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Interactive comment on "Modelled potential forest area in the forest-steppe of central Mongolia is about three times of actual forest area" *by* Michael Klinge et al.

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Reply to the interactive comments of Marijn Van de Broek to our Discussion paper: "Modelled potential forest area in the forest-steppe of central Mongolia is about three times of actual forest area"

Thank you very much for your interest in our research and the valuable remarks on the manuscript. In the following, we picked the main questions from your interactive comment and reply to them:

"As such, the study not only raises the question of the forest's areal potential and

present extent but also its biomass, factors, such as edge effects or permafrost, influencing the former two, the influence and history of fire as well as human disturbance. Additionally, each aspect is accounted for in multiple forms and categories (e.g. forest size, dominant vegetation). Therefore, we would advise to rethink the title of the manuscript and more precisely formulate the intended goals".

Yes, this is true and actually, the first working title of our manuscript was: "Effects of geoecological parameters on forest distribution and tree biomass in the forest-steppe ecotone of central Mongolia", which surely covered more aspects, but no information on the outcomes of our study. More and more journals expect that the title includes also the main outcome of the research. At the same time, it must be concise. Thus, we decided to concentrate on one dominant aspect, which was to proof the potential for more extensive forest area.

"The study area is clearly described and Figure 1 gives an overview of where the study area is located. It would be helpful to indicate the exact location of the study area (the northern slopes of the Khangai Mountains) and to label all mentioned locations on the map."

Figure 1b) clearly shows the position of the study area on the northern edge of the Khangai Mountains, and Figure 1c) represents the total study area, which is comparable to the maps in Figure 4 und Figure 10 in the Results chapter. Since it is a very remote area, there are only few locations to be mentioned. The Khangai and the only town Tosontsengel are shown in Figure 1. The label for Ider Gol is missing. We will insert it in Figure 1c).

"Further, the temperature and precipitation are mentioned only at Tosontsengel. We wonder how big the temperature difference within the study area is, which is located on a higher altitude"

Unfortunately, only the one climate station at Tosontsengel exists in the study area. The distribution of climate conditions in the forest area during the growing season is shown

in Figure 9. We can add information on the total range of precipitation and temperature in the chapter Study area. We can also add climate maps in the appendix.

“(1) The role of the auxiliary data named in the text, such as ground vegetation structure, soil profile and detection of permafrost, could require some information about its use.”

Thanks for mentioning that the use of these data is apparently not clear to the reader. We will add some brief explanations on it.

- Ground vegetation structure was important for checking, which portion of the NDVI of the forest sites was contributed by ground vegetation, because tree canopy closure is less than 53%.

- Soil profiles were used to distinguish between soils developed in sandy sediment or slope debris. Furthermore, soil pits were necessary to proof the permafrost distribution.

- Permafrost is a crucial edaphic factor for forest distribution due to its function as soil water reservoir. It is not part of the biomass and potential forest area modelling, but its ecological feedback needs to be discussed as a secondary parameter.

“(2) The selection of the reference year is not clear to us; there are no sources or measurements or data, which states that 1986 is a year of transition from “few fire events” to “extensive ones”. Could be interesting if some indications about the selection of this year are provided.”

We will try to make this clearer: The information about increasing fire-events after 1990 derived from two sources: We worked together with local authorities from the Tarvagatai National Park, who told us that the forest fires became more frequent since the 1990s, whereas the most severe ones occurred in 1996 and 2002. Goldammer (2002) reported that during the Soviet period, a team of fire fighters with an air fleet was established since 1969, but financial support from the Russian side ended in the 1990s and finished the project, resulting in the first extensive fire event in 1996. We

will add this information to the introduction chapter to clarify this background. Suitable Landsat satellite images (TM) from the 1980s are rare and the one from 1986 was the best fitting image for the period before the political change and onset of extensive forest fires.

“(3) The authors used the NDVI to highlight relations with the biomass. However, the role of the NDVI is not completely clear. If it serves to demonstrate that the NDVI is not recommended in such situation, which is supported by the results or if it is used to find a relation with the biomass. However, the NDVI is not recommended, since it is strongly affected by other components.”

Despite many known difficulties, the NDVI is still commonly used to delineate biomass. Its suitability depends on the scale and data resolution. As shown by Dulamsuren et al. (2016), it was possible to correlate NDVI with tree biomass at small scale for regional biomass estimation. Here, in our large-scale analyses, we could not proof a statistically significant correlation between NDVI and tree biomass. Instead, here the NDVI can be used to determine the forest vitality (incl. ground vegetation) by excluding the NDVI-biomass relationship.

“(4) The forest distribution analysis was pursued using climatic parameters: mean annual precipitation (MAP), mean growing season temperature (MGST) and mean potential evaporation (MAE). The creation of a potential forest distribution based on climate is very interesting, however, these data used for the analysis are strongly correlated, which may disturb the outputs.”

Yes, this is why in chapter 4.4.2. “PFA delineation based on climatic parameters”, line 379-381, we state that the potential evaporation is closely correlated with temperature and thus, we did not use it for modelling. Temperature and precipitation are independent parameters. Temperature follows a vertical gradient, and precipitation shows an additional longitudinal gradient caused by the main airflow (westerlies). Considering these local differences is the main advantage of the CHELSA dataset.

"(5) About the classification of site condition, it is well explained how the data are retrieved and how they are divided and used. However, all the classes were displayed together in Figure 4, which results quite chaotic and difficult to distinguish between the type of forests and/or the soil type. Maybe the removal of some classes in the image could improve the display."

Thanks for this comment. If it is too confusing, we can subdivide the information into two different maps.

"The first part (4.1) indicates the general distribution of different factors. However, these factors could better be described in two different subparts, as the burnt areas and permafrost areas were examined in very different ways and thus are better presented separately"

Yes, we can divide the two paragraphs into two different chapters "4.1.1. Burnt areas" and "4.1.2. Permafrost distribution".

"To a certain degree, we are not sure about the accuracy of the determination of total tree biomass with the chosen method, as on the one hand the authors reject the NDVI method due to low correlation but on the other hand you just use another method which might also induce an unknown error."

The statistical variance of measured tree biomass between different forest sizes and types is shown in Table 1 and Figure 5. The results indicate the (small) range of error, which can derive from the interpolation via spatial extension. Using the NDVI would only add a further source of error in the biomass modelling process.

"Furthermore, the authors present the different factors influencing forest distribution and size, with a focus on the importance of fires and permafrost. The discussion of the latter would profit from more literature and data to back it up." . . . "Additionally, we suggest revisiting the topic of climate change and the steppe's sensitivity, first mentioned in the introduction. In this context it would also be interesting to discuss the probability

of forest establishment to full potential, for example after decreases in permafrost".

Literature on permafrost distribution under the influence of logging, forest fire and climate change in the Mongolian forest-steppe is rare. Permafrost development and climate change was not in the focus of this study, which concentrates on forest distribution and biomass. We can nevertheless add some more information and references on this topic in the final version of this paper.

"We wonder, however, why windthrow was not included in the list of disturbances such as logging or fire".

We indeed observed windthrow inside the forests. However, windthrow occurred as individual deadwood inside the forests. Lying deadwood was abundant in some forests, but it was scattered and did not cause major clearings. Extensive windthrow occurs after intense logging has opened the forests and decreased the tree density. Thus, we conclude that windthrow is not an initial factor of forest disturbance on its own.

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