Review of "**Quantifying Soil Carbon Accumulation in Alaskan Terrestrial Ecosystems during the Last 15,000 Years**" by Sirui Wang, Qianlai Zhuang, and Zicheng Yu, revised version.

In their manuscript, the authors present a model study of soil carbon accumulation in Alaska over the last 15000 years with a special focus on peat carbon accumulation.

Compared to the authors' original submission I find the manuscript improved. However, a few issues remain.

This review of the revised version was, by the way, hindered by the fact that the "track changes" version of the manuscript changes was different from the submitted revision, therefore not showing the actual changes to the manuscript. I would also suggest to the authors to use the "compare documents" function the next time, since that shows differences between versions more clearly.

In my first review, one of my points was "Page 4, lines 94-95: the Spahni et al. Model has actually been evaluated with respect to the variables listed – see Wania et al. Publications on the LPJ-Why model on which Spahni is based." The authors have changed the passage I indicated to "In contrast, Spahni et al. (2013) used a dynamic global vegetation and land surface process model (LPX), based on LPJ (Sitch et al., 2003), imbedded with a peatland module, which considered the nitrogen feedback on plant productivity (Xu-Ri and Prentice, 2008) and plant biogeography, to simulate the SOC accumulation rates of northern peatlands. However, the model did not consider methane dynamics, which play an important role in affecting peat carbon dynamics, presumably due to its inadequate representation of ecosystem processes (Stocker et al., 2011, 2014; Kleinen et al., 2012). Furthermore, climatic effects on SOC were not fully explained."
Obviously the authors did not actually read the literature. The LPX model does indeed consider methane dynamics (Spahni et al., Biogeosciences, 2011 and Zürcher et al., Biogeosciences, 2013). In addition this statement is wrong in another way, since methane dynamics actually are not important at all in the peat carbon uptake, which is what the authors focus on in their manuscript. This passage needs revisiting. (Page 4, line 91 to page 5, line 97).
Furthermore, I asked the authors to provide a table with site locations used in their assessment at site level – instead they added a reference, which is inadequate. The aim of my request was to enable readers to quickly understand where these sites are, without requiring the original publications. In addition, the discussion of site reults is lacking some of the detail contained in the original manuscript.

I also asked the authors to describe how the change in peatland extent was determined. However, I was unfortunately not able to understand that from the description in the paper (page 9, lines 188-205). I understand the link between basal age and peatland extent the authors used to determine changes in peatland area, but that is very difficult to understand from the text since the connection is not made clearly. Please reformulate to make it clearer.

Page 13, line 277 refers to table 4, but this is table 2 in the revised version. I have not been able to check whether all other references to changed Figures and Tables are correct – I suggest the authors check this again before final publication.