ratio data, as well as the methods through which they were derived, are completely lacking. The reader will need more background to understand the presented patterns and the methods are not reproducible here.

Response: We added the explanations. Please read the replies to the comments (15), (16), (26).

(4) I have some concerns about confounding effects of seasonal availability of landsat ndvi data in shaping the temporal dynamics of ndvi and affecting relationships with other site data. In the line comments, I have added some examples and suggestions on how to deal with this. I think with an additional figure or potentially addition of covariates/interactions such issues could be resolved quite well.

Response: When we use the satellite image data, there are many problems such as temporal resolution and combination of different sensors. Please read the replies to the comments (14), (19), and (20). We tried to describe as much as possible.

Answers to the Line comments:

(5) L. 29: Could you reflect briefly on the implications of your results to place them in a wider context? Parts of the Siberian Arctic show record browning in recent decades, as you undoubtedly know better than anyone. Perhaps you could reflect on the potential role of moisture dynamics, drought and waterlogging in this browning trend? (Just a suggestion).

Response: Yes. As you know, boreal forests in northeastern Siberia are experiencing browning, because of not only by temperature-induced drought but also waterlogging and nitrogen dynamics as we showed in our manuscript. We added the sentence to the abstract on L. 31-32 (L. 31-32).

(6) L. 31-32 "occupy a large forest area, approximately 27 % (Fao, 2020)" --> I assume you mean 27% of the world's forest cover? Could you rewrite this to make it clearer what the statistic refers to? Also consider writing "FAO" instead of "Fao" as you also write it in the reference list.

Response: Thank you very much. We changed Fao to FAO. According to FAO, 27% is the percentage of boreal forest in the total forest cover, on L. 35 (L. 35).

(7) L. 39 "and change the ecosystem" --> Could you provide a few concise examples and references?

Response: We removed the sentence after restructuring introduction.

(8) L. 31 - 66: Please consider adding some thematic structuring to the introduction; the introduction seems to give an overview of earlier work that is mostly focused on C-exchange, while the knowledge gap decsribed on L. 65-66 focuses on NDVI and foliar parameters.

Response: We thematically structured the introduction as the following: boreal forests on L. 34-51 (L. 34-51) -> dry Siberian forests on L. 52-67 (L. 52-63) -> not only drought but also extreme wet event affects the forest on L. 68-84 (L. 64-74) -> knowledge gap about NDVI observations on L. 85-91 (L. 75-81) -> aim of the study on L. 102-108 (L. 82-88).

(9) L. 67 - 70: The research aim is described as "assessing how the forest has changed", which seems unnecessarily vague. Could you provide more specific aims or research questions and (optionally) hypotheses? Setting more specific aims may also help provide structure and direction to the introduction paragraph above.

Response: The sentence on L. 103-104 (L. 83-84) described the outline of our aim, and this looks vague. We changed the paragraph as shown on L. 103-108 (L. 82-88).

(10) L. 78: "consists of deciduous species" --> any information which ones? do they occupy a significant share of the canopy compared to dominant larch vegetation?

Response: The deciduous species are larch and birch. To avoid misunderstanding, we changed the description on L. 116-118 (L. 96-98).

(11) L. 80 " and other grasses" --> please remove "other" (as the shrubs mentioned before are not grasses)

Response: Removed on L. 118 (L. 98).

(12) L. 95: "Regenerating forests RF-2 had moderate forest conditions between RF-1 and DF" --> what do you mean by moderate forest conditions?

Response: We described the difference between the RF-2 and RF-1 on L. 130-131 (L. 110-111) and removed the sentence on L. 134 (L. 113-114).

(13) L. 108 - 110: " The transect plots, which consist of pixels not attributed to quality pixels (clear terrain, low-confidence cloud, and low-confidence cirrus) in the quality assessment bit index band according to Landsat Surface Reflectance product guides, were excluded from the analysis. --> due to the structure of this sentence it reads to me as though all transect plots ndvi values were excluded from analysis, but as the text continuous you describe how it was used in further analysis, so I assume you only removed pixels (or transect plots?) that were flagged in the QA product? Perhaps you could rephrase this more clearly (e.g. that "pixels flagged in the quality assessment bands were omitted from analysis"? or that "transect plots that contained pixels flagged in the quality assessment bands were omitted from analysis"?).

Response: We rephrased the sentence on 148-150 (L. 128-130).

(14) L. 120: can you provide an assessment of fit among the different sensors, e.g. on days for which multiple products are available? how accurate is the estimate for the one sensor based on another sensor compared to the actual value? Roy et al 2016 recommend to use a locally parameterized regression, although it would be understandable if insufficient overlap in acquisitions among different sensors prevents establishment of specific regression parameters for your site.

Response: We understand that local parameterization is important, because it is not possible to combine different sensors perfectly. However, unfortunately, we cannot show the sufficient data of assessments for publication. In our study, three Landsat images (Landsat 5 TM (L5), Landsat 7 ETM+ (L7), and Landsat 8 OLI (L8)) were available. L7 had the longest observation period, but actually data quality was not so good, compared to L5 and L8 (after the scan-line corrector failure of L7 in 2003). After the selection of image data and conversion by Roy et al. (2016) and Ju and Masek (2016) as described in Methods 2.2, and we again selected the NDVI data for comparisons between L7 and L5 for the period 1999-2011, and L8 and L7 for 2013-2019, by the following conditions.

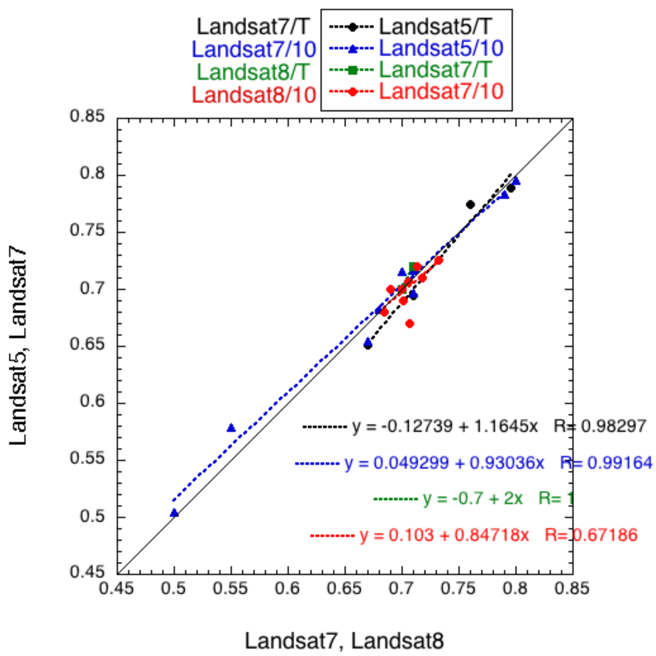
- For transect plots, all 34 transect plots were observed. For 10-km plot, more than 96% of pixels in L5 and L8, and more than 75% of pixels in L7 were observed.
- There was one day difference in the acquisition dates between L5 and L7 and between L7 and L8, and NDVI signals were close.

- If the average value for the short period in summer (NDVI shows usually small change in July to beginning of August) was calculated, we used the average value.

Eleven data (including transect and 10-km plot) for comparison between L7 and L5 and twelve data (including transect and 10-km plot) between L8 and L7 were identified. The results were close to the 1:1 line (see the figure below).

There are many problems on statistical procedure if we show these assessments in our paper. But we believe that the conversions by Roy et al. (2016) and Ju and Masek (2016) can be used realistically.

We put some sentences to Methods 2.2 L. 160-163 (L. 140-143) and Discussion 4.1 L. 365-367 (L. 337-339).



(15) L. 133-136: this paragraph lacks context of the ecological or physiological meaning of isotope ratios and C/N ratio. More explanation and literature is needed for the non-expert reader to assess what the d15N, d13C, C/N ratios and ring widths actually mean and what questions you are answering by including these data (alternatively, you could also already explain how the different types of datasets relate to the research aims in the final introduction paragraph)

Response: We revised and added some sentences for explanation and literature on L. 184-199, 209-211 (L. 164-179, 189-191).

(16) L. 133-136: There seems to be no explanation of how the d15N, d13C and C/N ratios were derived, Add methodology (which tissues were sampled, how many grams, how were they analyzed, on which instrument, against which isotope standards at what precision?). If the data come from an existing dataset or study, please cite it so the reader can understand how the values were derived.

Response: We put the outline of methodology in Methods 2.3 L. 200-207 (L. 180-187) and the methodology details in the caption of Figure S1.

(17) L. 150: can you explain why you chose a pearson correlation, rather than a spearman correlation or crosscorrelation function (which in my experience are more appropriate choices for relatively short timeseries)? Not that I doubt the outcome of your analysis (you present very clear visual and temporal patterns), but the backing of your choices could be stronger.

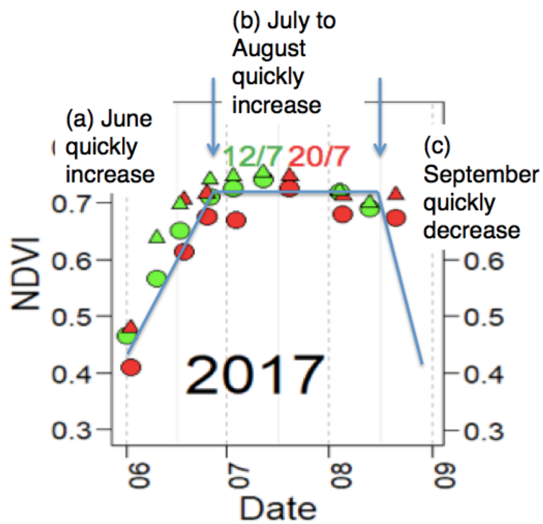
Response: Since we obtained a simple linear relationship between two parameters, the most common test (Pearson correlation test) was used. We added the short description on L. 221-222 (L. 201-202).

(18) L. 152: "differences between the two groups" --> which groups are you referring to? there are more than two types of forests mentioned in earlier in the methods. It is also unclear to me why an unpaired test was selected if data from the same years or acquisitions is available for different forests. I am probably misunderstanding what you are describing here, so perhaps that is an indication that better explanaiton is needed.

Response: Two groups mean two different forest types among four (TF, RF-1, RF-2, and DF), but we changed the description about the statistical tests on L. 223-228 (L. 203-205). It is better to use tests comparing 4 forest types at the same time in order to avoid Type I errors. So, we changed statistical method to Kruskal-Wallis test with pairwise Wilcoxon rank sum test. We preferred this test to ANOVA because of a relatively small number of samples in the forest types.

In the manuscript supplemental, we replaced the old Tables S3, S4 with two new tables, and the old Table S5 was removed. Consequently, we changed the table number on L.229 (L. 205), 296 (274), 311 (288), 326 (298), 327 (299), 330 (302), 344 (316), 383 (355).

(19) L. 163-165: "The seasonal maximum of each year was observed from 25 June to 13 August, except for 1999 (shown in Table S2). The maximum transect NDVI in 1999 was observed on 27 August (0.75 ± 0.02, n = 34) because the Landsat data in 1999 were limited to the latter half of August. " --> landsat scene availability throughout the summer can be highly limited. to what extent is the seasonal maximum an artfeact of data availability (e.g. it would obviously fall in June if no data from July and August are available, even if the true maximum would fall in july or august). Please add an indication or statistical backing (maybe in SI) of how the timing of the seasonal maximum relates to scene availability, because otherwise it cannot be called "year to year variation" and it would be unclear whether the time series you describe in fig. 2a is robust, or merely an artefact of seasonal timing.



Response: The figure above is an example of the time course of NDVI in 2017 summer period. Red triangles and circles are L7 data for transect and 10-km plots, and green triangles and circles are L8 transect and 10-km plots. We divide the growing season to three stages (a) to (c). In June (a), NDVI values quickly increased, and during the late June - the mid-August (or in the beginning of September) (b), NDVI values are relatively stable because vegetation has the maximum biomass. Finally, during the late August-September (c), NDVI values decrease due to the leaf senescence. Although the timings of (b) (start and end) varied depending on the weather and soil moisture conditions, maximum biomass stages (b) continued more than one and half months. We obtained NDVI on this stage as seasonal maximum (the manuscript’s supplemental Table S1). The example of NDVI in 2017 (shown in this response letter) is the highest temporal resolution, and other years are lower temporal resolution than that in 2017. But the data in most years had more than 3 data acquisition days in the period of (b). Only one acquisition day during the period of (b) was for 1999 and 2003. For 2003 the observation day was 21 July, and we used this value. For 1999, it was on 27 August. We recognized this NDVI value as seasonal maximum, since this day was in the period of (b). Because of large amount of precipitation in August in 1999, we observed high soil moisture in August 1999 (Figure 3c), and recognized in the period of (b).

We added some sentences on L. 176-182 (L. 156-162).

(20) L. 191 - 192: "To consider the historical variation in the NDVI of typical forests in our study area, the TF NDVI and observed parameters were compared (Fig. 2 and 3)." --> I would strongly urge you to account for landsat scene availability throughout the season, for instance by adding the date within the season as a covariate or interaction. This would give additional information of the association with other parameters may vary across the season and would account for the possibility that the temporal dynamics of ndvi are influenced more by scene availabiltiy than annual dynamics in site conditions.

Response: As we already described in the reply to the comment (19), seasonal maximum during the NDVI stable period was determined in each year, and we believe that the NDVI can be compared with ecosystem parameters.

(21) L. 197: "TF NDVI did not show any correlation with summer temperature" --> you present correlations of NDVI values at different seasonal timings (june / july / august) to overall JJA temperatures. wouldn't it make more sense to compare the ndvi to mean temperatures of degree days up until the moment of ndvi acquisition?

Response: We believe that in the temporal dynamics, the soil moisture and nitrogen availability may be the main environmental factors affecting the NDVI. The summer temperature does affect the NDVI, but in short time periods, e.g., drought events in 2001-2002.

(22) L. 218: the header of the next section accidentally ended up in the figure caption here.

Response: Thank you very much. The next title was mistakenly added. It was removed on L. 287 (L. 265).

(23) L. 275: "In most years before 2007, the NDVI values in RF and DF were higher than those in TF" --> could this be related to topgraphy; i.e. DF and RF are damaged by floods since they occur in depressions and hence suffer less from drought but more from flooding? the role of terrain is hardly touched upon but potentially very important. It might also be helpful to present some indication of terrain variability; what is the magnitude of elevation differences between typical DF and Tf sites, for example?

Response: Yes. Before the wet event, soil moisture at RF and DF were higher than TF because of lower elevation. This topographic condition at RF and DF makes lower possibility of drought. We did not observe the altitude in situ, but the difference in elevation between north and south ends by Google Earth Pro was about 5 m. We added the explanation on L. 345-348 (L. 317-320).

(24) paragraph 4.1: Please discuss whether waterlogging may have influence ndvi directly, independent from tree properties, due to its influence on near infrared reflectance.

Response: Yes. Water shows lower NDVI. We had already described about the possibility of surface water on L. 353-354 (L. 325-326). We also added some explanation on L. 354-355 (L. 326-327).

(25) L. 312 - 317: I know it is very likely the case, but here you seem to derive causation from the presented correlations. Tone down these causal statements (e.g. "which likely contributed" instead of "which contributed"), or provide more backing for why carbon storage in previous years should be the cause of NDVI dynamics in this period.

Response: We changed the expressions, e.g. on L. 388-393 (L. 360-365).

(26) L. 327 - 328: "The mechanism by which plant δ13 C responds to changes in light and water availability has been well explained in previous studies (e.g., Farquhar et al., 1989). " --> I don't doubt it, but it is very difficult to place your findings on isotope ratios in the appropriate context without some minimum amount of explanation of their meaning and key processes driving isotope fractionation in trees. Please add this (or see comments regarding lines 133-136)

at some point so the reader can understand the meaning of the presented work on isotope and c/n ratios to some degree without having to refer to cited work.

Response: The $\delta^{13}C$ value of plant tissue (e.g. leaf) is expressed by the following equation: $\delta^{13}C = \delta^{13}C_{atm}$ –a-(b-a)(C_i/C_a).

$\delta^{13}C_{atm}$ is $\delta^{13}C$ of atmospheric CO_2, a (4.4‰) and b (27‰) are isotope fractionations of diffusion and enzymatic reaction of photosynthesis (Rubisco), and C_i and C_a are inter-cellular and atmospheric CO_2 concentrations. When water availability decreases, stomatal conductance increases, which results in the decrease of CO_2 incoming to the intercellular, and C_i decreases, resulting in C_i/C_a decrease and $\delta^{13}C$ increase. When water availability increases, $\delta^{13}C$ decrease. For the light condition, when light condition increases, more CO_2 is photosynthetically reacted, and Ci decreases, then $\delta^{13}C$ increase. Under low light condition, C_i increases and $\delta^{13}C$ decrease.

We already described what was happening at our site during the drought. The years 2001 and 2002 were severe drought period (low precipitation and low soil moisture). Under such condition, it is reasonable to consider that stomatal conductance decreased. This is also demonstrated by $\delta^{13}C$ values. Larch tree is deciduous, therefore C photosynthesized in the year makes needles in the next year. Carbon fixed during the drought 2001-2002 makes needles in 2002-2003.

It is not possible to describe the detailed explanations above in our main text, but we add the equation and short explanations in the Methods 2.3 after the explanation for comment (16). Besides the following explanation, we also revised the structure of 4.3.1 Water availability, according to the comment (2), and to avoid misunderstanding (see the reply to the next comment).

(27) L. 329: "Under drought stress during 2001–2002, there was a decrease in needle stomatal conductance" --> this is another example of a conclusive statement that does not seem to be backed up by data or a reference. Please check the entire discussion for statements like these and either back them up or tone them down ("has likely decreased stomatal conductance, as suggested by d13C values")

Response: Referee said this statement does not seem to be backed up by data. But for us, "drought -> reducing stomatal conductance ($\delta^{13}C$ increase) -> usually decrease in carbon assimilation" are almost 100% sure. We would like to describe that after 2007 "wet condition -> increase in stomatal conductance ($\delta^{13}C$ decrease) -> usually increase in carbon assimilation but actually decrease in carbon assimilation". We observed low NDVI in wet condition, which is probably caused by lower nitrogen availability. To avoid misunderstanding, we changed the structure of 4.3.1, and added some sentences on 402-434 (L. 374-394).

(28) L. 354 - 346: "Therefore, the decrease in the TF NDVI in wet years may be due to factors other than the carbon assimilation process" --> here you should probably discuss the direct influence of water on near infrared reflectance and ndvi.

Response: As already described in the response to comment (27), we also revised the manuscript. About the effect of surface water, we already described in the response to comment (24).

(29) L. 400 - 401: "However, the TF NDVI and RWI were not significantly correlated after 2007, whereas there was a significant positive correlation before 2007. " --> please consider alternative explanations. For instance, the use of detrending methods in tree ring width series can remove long-term decreases or increases from the time series, and your RWI likley only reflects year-to-year variation in ring width. In this sense, do you think the RWI series reflect any long-term decreases due to for instance waterlogging events and comprimised growth over longer timescales?

Response: As describe by the reviewer, RWI reflects more long-term. There are many interesting things on tree growth. For example, dead trees from waterlogging were affected by not only the waterlogging but also drought several years ago (Tei et al., 2019 Ecohydrology). But radial growth of tree is not our aim in our study. Therefore, we cut some sentences on L. 487-491 and revised L. 486-487 (L. 445-446).

(30) L. 432-434: "To better understand changes in the forest, long-term observation of variations in soil N availability depending on soil moisture and other factors is necessary" --> Perhaps we would also need better understanding and forecasting of precipitation extremes or weather extremes in general?

Response: Yes, of course. For the studies of ecosystem change, we need the predictions of climate and weather. But these are totally beyond our aim. So, we did not add the explanation.

(31) L. 435-452: In general, I think the conclusion presents some statements that rely on interpretation quite a lot, and presents a lot of statements that are merely repetition of the results. I do not disagree with your interpretations (I think they are well found), but it should be clear for the reader which statements are interpretations and which are not (e.g. by adding "which we attribute to .."). Also see my main comment; the conclusion does not go beyond the distinct physiological response observed in this ecosystem and does not discuss implications. To be of value to a wide readership, please try to "zoom out" a bit beyond Spasskaya Pad. Maybe mention and discuss the importance of findings such as the long-term alteration of relationships between moisture availability and tree performance, or provide recommendations for future studies.

Response: We revised the conclusions on L. 534-564 (L. 489-506). The obtained results and their interpretations were explained together, and the expressions of the interpretations were changed. The implication of the results was also added.

(32) Table 1: The added value of this table relative to the clear patterns in fig 2b, are unclear to me. I also find it unclear why only TF and Rf1 are presented. Due to nestedness (transect plots within years within groups), the p-values should be corrected for pseudoreplication. A visual overview might be stronger here and you could consider replacing or omitting this table.

Response: We removed the Table 1 from the manuscript and revised sentences on L. 304-305 and L. 400-401 (L. 372-372), which mentioned the Table 1.

(33) Figures 4 & 5: "p-values and R2 describe the significance and the degree of variability of the regression models, respectively" --> degree of variability is probably not the appropriate term here, I assume this is a coefficient of determination?

Response: Yes, this is the coefficient of determination. To avoid misunderstanding, we revised the description on L. 286-287 (L. 264-265) and L. 340-341 (L. 312-313), the caption of Figure S4.

(34) SI tables S4-S5: How reliable are the p values derived for differences among degraded forest and other forest types, if there were only two transect plots with data for degraded forests? I also find it hard to understand why the others use pairwise tests rather than anova/kruskal-wallis tests with post-hoc tests? Throughout the supporting tables S4-S10, you perform very large amounts of t-test and if you want to use these values to support your findings, you should discuss the role of Type I errors.

Response: We changed the statistical test. Please read the response to the comment (18).

Reviewer #2.

In "Historical variation in normalized difference vegetation index compared with soil moisture at a taiga forest ecosystem in northeastern Siberia" the authors investigated the variation in NDVI among forest conditions (typical mature, TF; regenerating-1, RF-1; regenerating-2, RF-2; and damaged forests, DF) and field-observed parameters (from 1998 to 2019) such as RWI, soil moisture, changes of larch needles (δ13C, δ15N, C/N), air temperature, and precipitation. The authors determined that prior to the 2007 extreme wet event, wet areas like DF and RF had higher NDVI values than dry TF sites due to greater water availability. However, following 2007, the TF had a greater NDVI than the DF and RF, although being visibly unaffected by the wet event.

Studying historical variations in NDVI compared with soil moisture at a taiga forest ecosystem in north-eastern Siberia is important for several reasons. Firstly, NDVI data can provide valuable information about temporal and spatial changes in vegetation distribution, productivity, and dynamics, which allows for the monitoring of habitat degradation and fragmentation. Secondly, the comparison of historical variations in NDVI with soil moisture can provide insights into the impact of extreme weather events on vegetation, such as the extreme wet event in 2007, which resulted in high tree mortality and a decrease in NDVI at affected sites. Understanding the ecological effects of climatic disasters such as drought or fire can be assessed using NDVI data, making it a valuable tool for monitoring changes in vegetation due to climate change. Overall, studying historical variations in NDVI and soil moisture in a taiga forest ecosystem can provide valuable insights into the impact of extreme weather events on vegetation and the effects of climate change on vegetation dynamics. Therefore, this paper has the potential to make an important contribution to the body of knowledge concerning the impacts of global change on sensitive and complex permafrost ecosystems.

It is my opinion that the authors used sound methods to address the study aims and presented the research findings clearly and concisely and they used appropriate figures to illustrate the NDVI values of the forest types and the trends in the transect and 10-km plot, which could be useful for researchers and policymakers. However, I agree with referee 1 about their main points raised as well as the minor comments provided. To avoid repetition and in the interest of brevity, I will not be going over them again in this review, but I strongly advise the authors to make the corrections already suggested. Instead, I will just add a few points concerning the discussion section that I would like to see addressed before publication. When the authors revise these issues, I recommend the study for publication in Biogeosciences.

In the discussion, the authors considered the probable reasons for the differences in NDVI values among the forest types, such as the change in vegetation and the presence of surface water and saturated soil. However, the section could benefit from a more critical evaluation of the results and their implications. For example, the article does not address the limitations of using NDVI as a proxy for vegetation health and productivity, which could impact the accuracy of the results. NDVI measures the amount of chlorophyll in the uppermost layers of vegetation. This means that it may not accurately represent the health and productivity of plants with lower canopies or those that are hidden from view. The limitations of using NDVI as a proxy for vegetation health and productivity may be particularly relevant in taiga/permafrost ecosystems due to their complex vegetation structure and sensitivity to environmental changes.

Additionally, the article does not explore the broader ecological implications of these findings, such as how changes in vegetation health and productivity may impact ecosystem services or the ability of forests to sequester carbon. Finally, while the article notes the potential for using the observational data for analyses of ecosystem changes at the plot and regional scales, it does not explicitly state what these analyses might entail or why they would be valuable. A more explicit discussion of the practical applications of the research could make the findings more accessible to a wider audience.

Response: We added the explanations for the limitations of using NDVI on L. 85-91, L. 365-367 (L. 337-339), L. 520-523 (L. 475-478). The NDVI was shown to be affected not only by the overstory vegetation but also the understory vegetation. But our study was mainly focused on the typical larch forest, which was not visibly damaged by the extreme wet event. The typical forest presumably showed higher contribution of the overstory (larches) to the NDVI than damaged forests. The NDVI and larch needle C/N showed a significant correlation, so it is likely that NDVI showed the overstory conditions.

We also added some descriptions about explicit discussion of the practical applications in the discussion 4.4 L. 520-532 (L. 475-487). This phenomenon observed at our study site might happen in other dry regions. The implication of our results was also shown in the abstract L. 31-32 (L. 31-32) and conclusions on L. 552-554 (L. 504-506).

Copernicus editorial.

I noticed that your Figure 1(a) contains a map.

For the next revision, I kindly ask you to clarify whether you have created the maps or were they created by a map provider?

If the maps were not created by you, please provide in your revised file that the copyright is denoted in the figure itself. If this is not possible, please provide it in the caption.

Response: We added descriptions about the providers of the maps used in Figures 1 (a) and 1 (b) in the caption of Figure 1 on L. 117-118 (L. 137-138) and on Figure 1a itself.

Authors' revisions.

We updated the ring width index (RWI) dataset after adding a greater number of larch tree paired increment core samples. As a result, the results of our study, namely the descriptions of Pearson correlation (r, p-value) and linear regression models (R^2, p-value) between the TF NDVI and RWI, after the update were not significantly different from those before the update, so there were no changes in their interpretations. The revisions were made in the results section 3.3.2 on L. 289-297 (L. 267-275), the discussion section 4.3.3 on L. 473 (L. 433), Figures 3b and 4a, and in the supplemental Figure S4g, Table S2 and S5-S7.