Authors reply to the interactive comment on "Climate effects on the vitality of boreal forests at the treeline in different ecozones of Mongolia"
of the second anonymous Referee no2

Dear Referee,
thank you very much for reviewing our manuscript especially from the technical point of view. There are many valuable proposals, which we will follow and work on it in the next version of our paper. This will surely help to improve our work. However, it seems that we have not well described some issues. So we rephrased them to make the statements clearer. In the following text, we formatted your comments in italic and we inserted our replies at relevant places.

Ref.: The manuscript by Klinge et al intends to quantify the influence of climate variables on the distribution of different land cover characteristics in Mongolia and particularly focusses on the upper and lower treeline. A land cover classification is conducted based on remote sensing data for the period 1999-2013 and the position of treelines is derived by means of a complex interpolation strategy.

Rep.: We did not perform a complete land cover classification. All the ecological zones we used were gained from maps published by Gunin and Vostokova (2005). These maps are based on longtime field research from Mongolian and Russian botanists. Although there are also landcover maps existing, which were produced by remote sensing classification, we did not use them, because these data are more imprecise than the latter and mostly they do not fit to the question of vegetation zones of our investigation. The only classification process we have done here, is to differentiate between forest or no forest in every single Landsat image. However, this was the first step before manually reworking the previously classified forest polygons by visually checking every Landsat image.

Ref.: (…) However, in my opinion, the manuscript requires some major revisions before potential publications. In the following I will summarize major concerns without going into detail:
1) Manuscript Structure and Terminology: The structure of the text is partly confusing and a clear separation into introduction, data, methods, results and discussion is not clear. I recommend to shorten the manuscript (and avoid repetitions), particularly the introduction, and to focus on information, which is relevant for the remote sensing based analysis. Further, the methods and results include a lot of additional information, which is not supported by the study. Please clearly separate between data, methods and results of your study and other relevant studies (which can be reviewed in the introduction or the discussion).

Rep.: We have rearranged many parts of the text to receive a more consistent structure. Repetitions and less relevant information were mostly deleted. We also tried to shorten the text. However, we had to insert more detailed description where our statements were unclear.
With respect to the journal's scientific orientation the focus of our manuscript is a bio-geoscientific approach to the treeline in Central Asia by a GIS-analysis of different indicators and remote sensing is only one part of it.

Ref.: Further some of the figures are only very briefly mentioned (especially 6 and 3). Please check, if these are needed.

Rep.: Figure 3 it is very important to give the reader a visual idea how the different dataset spatially fit to each other and how the treeline points and the forest boundary was mapped. We inserted more relations to Fig. 3 in the text where it was necessary.
Although figure 6 would make sense to describe the tree species compositions at different subunits, we will delete this information totally from the manuscript. Maybe there is a chance to publish it as supplementary material.

Ref.: In general the figures are in a confusing order which does not follow the structure of the manuscript.

Rep.: You are right. We put Figure 4 backward, so first the treeline maps appear and afterwards the statistical discussion.

Ref.: Finally the language of the manuscript is partly unclear, misleading or imprecise. E.g. the term "trend" means change with time (I feel the authors often mean "spatial gradient").

Rep: You are right; we have changed the term into gradient where we are not talking about temporal developments

Ref.: The same applies for terms like "decreasing" or "increasing", which indicate temporal change (and not spatial variability).

Rep: You are right, we have changed the terms into rise, ascending, enlarging and descending, reducing, where we are talking about spatial patterns.

Ref.: The term "relief parameter" should be changed to "terrain parameter" throughout the manuscript.

Rep: You are right, we have changed the term

Ref.: 2) Land Cover Classification: The paragraph on the classification algorithm is very short. Please give more information on the methods (how many training regions are used, how accurate is the classification, please also quantify the uncertainty)

Rep.: The only classification process we have done here, is to differentiate between forest or no forest individually for every Landsat image. However, this was the first step before manually editing the previously produced forest polygons by visually checking every Landsat image. We compared the mapped forest area with air photos in the basemaps provided by ArcGIS and corrected them where necessary. Producing a map in that way does not enable to create a reliable confusion matrix.
The Land cover classification was adapted from the Ecosystems Atlas of Mongolia (Gunin and Vostokova, 2005), which we now precisely state in the manuscript.

Ref.: 3) Data: Particularly the climate data are only rudimentarily described. Please give more information on the generation and quality of the data set. This is particularly important for precipitation. Personally I am not sure if the data set is able to reproduce the elevational gradient of precipitation, which appears to be an important trigger of the lower treeline. I suggest showing precipitation and temperature maps and (if possible) comparing seasonal and annual data with available station data for a rough quality assessment.

Rep.: We used the CHELSA dataset especially because it considers the elevational gradients and local relief. We think that it should be sufficient to mention the advantages using this dataset for our investigation in the manuscript. A detailed description of the used dataset can be found in the respective publication of the data: https://www.nature.com/articles/sdata2017122. This paper also included a wide variety of validation tests for the spatial and temporal performance of the used data compared to independent datasets. We do not have independent data available for Mongolia, so we cannot perform an independent comparison in this case. We also think that the dataset is sufficiently validated and peer reviewed and therefore would leave the validation for the reader to look up in the respective publication.

It would be no problem to provide any additional climate maps for the publication, maybe as supplement material. However, we would leave this decision to the editors.

We however attached a few figures to underline our selection of climate data:

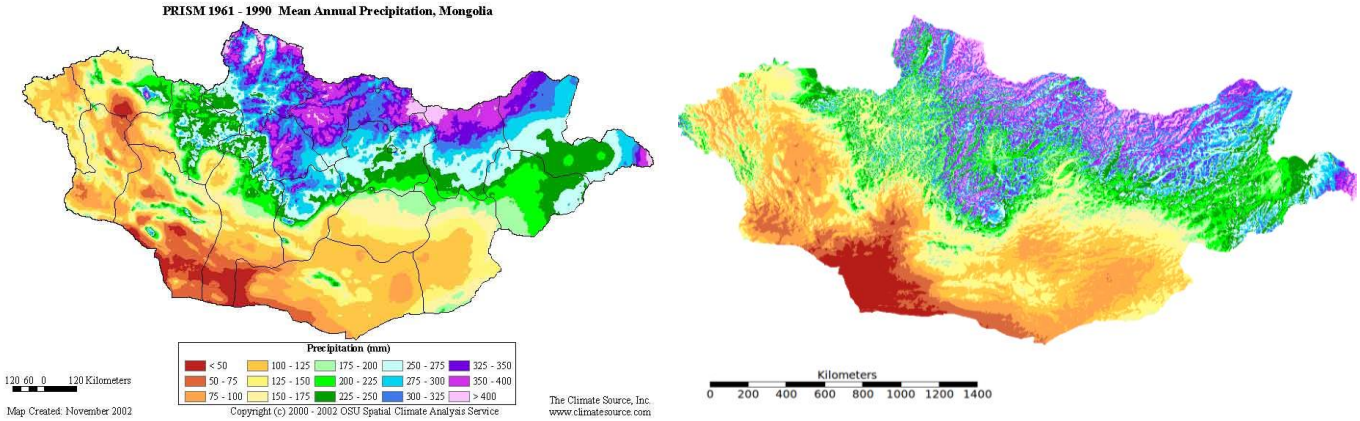

Figure 1: Comparison of PRISM and CHELSA for Mongolia. PRISM is at a resolution of 4km and CHELSA at a resolution of 1km.

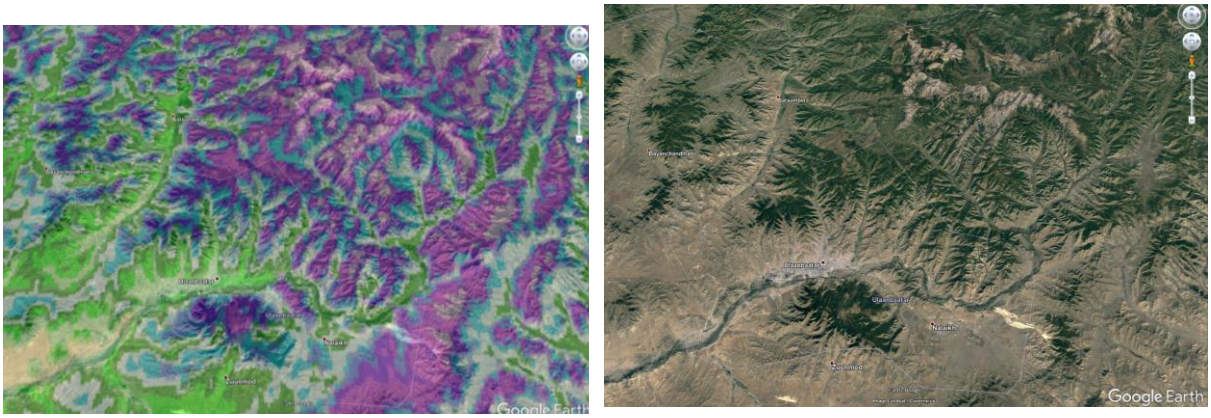

Figure 2: Small scale comparison of mean annual precipitation and Forest occurrence in the region of Ulanbatar. Visible are the dryer valleys and wetter mountain slopes. The lower Treeline in this region is at around 1650 m, what would correspond to a mean of 240mm of annual precipitation (Figure 3).

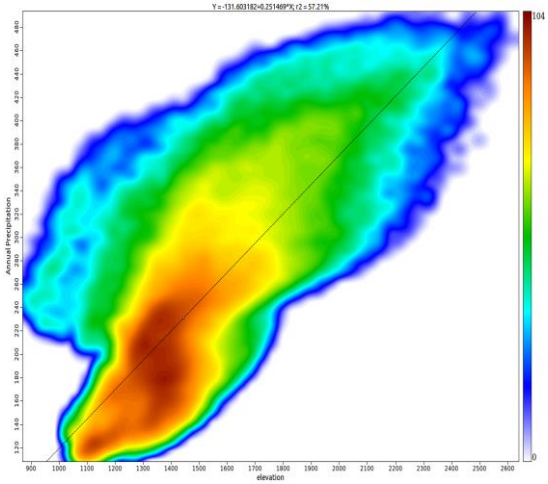

Figure 3: Annual precipitation vs. elevation in the region of Ulanbatar.

Ref.: Further the generation of solar radiation data is not clear to me? Is it solely based on the DEM or is any atmospheric information included?

Rep.: It was simple calculated from the DEM with a GIS-tool. We added this information to the text

Ref.: 4) Ecozones and land cover classes: From my understanding, the larger scale "ecozones" are driven by climatic conditions (just like the land cover classes derived from remote sensing data). A habitat of a particular vegetation type (class) should actually occupy a certain climatic niche (regardless of the large scale "ecozone"). Please clarify why "ecozones" were used to separate the data.

Rep.: You are right with the definition of the term "ecozones" and we changed the term all over the manuscript. At least we are focusing on the boreal forest ecosystems, so we change the term "ecozone" to "type of boreal forest" and the class "Total ecozone" to "Total ecosystem unit" throughout the entire manuscript.

Ref.: Further, the results indicate different statistical relationships between treeline patterns and climate variables in different zones (inverted relationships, e.g. l.369ff). I wonder if there could be any ecological mechanism behind or whether this is rather a statistical artefact (especially since there is only one ecozone, which is mainly covered by forest).

Rep.: There are different ecological mechanisms at different types of boreal forest. This is shown by different tree species composition and by different physical adaption of individual trees.

Ref.: 5) Statistical Methods: The statistical methods for the analysis of climate-landcover relationships are parametrical, i.e. they require normally distributed input data (spatial data depend on the terrain and are certainly not normal distributed). This is relevant for the assessment of significant differences (Tab. 1) and also for the linear regressions (Fig. 4). I feel, the use of the regression is appropriate (since only the direction is discussed in the text), however, limitations should be clearly stated. Table 1 could be shown as boxplots and significance testing should be avoided.

Rep.: As you can see in Figure 4 all of the data is mostly normally distributed. We added information about the restriction of the regression analysis.

Ref.: 6) Spatial Interpolation of treeline elevation: The treeline is (as the analysis shows) highly influenced by local scale climate conditions. Thus the spatial interpolation seems to be misleading to me. I recommend illustrating the treelines as polygons.

Rep: Spatial interpolation of the treeline as surfaces is standard method in Geography.

Ref.: Further the potential anthropogenic influence on the treeline location should be elaborated.

Rep.: We reported human impact on treeline and forest distribution as tree cutting, forest clearing, fire setting and wood pasture since long time.

Ref.: Further in Table 3 spatial ratios of forest cover are calculated for the lower and upper treeline. I do not understand, what these ratios mean (the treeline is actually not an area, but an ecological border)?

Rep: The constitution of boreal forest in Central Asia is strictly differentiated between more or less closed forest stands and grassland areas. Although the treeline may be visually a line on a small scale local view, depending on topographic effects, human impact and reproductive processes it covers an area of advancing and retreating forest. From the geobotanical view the fragmentation of forest stands is a main ecological factor for resilience in the ecotone. is As spatial boundary the treeline covers a distinct area where forests extend or reduce simultaneously over a short distance.

Ref.: 7) Treelines and climate change: The authors use the spatial pattern of treelines and spatial variations of NDVI in order to discuss the potential impact of climate change on tree growth. For me this link is not trivial and should be better investigated. I recommend to also analyze the temporal variability of NDVI, e.g. with respect to warm, cold, dry or wet years during the observational period.

Rep.: Doing the analysis in the proposed detailed way is a method to be used at a more local scale and does not fit to our regional processing.

The spatial distribution of forest and treeline does not react to annual or seasonal extremes, they integrate a longer period. The NDVI of trees is furthermore strongly controlled by soil site conditions. This fact we have discussed in the manuscript. From field dendrochronological investigation we know that trees can suffer for several decades after one year of drought. How do indicate such an event exclusively by NDVI trend analysis?

On the one hand we had to generalize our data with respect to the broad investigation area; on the other hand more detailed analysis needs more precise data in spatial and temporal resolution, which does not exist. Thus our approach meets the most comprehensive method to delineate climate limitations at a regional scale. Moreover, modelling climate projections does not provide annual or seasonal data; it can only produce trends for periods. Therefore our “rough” data fits to the possibilities of these scenarios.

Ref.: Further, the manuscript contains very little information about climate change scenarios (and potential changes of temperature and precipitation) for Central Asia. This would be an important basis for the discussion. Once again, please focus on results, which are really supported by your study! E.g. sentences in l. 491/492 or 498/499 (but also others) are very speculative and not proven by the analysis or by literature.

Rep.: We added some information about climate change scenarios at those places, where it was necessary to substantiate our statements for potential developments in the future. However, it was not our aim to make any forecast into the future. We only want to point out that our results can be used by modelling future and former climate and environmental conditions by the distribution of boreal forest as proxydata. Our main aim was to focus on the recent forest under the climate, which can be seen in the field as well as in the remote sensing data, to show that the results of our investigation fit well to those from biologist and geomorphologist.