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Interactive comment on "Biogeophysical feedbacks enhance Arctic terrestrial carbon sink in regional Earth system dynamics" *by* W. Zhang et al.

Anonymous Referee #2

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General comment:

Zhang et al. used the regional model RCA-GUESS to simulate biogeophysical feedbacks in the Periarctic region. In particular, the authors focus on the carbon cycle and on how the biophysical feedbacks could potentially affect the carbon stocks in high latitudes in the context of a global warming. The authors effectively present interesting results, showing how biogeophysical feedbacks in high latitudes are important in order to determine the land carbon sink and the vegetation distribution.

The article is generally well written, and the authors are clear in explaining the results which are well supported by images and tables. On the other hand there are some few

general comments that require a revision, as these issues should be addressed in a more complete discussion, which would explore the limitations of the study.

The main issue is the lack of discussion about the importance of the representation of permafrost and freezing/thawing processes in the RCA-GUESS model and the lack of discussion of this potential limitation in the paper. It is not clear how the model used in the present study can deal with permafrost-related processes. This issue is particularly relevant in the region of focus in the present study, and the implication of this model feature should be addressed. Permafrost and permafrost-related processes are fundamental drivers of hydrology, energy balance and carbon cycle in the Periarctic region. Some of these issues are well discussed, e. g., by Koven et al. (2011), Permafrost carbon-climate feedbacks accelerate global warming. PNAS, doi:10.1073/pnas.1103910108. The influence of permafrost-related processes can also affect the size of the terrestrial carbon sink (due, for example, to permafrost-related processes, such as cryoturbation). The magnitude of the permafrost-climate feedback should therefore not be underestimated, and the lack of representation of these processes can deeply affect the results published in this paper. In particular, the magnitude of the effects of the biogeophysical feedbacks the authors find are in the same order of magnitude of the permafrost-carbon feedback recently found in MacDoughall et al. (2012), Significant contribution to the climate warming from the permafrost carbon feedback, Nature Geoscience, doi:10.1038/ngeo1573. I understand that the biogeochemical effects are not the focus of the paper, but their presence/absence should be considered in a more complete discussion.

Other issues/questions to be addressed:

The article presents results from simulations of a regional model forced with boundary conditions from climate projections under the RCP 8.5 scenario. Why did the author choose only this scenario for the boundary conditions?

In the 6 PFTs described in Section 2.1 there is no mention of mosses/shrubs, which on

the other hand is relevant to the region of focus. Could the differences in albedo with C3 grasses and the insulating effect of mosses potentially affect the results the authors show in the paper?

What is the role of the competition for nutrients in this study? Are N and P cycles considered? How would the results of the simulations without dynamical vegetation change by considering the nutrient cycles? Goll et al. (2012), Nutrient limitation reduces land carbon uptake in simulations with a model of combined carbon, nitrogen and phosphorus cycling, Biogeosciences, doi:10.5194/bg-9-3547-2012 showed how relevant is the impact of nutrients for the estimation of the land carbon sink. Once again, I understand that this one is not the focus of the paper, but this other potential limitation should be mentioned in the discussion.

Specific comments:

The paper is generally well written, and I have only very few comments on the text.

Page 6716, line 11: “an GCMs CMIP5. . .” should read “a GCMs CMIP5. . ."

Page 6719, line 9: I am not sure about the citation of Rietkerk et al. (2011). The authors of that paper showed how small-scale vegetation-climate interactions could potentially affect larger scales, and they referred to small-scale soil and vegetation features and to their effect on land-atmosphere fluxes. These small-scale features are not resolved in GCMs, but neither they are in RCMs, despite the increase in resolution. This scaling issue is indeed of interest, and it could also be addressed in the discussion. The use of a RCM, though, in my opinion does not address the small surface heterogeneities, such as the ones highlighted by Baudena et al. (2013), Vegetation patterns and soil-atmosphere water fluxes in drylands, Advances in Water Research, doi: 10.1016/j.advwatres.2012.10.013 for drylands, and for the Arctic environment by Cresto Aleina et al. (2013), A stochastic model for the polygonal tundra based on Poisson-Voronoi Diagrams, Earth System Dynamics doi:10.5194/esd-4-187-2013. In this way the citation is misleadig, as this issue of scales remains unresolved also in the

approach used by the authors.

Page 6720, line 2: “CO2 fluxes measurements” should read “CO2 flux measurements”

Page 6720, line 23: I guess that adding a noun near “This” would improve readability. Maybe “This effect”?

Page 6721, line 9: Something is missing after “but”. Maybe “but we made this choice in order to...”?

Figures:

The figures are generally understandable and well discussed in the text, but:

In Figure 4 (c) colors are not consistent with the other two panels.

In Figure 9 (b) and (c) lack of labels on the x axis. At least the units (which are in the label) should be shown. In the figure label I do not understand the sentence "...for both the peak C uptake rate ...". I guess something is missing here.

Interactive comment on Biogeosciences Discuss., 11, 6715, 2014.