Author comment on "Simulating the effects of temperature and precipitation change on vegetation composition in Arctic tundra ecosystems" by H. van der Kolk et al.

Reply to comments by anonymous Referee #2

This is an interesting and relevant studied, which in my opinion deserves to be published in Biogeosciences. The paper presents in a clear and interesting way potential changes of Arctic tundra under warming/precipitation change/permafrost thaw. Especially, addressing 3 factors in combination, i.e. warming, precipitation and permafrost thaw is a relevant contribution to our understanding of tundra change.

REPLY: We thank the referee for his/her constructive comments. We appreciate that the referee recognizes that our study has the potential to provide interesting insights in the response of tundra vegetation composition to climate change, gradual permafrost thawing and abrupt permafrost thaw.

The paper is well written and the results are clearly presented. As this study represents a modelling approach, I would find it helpful if some modelling related issues could be clarified. In particular, many parameters in the NUCOM-tundra model were defined based on e.g. vegetation composition found in the field, so I was sometimes uncertain what I learned in the paper about mechanisms responsible for changes in the tundra.

REPLY: It is correct that some of the parameter values for the model have been derived from field measurements at our study site in north-eastern Siberian tundra, see further reply to comments below. From our field studies we have gained insights in the functioning of (dwarf) shrubs and graminoids which have been used for the model. For example, the graminoids are deeper-rooted than the shrubs which implies that the graminoids are better competitors for nutrients in the deeper soil layers. The model synthesizes own findings from field studies with knowledge on Arctic tundra ecosystem functioning from literature.

Also related to this issue: of course simplifications/assumptions need to be made for a model, especially if access to measured data is limited. However, I asked myself a few times if the simplification were justified.

A few examples. Abstract. L.24. The simulations suggest that shrubs are better light competitors... etc. If I understand the model right, shrubs are good competitors because they were defined as good competitors in the first place. Not that this would be incorrect. But several times I get the impression that findings are not necessarily a result of the model but a result of how the model was set up, which assumptions were made and which data were used to feed the model. Again, this is certainly an issue that can be said for all models. But I think the text needs some rephrasing to be clear about what is indeed a model outcome (e.g. increase of graminoids under wetter conditions) and what is not. To me the text seems to go too far, which mechanisms can actually explained by this model and which cannot. See related comments below.

REPLY: Of course a result of a model depends on how the model was set up, etc. This has the advantage that it is possible to trace back how a certain model outcome has been realised and provides a potential mechanism for the simulated vegetation change. It is correct that the mechanism is not independent of how the model was build and we agree that this should not be overstated. Still, we think that models are a helpful tool for exploring mechanisms of vegetation change.

We disagree that shrubs were defined as good competitors in the first place. Several plant traits (parameters values) are related to competition for light or nutrients. For light competition, leaf area and light extinction are important, which are influenced by parameters as specific leaf area, biomass allocation to leaves, leaf mortality and light extinction coefficient. Similarly, nutrient competition is influenced by parameters as specific root length, root distribution and nutrient requirements. Values for the parameters have been assigned independently, partly based on literature values and partly derived from own measurements. It is the combination of multiple plant traits and environmental conditions that determines which plant type is a good competitor. Based on our field observations, we assumed the (dwarf) shrubs and graminoids to be equally tall in the model. Therefore, the shrubs were not a-priori the better light competitor.

Questionable assumption? p4 l21. Graminoids and dwarf shrubs are assumed to be equally tall. The authors may have their reasons to do so, but this is not entirely clear to me. Betula nana can grow easily 2.5 m tall (e.g. in parts of Alaska) and arctic graminoids don't. An incorrect assumption here could have a large influence on the results.

REPLY: Based on observations at our study site in north-eastern Siberian tunda we assumed the graminoids (*Eriophorum* spp.) and dwarf shrubs (*Betula nana*) to be equally tall, which is a simplification, but not unrealistic. Also in Alaska, in moist tussock tundra as in Toolik Lake, dwarf shrubs and graminoids are equally tall (Heijmans, personal observations). We acknowledge that in some other tundra areas the shrubs, including *B. nana*, can grow taller than the graminoids, although this may be limited to places with some additional nutrient supply. For a wider application of the vegetation model, i.e. to include transitions to tall shrub vegetation, a variable plant height up to a maximum would be required.

Explaining mechanisms? P. 9 l16ff. The authors state that the NUCOM model was developed to assess which mechanisms are responsible for tundra change. I found this statement somewhat questionable because many very important mechanisms remain unknown when assumptions are made for models. The biomass example above is one such example. The issue that rooting depth in a warmer climate is not known is another example, but discussed later in the discussion. It might be helpful if the authors adapt their wording a bit. E.g. that they refer mechanisms to effects of warming vs. precipitation, which is the novel contribution of this paper.

REPLY: The focus of this modelling study was on exploring potential vegetation shifts in response to changes in temperature, precipitation and permafrost thaw, as is evident from the abstract. We agree that in parts of the main text the wording is a bit misleading with too much emphasis on mechanisms. We will go through the whole text and adapt the wording where needed.

So I suggest that the authors go through the entire manuscript another time and re-think carefully how to not over-sell their results. Apart from that, to state this again, I like the paper and find it helpful and novel.

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