Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2017-220-RC2, 2017

Biogeosciences Discussions

Interactive comment on "Climate effects on the vitality of boreal forests at the treeline in different ecozones of Mongolia" *by* Michael Klinge et al.

Anonymous Referee #2

Received and published: 15 December 2017

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. Climate statistics (based on the recently established CHELSEA data set) are shown for each land cover unit

Firstly I would like point out, that I am not a Biogeo-Scientist. Thus I review the manuscript from a technical and statistical point of view. In general, the target of the study is timely, since Central and High Aisan forests are highly affected by climate change and remote sensing data analyses of treelines are rare. 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). Further some of the figures are only very briefly mentioned (especially 6 and 3). Please check, if these are needed. In general the figures are in a confusing order which does not follow the structure of the manuscript. 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"). The same applies for terms like "decreasing" or "increasing", which indicate temporal change (and not spatial variability). The term "relief parameter" should be changed to "terrain parameter" throughout the manuscript.

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)

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 to show precipitation and temperature maps and (if possible) compare seasonal and annual data with available station data for a rough quality assessment. 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?

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. 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).

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.

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 to illustrate the treelines as polygons. Further the potential anthropogenic influence on the treeline location should be elaborated. 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)?

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. 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.

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