- **CC1**: 'Comment on bg-2023-148', Leanne van der Kuijl, 11 Nov 2023

This review was prepared as part of graduate program course work at Wageningen University. The review was not solicited by the journal, but it might be of some use.

Your review will certainly be of substantial use, and we thank you for careful reading and many good comments and suggestions.

This paper uses high resolution simulations to assess changes in vegetation composition, biodiversity, and available reindeer forage in the 21st century due to climate change and potential reindeer grazing in the Fennoscandian region. The authors parameterized the LPJ-GUESS dynamic vegetation model with the PFTs found in the region and added a reindeer grazing module. The model was forced using a downscaled high-emission climate scenario (RCP8.5). Validation on local scale was done using vegetation inventories and on regional scale using remote sensing maps. The vegetation shifts were analyzed in more detail in six diverse "hotspot" areas. The results show dramatic south to north and low to high altitude shifts in vegetation zones, that accelerate towards the end of the 21st century. Potential reindeer grazing ground will also shift to the north based on the availability of suitable forage, but other factors resulting from climate change are likely to impact reindeer husbandry more. The authors conclude scenario-based research is needed to better assess the vegetation changes in the future and their uncertainty.

As far as I know this paper is the first to attempt to integrate the effect of reindeer grazing, which is an important landscape forming factor in Fennoscandia, into a dynamical vegetation model. This paper is also the first the use a very high-resolution climate scenario, which better captures the local variation in complex terrain, on the entire Fennoscandian boreal and Oroarctic region. The research is necessary and valuable in showing the severity of the consequences of climate change for nature conservation and for the conservation of the indigenous culture of the region (reindeer husbandry).

The study seems well designed, despite only using one climate scenario, and the methodology, calibration and validation seem to follow generally accepted protocols for dynamic vegetation modeling experiments. Additionally, the figures in the paper visualize the results well. However, the methodology is hard to read, and the introduction is missing some key arguments as to why the study is important. I also would like to see some processes and effects that were not considered in the model added to the discussion, but most importantly, I feel like the uncertainty of this study needs to be stated more clearly in the summary and the conclusion.

We thank you for your positive words and hope that the problems you identified will be solved when we have dealt with the specific comments.

In my opinion this paper will be suitable for publication in Biogeosciences after moderate revision.

Major arguments

The model used in this study is only forced using one (extreme) climate scenario and there is a lot of (unquantified) uncertainty in the results. The authors do already state in the discussion that this uncertainty exists and could not be solved due to computational restrictions and because there currently are no different climate scenarios at the high resolution of this study available. Despite this, the study is still a relevant and necessary first step. However, I do feel it is very important to mention the uncertainty of this study more clearly in both the conclusion and the summary to avoid sensationalizing the results. If I were to read only the title, summary, and conclusion of this paper, I would not know this paper only describes general trends. Especially in the summary that states this region **will** be completely covered by forests at the end of the 21st century.

The conclusion and summary will be revised to clearly state that this is a high-emission scenario. Some of the uncertainty discussion will also be moved to the conclusion.

In my opinion both the methodology and introduction require some additions to make it easier for people to find the relevant information and to clearly express the relevance and necessity of this study:

The modelling methodology is in my eyes poorly described and rather wordy. It is unclear what model is used and where a detailed description of this model can be found. I had to consult additional literature to try and figure out what model was used (Gustafson et al., 2021; Smith et al., 2001; Miller and Smith, 2012; Smith et al., 2014) and I am still not sure, because Gustafson at al. (2021) only mentions LPJ-GUESS v4.0. This could be remedied by adding a schematic overview of the model highlighting which parts of the model are new (reindeer module and some PFTs) and where the detailed descriptions for the old parts can be found.

We will clarify what model was used and where it is described in detail. We don't intend to add a schematic overview, but do describe clearly the new features and parameterizations you mention.

Additionally, it is unclear which data was used to calibrate which parameters of the model. For example, lines 143-147 on page 3 mention test runs and fine-tuning to get a better match against distribution maps from observations, but it does not mention what maps. I feel adding a (supplementary) table of which data sets were used to calibrate which parameters (covering what time period) would solve this problem and prevent people having to comb through the entire paper to figure out what was done.

As also pointed out Referee #1 and #2, we admit that it is not clearly expressed. We will revise lines 143-147 to "For the IBS plant functional type some parameters were changed according to Gustafsson et al. (2021) in an application for Abisko, Sweden. Their revision was made to reflect the fact that the global IBS PFT in Fennoscandia mainly represents mountain birch (*Betula pubescens* ssp. *tortuosa*). Details of the IBS parameterization are found in Table S2." and add this text to S2 "Using the default parameters, test runs with a sub sample of gridcells showed a substantial underestimation of deciduous broad-leafed forest adjacent to the mountains, where it is represented by the Shade-Intolerant Broadleaved Summergreen tree plant functional type (IBS) in the simulations, when compared to the satellite-based products (see section 2.5.1). We therefore tested the IBS parameters from Gustafson et al. (2021), who had adjusted them to more specifically represent the small tree mountain birch

(*Betula pubescens* ssp. *tortuosa*) and calibrated it to grow and compete as expected for the Abisko area (Gustafson personal communication). Simulations with these parameters on the other hand resulted in a too large extent of deciduous broad-leafed forest and to reduce the competitiveness of IBS the original values related to shade tolerance and turnover were used instead. For the same reason, we also adjusted the alphar and turnover_sap parameters." In Table S2 we will then also add the parameters that were not changed according to Gustafsson and add a column so that there will be "default 4.1", "Gustafsson et al. (2021)" and "used value". Except for this there were no calibration done, and we don't think any table more than Table S2 will be needed.

In the introduction the paper is vague on what the consequences of a shift in vegetation composition might be and how reindeer grazing might affect and be affected by this shift. It just mentions increasing pressure on both ecosystems, holding species of great ecological, biological, and societal significance, and societies in the area. It seems to me that the consequences of such a shift in vegetation due to climate change are the most important reason for doing this research. I feel it would be beneficial to be more specific or perhaps add some examples to get the message across better. Stark et al. (2023) gives a lot of information about effects of reindeer on the ecosystem. It is also worth mentioning how culturally important reindeer husbandry is for the indigenous people of the region.

We will explain consequences of climate-change related shifts in vegetation composition to show that this research is important using the Stark reference and others, and will mention the cultural importance for the Sami. We will also more clearly express the task, objective and hypothesis of the study in the end of the introduction.

Building further on this point: The summary and conclusion mention more about the consequences than the introduction does. The conclusion for example mentions implications for recreation when this is not mentioned anywhere else. These extra points should be moved to or already be mentioned in the introduction.

As mentioned above we will mention aspects such as recreation in the introduction.

I would also like to see more discussion about processes and effects that were not considered in the model. The following points being most important:

Currently only one sentence in the paper (line 607) mentions seed dispersal capacity (and fire disturbance) as factors that may restrict vegetation expansion, particularly for predicted shrub expansion on non-shrub tundra. However, as mentioned by Gustafson et al. (2021) dispersal limitations are likely to cause lags in range shifts on larger spatial scales (Rees et al., 2020; Brown et al., 2018). Models that do account for seed dispersal limitations generally predict slower latitudinal tree migration than models driven solely by climate like LPJ-GUESS (Epstein et al., 2007). This warrants further explanation in the paper.

We will extend the discussion with possible consequences of not accounting for seed dispersal rate, including some of the suggested references as well as Zani et al (2022, https://doi.org/10.5194/gmd-15-4913-2022).

This paper discusses the direct effects reindeer grazing, browsing, and trampling might have on the vegetation and how the change in vegetation due to climate change might affect the food supply for the reindeer, but it does not discuss the effect reindeer might have on the climate and thus indirectly on the vegetation as well. Recently Holmgren et al. (2023) found that a high amount of reindeer summer trampling in low peatland areas may result in increased summer warming and decreased winter cooling enhancing permafrost degradation in these areas. On the other hand, in higher areas intense browsing and nutrient addition from reindeer may mitigate some climate warming effects (Macias-Fauria et al., 2020; Malhi et al., 2022). This uncertainty should be added to the discussion.

We will add a discussion about possible climate feedback from reindeer grazing.

Furthermore, it should be noted that the reindeer exclosure sites that are used to validate the reindeer grazing module likely do not represent the natural situation without grazing (Stark et al., 2023).

We will acknowledge this.

Soil vertical and horizontal movement caused by frost, and amelioration of such effects in the warmer future climate are not accounted for in the LPJ-GUESS model. These processes could affect survival and competition among the plant functional types, especially in the seedling stage when plants are most vulnerable to mechanical disturbance (Gustafson et al., 2021). This should also be added to the discussion, if relevant on high resolution larger scale.

There are a lot of simplifications when it comes to considering processes that affect the establishment of new plants; the mentioned soil processes, seed predation, plant browsing and killing by smaller animals like rodents and hares, deceases, snow damage, moose, etcetera. We will mention this in general terms, but an extended discussion of all these aspects would take too much space.

Minor arguments

1. Page 3, lines 104-105: Why was this study area chosen? The summary, introduction and title make it feel like the entire Fennoscandian area will be studied, when it is not. I feel like some explanation is necessary here.

In the introduction we clearly state that we address the “Fennoscandian boreal and Oroarctic region” (line 54, 89, 96). That the study is also restricted to the area used for reindeer herding is explained in M&M, but we will add this information also to the introduction (line 96). The eastern border in Finland, where a small part that could have been included is outside the assessed area, is due to the fact that we for computational reasons wanted to restrict the size of the complete 1985-2100 NetCDF climate files to less than 32 GB per climate variable, we will include this information in section 2.3.

2. Page 5, line 128: The return time of patch destroying disturbances (e.g., devastating pests or windstorms) is set to 150 years. What is this based on? Does this also include fires? This requires some explanation.

Fire is simulated separately, we will add information on that. 150 years is the default return time for arctic simulations in LPJ-GUESS 4.1, we will add that information as well.

3. Page 6, lines 158-159: Barthelemy et al. (2018) says that nitrogen in the form of urea can easily be taken up by plants directly (as in without transforming it, not as in how quickly). The way it is currently formulated in this paper makes it feel as if the assumption is that the nitrogen is immediately (time) taken up by the plants. Barthelemy et al. (2018) does not seem to state how quickly this happens other than that it happens 'rapidly'. Urine is also a very local phenomenon, but for the model it is taken up by plants in the entire patch.

We agree that "directly" is a misleading choice of word and will change to "rapidly" both here and in the supplement (line 56).

4. Page 7/8, section 2.4: Is the nitrogen data at 12 km resolution detailed enough for this study with a resolution of 3 km? This is not mentioned anywhere yet.

We have done a sensitivity test using the more coarsely resolved 0.5° Ndep data and compared it to simulations using the high-resolution data, and found very small differences in simulated vegetation composition. We will add this information but not provide specific results.

5. Page 15/16, section 3.2.2: I wonder what the effect is of leaving out the wetland areas in the calculation of the Shannon Diversity Index on the comparison between the different time periods. Specifically in Muddus, which has a lot of wetland area in the simulated data, but barely any in the satellite data. This should be mentioned.

We explain in M&M that we exclude cells that do not have a dynamic vegetation class like water and wetlands, but we will add this information also to the table text of Table 3. We will also reformulate the sentence of line 416-417 to "It should also be noted that the bog class, which covers a large fraction in Muddus, is excluded from the calculation of *D* as it is prescribed and not dynamic. Including it would increase *D* but we would not be able to assess the change over time.".

6. Page 22, line 556: I feel it would be better to mention just how much larger the reindeer herd size is in summer (60% larger after calving and before slaughter, Definitions - Sámi Parliament (sametinget.se)), because this could result in a significant underestimation of the grazing.

We can't see where the number 60% comes from, according to https://sametinget.se/statistik/renslakt (in Swedish) the number of slaughtered animals is 40 000 – 75 000 reindeers, which rather means that the summer population is about 25% larger. The animals that are slaughtered are also often calves that eat less. We already acknowledge that there is an underestimation in the current formulation. We don't intend to revise.

7. Supplement page 4 (S3), line 38: Please add and explanation as to why the Swedish reindeer population is (assumed to be) representative for the entire study area.

We will add the information that we will use Sweden as an example for verification. We write in line 105 in the main text that the focus of the study will be on Swedish ecosystems.

8. Supplement page 4 (S3), lines 53-59: This does not explain why 35% was used. Ferraro et al. (2022) assumes that 38% of the daily nitrogen consumed by reindeer is assimilated into its body mass and 62% is defecated (not all as urine), instead of the other way around. McEwan and Whitehead (1970) show that nitrogen divers a lot depending on the age and sex of the reindeer and on the season. Nitrogen retention during the second growth phase (14-24 months) was on average 35% in reindeer (page 909, table IV). The remaining 65% was presumed to be urine. I am presuming the 35% used in this study is based on this, but I am not entirely convinced this is representative for the entire reindeer population. Please explain in more detail.

It is not the other way around, entering the harvest pool is the same as being assimilated in body mass, e.g. being removed from circulation in the ecosystem. To go into detail of the proportion of different growth phases would be overly complicated for this simple model. Based on your recommendations, we will check the text carefully to improve clarity and add a reference.

Minor issues:

Main article

Page 1, lines 30-32: "Simulated … grazing ground." This sentence is unclear and needs to be rewritten.

For clarity, we will split the sentence into two: "Simulated potential for reindeer grazing indicates latitudinal differences, with higher potential in the south in the current climate. In the future these differences will diminish, as the potential will increase in the north, especially for the summer grazing grounds."

Page 2, line 37: Reference?

We will add this reference: Dobrowski et al., 2021. Protected-area targets could be undermined by climate change-driven shifts in ecoregions and biomes, Communications Earth & Environment, 2, https://doi.org/10.1038/s43247-021-00270-z.

Page 3, line 87: "have" should be "has".

"Data" is always plural (datum is the singular form), we will not revise.

Page 3, line 95: Perhaps add a more recent reference for the "state-of-the-art DMV" here like Gustafson et al. (2021). 2014 is not state-of-the-art.

We will add Lindeskog et al. 2021.

Page 6, line 175: "(see below)" What does this reference? There is nothing below.

We will remove this reference as it is not needed.

Page 7, line 182: "two" methods" are announced here, but only one of those methods is explained in the same paragraph and the other in the next paragraph.

We will place a colon after "using two methods" and then start a new paragraph, so that each method has a separate paragraph.

Page 8, line 246: “also” before “converted” should be left out.

We will revise the entire sentence to “Further, modelled LAIs for the specific PFTs within a grid cell were used to determine a vegetation class for comparison to two remote-sensing based vegetation products: the land cover of northern Eurasia (GLCE) based on SPOT 4 at 1 km resolution (Bartalev et al., 2003) and the CLC2018 Corine land-cover dataset (Corine) based on Sentinel 2 at 100 m resolution (https://land.copernicus.eu/pan-european/corine-land-cover/clc2018).”.

Page 13, line 363: “were” should be “was”.

We will revise.

Page 21, lines 524-525: “which is not far from the current trajectory” reference?

We will revise the entire sentence: “Thus, with a continued warming of up to 5 K by the end of the century, which is not totally unlikely as the world is currently on track for a 2.9 K increase in global warming (Ref to UN Emissions Gap Report 2023, https://www.unep.org/resources/emissions-gap-report-2023) and given the Arctic warming amplification (Rantanen et al., 2022), the Fennoscandian vegetation is likely to undergo a rapid shift.”.

Page 22, line 554: either “since” or “as” needs to be left out.

“since” will be removed.

Page 29, line 805: This references the preprint of this paper, not the published version.

It will be fixed.

Supplement

Supplement page 4, line 34: “reduce” should be “reduces”.

We will revise accordingly.

Supplement page 4, line 39-40: add the specific webpage where the numbers used can be found.

The webpage we are referring to specifically provides these numbers, though it is in Swedish.

Supplement page 4, line 40: “280 00” should be “280 000”.

The missing zero will be added.

Supplement page 4, line 41: “eat” should be “eats”.

We will revise accordingly.

Supplement page 4, line 41: I think “path” is supposed to be “patch” here.

Correct, we will revise.

Supplement page 4, line 46: missing “the” between “large” and “fraction”.

We will revise accordingly.

Supplement page 4, line 47: add “is” between “consumed” and “relative” and perhaps add “where” between “and” and “herbivore_int” to make the sentence clearer.

We will revise accordingly.

Supplement page 4, line 55: missing “a” between “is” and “functionality”.

We will revise accordingly.

Supplement page 4, line 55-56: add a reference for the assumption that N leaving the herbivore as urine is directly taken up by the plants.

We will add a reference, see also comment to “Minor argument nr 8”.

Supplement page 7, line 112: The Bartalev et al. (2003) reference is missing in the reference list.

Supplement page 8, line 119: Babst et al. (2014) is not referenced in the text.

By mistake Babst et al. (2014) was inserted in the reference list instead of Bartalev et al. (2003). The mistake will be corrected.

Supplement page 18-19: What do the colours in this figure mean?

We will add an explanation of the colours

References

Barthelemy, H., Stark, S., Michelsen, A., & Olofsson, J. (2017). Urine is an important nitrogen source for plants irrespective of vegetation composition in an Arctic tundra: Insights from a 15 N-enriched urea tracer experiment. Journal of Ecology, 106(1), 367–378. https://doi.org/10.1111/1365-2745.12820

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