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Interactive comment on “Carbon cycle uncertainty in the Alaskan Arctic” by J. B. Fisher et al.

Anonymous Referee #3

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Carbon cycle uncertainty in the Alaskan Arctic - by J. B. Fisher et al.

The main aim of this paper is to estimate the recent carbon balance and its uncertainty of the Arctic in Alaska. To achieve that a multitude of bottom-up model results, mainly from prognostic terrestrial biosphere models have been used.

In the present form I cannot agree with a publication of the manuscript based on two main points A and B:

A) Comparing the manuscript with the already published carbon balance and uncertainty estimation by D. McGuire and colleagues (Biogeosciences 9, 3185–3204). I cannot see the added value of the manuscript that would merit a publication. My reasoning is explained in more details in points 1 and 2 below.

1. When focusing on a small region like the Alaskan Arctic it makes no sense to me to include as many models as possible into a statistical analysis of result regardless

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of the processes that are represented by these models. In fact, results by models that include permafrost-specific processes should be more valid. Hence, I would strongly recommend grouping model results according to process representations. McGuire et al. also include TRENDY model results into their analysis but differences to the more appropriate models for this region are visible and discussed.

2. Using both, bottom-up and top-down approaches, McGuire et al. provide a more comprehensive analysis of the Arctic carbon balance and its uncertainty. McGuire et al. provide a break-down into regions with North America being the smallest kind of scale. It needs to be much more motivated what do we need a new paper with a particular focus on Alaska only?

B) There is a general mismatch between different sections of the manuscript (abstract, introduction, results, discussion, conclusion) in terms of aims of the paper and analyses done. See my comments 1-3 below.

1. The objective of the paper was to identify structural versus parametric uncertainty of the models (introduction). Maybe I overlooked a substantial part of the results but I cannot find this kind of analysis. I would assume that parameter uncertainty is assessed by a kind of Monte-Carlo simulation run, maybe at site level? Instead, the authors discuss the uncertainty coming from different forcing data and using a different spin-up procedure in comparison to structural differences.

2. The beta-gamma-analysis of results is not motivated in the introduction and is not included into the discussion, e.g. compared to already done analyses of this kind.

3. The aim of the site-level comparison is unclear. Was the aim to show the reliability of different concepts and assumptions (model structure) or different parameter values? Was the aim to use site-level evaluation results for a weighted average of regional-scale carbon balance results?

4. Most of the conclusion is a repetition of aims and methods but no conclusion about

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the main objectives of the paper is given.

BGD

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