Reply to the interactive comments of Anonymous Referee # 2 to our Discussion paper:

"Modelled potential forest area in the forest-steppe of central Mongolia is about three times of actual forest area"

General replies to the first paragraph of the comments of referee 2:

Here, we present a new concept for spatially delineating tree biomass data from field investigation by topographic and climatic parameters and forest-type classification. We indicate suitable parameters and gain quantitative information.

- Concerning our hypothesis (I): There is no investigation on the variation of tree biomass related to soil conditions and topographic position in the Mongolian forest-steppe so far. Furthermore, we have shown that selective logging has different impacts on biomass reduction of different forest types.
- Concerning our hypothesis (II): We have shown that the NDVI is not a suitable parameter for biomass assessment at high resolution, but it is an indicator for overall vegetation vitality in forests.
- Concerning our hypothesis (III): Our geo-ecological approach, based on topographic and climatic parameters, allowed us to specify the distinct limitations for forest distribution in the study area (L32-36).
- Concerning our hypothesis (IV): The potential extent of natural forest area in the forest-steppe ecotone is discussed controversially in the scientific literature. We provide a new approach to estimate the potential forest area, and we are able to present quantitative results.
- Although our hypotheses might look like common knowledge, we have shown that some of them do not fit to general assumptions. We worked out multicausal criteria; for example, with respect to the relationship between natural forests and permafrost distribution. Furthermore, those parameters that were statistically proofed, were used for forest area delineation and biomass estimation.
- The validation of the listed hypotheses provides the background for our research questions (L111-117), for which we try to obtain quantitative answers.

We reply to your specific comments step by step:

L24: what do you mean by landscape unit? I assume you mean vegetation type, but it wasn't introduced beforehand. Moreover, why different vegetation type and site (which possibly include site-specific soil, climate and disturbance history) showed minor difference in vegetation biomass? Is it a technical issue preventing you from showing a difference? Or is it something embedded in vegetation structure/landscape heterogeneity?

- You are right; this statement needs improvement and clarification in the abstract. It is based originally on the variance analysis shown in Figure 5, where we did not obtain statistically significant differences between the forest size classes and environmental conditions like: "fire / no fire", "logging / no logging", "slope debris / sandy soil", with the exception of the pair "forest edge / forest interior" (L287-289). It is true that the term "landscape unit" does not fit the statement here. Thus we propose to change the sentence for clarification to:
  "Variance analysis did not proof a significant difference between the mean tree biomasses of forests under different conditions (with respect to forest fire, exploitation, and soil type) and forest size classes, with the exception of a significant difference between the forest interior and the forest edge (defined as 30 m wide belt)."
- The corresponding statement in the Conclusion L 552-553 will be corrected in the same way.

L24 – 25: Some explanatory texts to illustrate the why's would be helpful – currently this is a plain description of the result. Later in your method you indicated that "NDVI should be applicable in the study area" (L83 – 84).

- You are right; the statement concerning the lack of correlation between NDVI and tree biomass needs clarification:
  "We found no significant correlation between tree biomass and NDVI (normalized differentiated vegetation index). This can be explained by two main reasons. The needle biomass of adult larch trees, which produces the relevant NDVI signal, remains almost constant with tree age, whereas wood biomass increases. The NDVI signal is strongly modified by the understory vegetation because of the low canopy closure of the larch forests."

L26: A quantitative definition of forest edge is needed here. All the subsequent estimate of forest biomass really depends on your definition of forest edge, especially given that "interior" and "edge" biomass are different.

- Thanks for this comment. We will give this information in the sentences before, when explaining the variance analysis (see above).

L29: Is the range for the forest biomass a minimum and maximum or a confidence range? Please define it.

- No, it is the full range of the mean values. To make this clearer, we may write instead:
  "The mean tree biomass in forests of 10-500 ha ranged between 199 and 220 Mg ha^{-1}"

L33-36: Can you rank the relative importance of these factors based on more advanced statistical analysis (e.g. AIC)?

- All these factors really represent the limits of forest distribution in the study area as described in the chapters 4.4.1. and 4.4.2. Truly limiting factors cannot be ranked, as they all equally mark the boundaries of potential forest growth.
  To clarify the limiting quality of the parameters, we will change the statement to:
  "Presence of forest was controlled by the limiting factors: elevation (<2600 m a.s.l.), aspect (no southern slopes below 2100 m a.s.l.), slope (<25°), mean annual precipitation (160-340 mm) and mean growing season temperature (6.5-10.8 °C)."

L37-40: Unclear how you did the modelling – but I take this as just a statistical interpolation without any mechanistic processes involved. Essentially you assumed factors affecting vegetation distribution are static in historic and current time period. That's a huge assumption, and tonnes of literature are out there dis-proving this assumption. Up to this point (which reaches the finishing line of the Abstract), I am afraid I do not see any new insights that this study reveals. In fact, the results described here is merely a technical report rather than scientific discovery. Yet, the title is misleading – it implies a major scientific discovery.

- It looks like we did not properly transfer our "modelling" procedure from the results into the abstract. We will avoid the term "modelling", which we used for our analysis of the potential forest area based on its actual restrictions, through the entire manuscript. At least, we did "delineation" and no interpolation where forest may have a geo-ecological potential to exist under the present environmental conditions.
- *"The distribution of forests showed distinct limitations related to topographic and climatic parameters, which we used for the spatial delineation of potential forest area under the present climatic and geo-ecological conditions."*

L41: Surely there are a lot more to say (e.g. implications/limitations) than this one sentence!

- We may separate and extend the statements of forest area delineation and tree biomass estimation to better explain the novelty of the technical procedure presented here.
- *"The actual forests of the study area covered 1,086 km^2. In 1986, prior to extensive forest fires, it was 1,898 km^2. Delineating the potential forest area resulted in 3,552 km^2 (based on topographic parameters) and 3,113 km^2 (based on climatic parameters), which is about three times of the present forest area. We calculated the total tree biomass for three scenarios based on the mean values for interiors and edges of different forest size classes, derived from our empirical data of undisturbed forests. The actual tree biomass of 20 x10^9 g was 57 % of the tree biomass in 1986, whereas the potential tree biomass would reach between 58 x10^9 g and 65 x10^9 g, respectively. Different from a spatial interpolation of a single mean tree-biomass value and unaffected from the presented insufficient correlations between tree biomass, NDVI and topographic parameters, the proposed evaluation procedure considers the distinct differences between forest size classes for the estimation of tree biomass."*

L72 – 73: Is there a citation to support your statement?

- This statement derives from the various study sites in Mongolia that are reported in the literature listed in the two sentences before. We will change it to:
  "Tree biomass reported from the authors ranged between 123 and 397 Mg ha^{-1}. The distribution of their study sites points to a decrease of average tree biomass from the more humid north to the arid south."

L84: That's quite a weak statement. What's the range in crown closure? What is the evidence for applicability of NDVI to Mongolian boreal forests? The next sentence was a simple description of who did what – there is no evidence in your description.

- We depict the information from the literature source. Goldammer (2007) stated this mean value and no range. However, this statement provides sufficient information for our intention to describe that canopy closure is not too dense. This fact is proofed by our investigation later in the text.
- In the previous sentence (L80-82) we point to the problem, which arises when executing NDVI analysis in regions with more than 100% canopy closure. What we intend to point out here is that NDVI analysis may potentially be applicable in our case, because the signal in the forest-steppe is not saturated.
- The entire paragraph deals with the possibility of quantifying tree biomass from field measurements by remote sensing data, because we use such statistic approach for our research. This is the introduction chapter where we introduce the state of research. Thus, here we report the scientific background, including who has done what in the study region.

L88: But relating NDVI with climate only gave you inference of observational relationships. There is really no process-based extrapolatable power in these relationships.

- As the aim of our approach was to identify the potential forest distribution and to quantify tree biomass by geo-ecological parameters, the inference to observational relationships provides a valuable base for the delineation process, especially because we used strictly limiting factors, which work well for delineation, even without process-based extrapolation.

L103 – 109: These hypotheses are not really well-linked to the background appeared before. Plus, what novelty do you have in these hypotheses? Aren't these obvious already? At least I wasn't convinced that there are novel insights to be revealed by the current set of hypotheses.

- Based on the hypotheses, which may be regarded as common knowledge on geo-ecological relationships, we built up our research questions (L11-117) that better point to the novelty of the research: to obtain quantitative results.
- In a first step, we had to check, whether these hypotheses can be proofed by our biomass data empirically measured during fieldwork and by remote sensing data. Later on, in the results chapter, it was shown that these common assumptions are not comprehensive in any case.
- After identifying distinct parameters, for which significant causal chains could be proofed, these were used as the base for the second step. This step was the spatial delineation of the potential forest distribution and the estimation of tree biomasses.
- This is a new approach for detailed quantification of forest area and tree biomass.

L144-145: This description on vegetation pattern already proved some of your hypotheses, no?

- These general statements were replaced by more precise values in the study area.
- Our hypotheses extend the view from "vegetation pattern" to (I) tree biomass and (III) tree vitality, which are different issues and have not been investigated in the Mongolian forest-steppe before.

Figure 2 really shouldn't be a main text figure.

- This climate diagram can be replaced by two climate maps (MGS and MAP) produced from the CHELSA dataset.

L176: Why using the mean of the two methods? You must demonstrate the performance of these two methods – citing a paper without explaining the appropriateness of these methods to your data is not the way to convince your readers. Also, it remains unclear how belowground biomass was estimated. More details are needed.

- The methods of biomass estimation (including belowground biomass estimation) and determination of the allometric functions for Mongolian larch forest are well established and described in detail in the cited literature (e.g., Battulga et al. (2013) and Dulamsuren et al. (2016)). The reader may refer to the original sources, if there are any doubts about the appropriateness of these methods.

L192-193: Here it seems that you proved your 1st and 3rd hypotheses too. Why making the assumption that fire was the only factor affecting forest cover in this period? In L152-155, you've indicated that there are logging activities in the region. More importantly, you are essentially assuming static vegetation distribution in these two distant periods. Clearly there are so many factors affecting vegetation biomass and dynamics over this period (regrowth, climate, CO2)!

- There are three different issues in our investigation, which are clearly distinguished in the hypotheses and should not be mixed up: (1) Forest distribution (area) (2) tree biomass (volume) and (3) tree vitality.
- In L192-193, we describe how we did the mapping of forest area. Spatial forest destruction occurred by forest fires, whereas logging is mostly done selectively. We rarely observed small clear cutting from socialistic times in the fields. Thus, forest-area change in the study area mapped by remote sensing data is predominantly caused by fire.
- In Line 311-314, we explain that we used satellite data (2013-2018) from the same period of our empirical tree biomass data measurement for correlation analysis.

Figure 3: really poor quality figure. Font size is small and some texts are blurry.

- The blurry text developed from the pdf-creating process during manuscript submission. We will increase the font size of the figure for better reading in the final version of the manuscript.

L209-212: You already knew forest coverage in 1986 is higher than current period – hence the potential forest area must be larger than existing forest area. That's your hypothesis 4 proved, is it not?

- No, it is not. Hypotheses 4 is about the potential forest area, which is much larger than the forest area in 1986. The advantage of the forest area in 1986 is that its status indicates more sites, which can be covered by forest compared to the situation of today. Thus, the forest distribution of 1986 provides a better database for analyzing topographic and climatic limitations.

L212 – 213: I really am having trouble with relating forest area in 1986 with your predictive variables. You can't assume forest in 1986 was un-disturbed. Fire is part of nature. Additionally, all your ground-based measurements were performed in ~2018. You are assuming vegetation remained unchanged, whereas there are so many factors that already led to changes. Just to name one, CO2 concentration in the air – the CO2 fertilization effect.

- We responded already to several aspects of these objections above. Yes, forest fire and logging may also have influenced the forest area in 1986. This is not a problem for this approach. The only important point is that, due to the larger forest area in 1986, more potential forest sites for statistical analysis of the potential forest area distribution are available. For the spatial analysis of forest distribution, we used topographic parameters, which keep stable over a long time, and we used climatic data, which was normalized for the period 1973-2013.

L214 – 216: What about CO2? There are more advanced modellings (e.g. Maxent) available out there in the literature than this simple approach. Strong justification for the current method is needed. Also, downscaling climate data from coarse resolution to fine resolution means that you have so many small grids with essentially the same climate data. Is this the reason why you didn't see climate effect on vegetation biomass?

- Forest distribution in the Mongolian forest-steppe is not controlled by CO_2 fertilization. It can be expected that CO_2 content in the air is generally evenly distributed over the region. CO_2 is not a limiting factor for forest distribution, and the lack of forest on southern slopes or the upper treeline is not caused by absence of CO_2.
- In Lines 216 to 220, we explained the advantage of the CHELSA V1.2 dataset, which was created using specific physical properties and topographic parameters for downscaling of climate parameters to receive more accurate spatial data at higher resolution.
- The additional resampling to 30 m resolution was done by bilinear interpolation.

L262: up to this point, I don't see any quantitative definition of forest edge/interior. Given that the comparison of edge and interior was a major result in the Abstract, and given that this comparison really depends on the definition of edge and interior.

- In Lines 229-230, we mentioned that we used the outer 30 m belt of trees as edge and the remaining core part of the forests as interior, in agreement with Dulamsuren et al. (2016; 2019).