Response to Referee #1 (Dr. Kleinen)

We would like to thank Dr. Kleinen for his thoughtful and constructive review. Our responses to all of the referee's comments are italicized below.

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. Overall, the manuscript is quite interesting and suitable for publication in biogeosciences. However, a number of minor issues remain before it is ready for publication.

We agree to make all edits and changes brought up by the referee. We respond to the comments below.

Figure 2 shows the vegetation distribution used to drive the model for 5 time slices. However, results are presented in Figs. 7 and 9 for 6 time slices. This is confusing for the reader. At the very least it needs to be clearly marked in the figure caption. The symbols used in Fig. 2 to show peat basal dates are also very difficult to make out. Maybe it is possible for the authors to make this Figure clearer.

We have five vegetation distribution maps for five time slices. In Fig. 2, we present those five maps. We present six maps of carbon density distribution in Figs. 7 and 9. The reason is that during 5 ka-1900 AD, there was climate change affecting carbon accumulation rates, although the vegetation map remains the same during 9ka-1900 AD. Yes, it is indeed confusing for readers. We agree to make explanation in the figure caption to clarify this. Also, we agree to make all the figures clearer and change the symbol size much larger.

Figure 7: Figure caption is unclear. How can you show cumulative SOC density? This would imply only the very last time in the time interval is shown. I assume you actually mean the mean SOC density.

Correct. For non-peatland soils, we want to show the mean carbon density during each time slices, as there is no significant change from period to period. We agree to make the caption clearer to readers and change the "cumulative" to "mean".

Figure 12: Caption also unclear. I assume the late 20th century distribution is shown? Figure caption shows 15ka to 2000AD, implying a mean value over this time frame.

The maps of total SOC density in Alaska are the sums of SOC during all periods, from 15ka, which is the beginning of the simulation, to late 20^{th}, which is the end. This is the cumulative carbon density, which is the amount at the very last year. We will make the caption much clearer.

Since a large part of the results hinges on the modelled changes in peatland area, it is essential that a description of how area changes are determined is provided. Currently it is only stated that area is prescribed from Matthews & Fung, implying no change is possible.

Page 15, line 339 – page 16, line 349: how were peatland area changes determined? Completely unclear (see comment #4)

The change of peatland area is determined by the basal ages of the peatland. And from the distribution of the basal ages in Fig. 2, we link some vegetation types during each time slice to the ages. Say, during 15-11ka, the peatland was formed based on the alpine tundra (Table 2). However, within each pixel, we assume the inundated area represents the peatland area and other area represents non-peatland. This is from the modern inundation map and does not change. The amount of pixels which have peatland vegetation types is changing through time determined by the basal age distribution. The percentage of peatland cover within each pixel is unchanging. We agree to discuss more on this.

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. Thanks for pointing out the wrong statement here. We agree to check the related references and make changes of the statement.

Page 5, line 97: Why do you cite Kleinen et al. 2010? They do not use a processbased peatland model, but rather prescribed peat accumulation. I assume you actually meant to cite Kleinen et al. 2012?

We will cite Kleinen et al. (2012) instead.

Page 7 and 8, lines 159-166: The aboveground vegetation in your calibration site is significantly different from the Mer Bleu site you use for belowground calibration. In addition the climatic situation at the two sites is significantly different. Therefore is seems quite a stretch to argue that belowground processes are basically the same. Please provide more justification for this assumption.

Assuming the belowground carbon in Mer Bleu is the same as that in APEXCON would be wrong. We agree to make further justification and if necessary, we will replace this value by a more persuasive one and re-calibrate the model.

Page 8, line 173: Please correct date for late Holocene time frame

We will correct the date from “9 ka-1900AD” to “5ka-1900 AD”.

Page 9, lines 194-195: The Shuttle Radar Topography Mission (SRTM) only covered latitudes 56S-60N. Therefore there is NO SRTM data for Alaska. You obviously used some other source for topography data – please provide correct reference.

We directly used the data from He et al. (2014). We agree to find the correct source of the elevation data and will cite the correct reference.

Page 9, lines 197-203: Downscaling / bias correction is unclear. From the text one gets the impression, that ECBILT fields and CRU data may be significantly different for 20th century. However, my reading of the original publications is that bias correction minimised that difference. Please clarify this – it would strongly strengthen the text.

Yes, the climate data were bias-corrected to minimize the difference between ECBILT and CRU. We agree to make clarification on this.

Page 12, line 256: References to Figures 2 and 3 mixed up, please correct.

We will change the sequences of the references to correct such mistake.

Page 13, lines 279-289: Study sites unclear. Please provide table of site locations.

Below is the table of descriptions of those sites. We agree with adding this table or citing it in Wang et al. (2016) JGR-Biogeosciences paper.

Sites used for comparison of carbon accumulation rates between simulation and observation [*Jones and Yu,* 2010]

Site name	Location	Peatland type	Latitude	Longitude	Dating method	No. of dates	Basal age (cal yr BP)	Time-weighted Holocene accumulation rates (g C m^{-2} yr^{-1})
Kenai Gasfield	Alaska, USA	fen	60º27'N	151º14'W	AMS	12	11,408	13.1
No Name Creek	Alaska, USA	fen	60º38'N	151º04'W	AMS	11	11,526	12.3
Horsetrail fen	Alaska, USA	rich fen	60º25'N	150º54'W	AMS	10	13,614	10. 7
Swanson fen	Alaska, USA	poor fen	60º47'N	150º49'W	AMS	9	14,225	5.7

Page 14, lines 314-316 and table 4: table 4 only lists uncertainty ranges for peatland vegetation. How were uncertainty ranges for upland vegetation derived? Certainly not from the ranges in table 4. Please clarify. Page 15, lines 333-334 – see comment #13.

Thankfully, the authors have used a spellchecker, so there are very few typos. However, there are numerous places in the text, where grammar needs checking: Temporal forms are not always consistent, and some sentences are missing single words or larger parts. Therefore CAREFUL COPY-EDITING is highly important.

The uncertainties of the upland vegetation are from the uncertainties of parameters in previous study (Tang and Zhuang, 2008; 2009). We will clarify this and add the ranges of parameters.