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Interactive comment on "Beneath the arctic greening: Will soils lose or gain carbon or perhaps a little of both?" *by* Jennifer W. Harden et al.

Anonymous Referee #1

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The ongoing climate change is affecting permafrost soils and the carbon stored within in a very drastic way. Big uncertainties exist about the vulnerability of millenial old carbon to be mineralized and leave the thawing soils via CO2 or CH4. Thus the study aims for the better understanding of highly important topic, the long term fate of permafrost soil carbon. I have a strong major concern with the layout of the study. Using a space for time approach the authors compare three single soil pits with thousands of miles distance in between. The authors take the data of 3 soil pits and model soil OC development over 300 years into the future. All uncertainties, all vegetation and climate and parent material differences are just neglected, and the whole model is based on some 14C and C data. The results look feasible (of course there will be depth trends in OC), and they might be if you think a Gelisol might become an Inceptisol and Mollisol

with changing from a 300 mm to 800 mm precipitation ecosystem. But the whole study overstretches the space for time approach by far! It is already complicated to correlate soils in one catchment using this approach, but on completely different parent materials and ecosystems... The warming Arctic and its OC fate is a big topic, but is this worth putting together old data and squeezing it into a questionable modelling approach? You have a nice data set, so maybe its worth rethinking your approach and re-write it with what it is, three single soil pits. Based on that you could really go into detail discussing OM distribution and possibly also stabilization, but not telling a story that "this" Gelisol might be "this" Mollisol in 300 years. Please see detailled comments below: page 2 line 31 and following - If you only look into Hugelius this might be right for the permafrost regions, but there is a growing number of studies on subsoils globally. With this there is also a growing understanding of what drives subsoil C stabilization. There is also already some work on SOM fractions in the Arctic, so maybe worth checking for OM vulnerability in permafrost soils (e.g. Gentsch et al. EJSS 2015; Mueller et al GCB 2015). page 3 line 25-29 I doubt that todays arctic permafrost soils can via a space for time approach be related with Iowa soils. Space for time approaches even when conducted in the exact same ecosystem have a tremendous number of assumptions. In your case you are pushing these assumptions far of a meaningful level. page 3 line 30 and following - I clearly doubt that the research sites can give you a reliable answer. Of course you see differences between the sites, but what are the factors driving these changes, definitely not just a permafrost you find at one spot but not the other! page 4 line 8-10 Please give detailled mineralogy together with pedogenic oxides to show comparability of study sites with respect to aggregation and organo-minerl associations. page 4 line 14 - You get Tephra in at 1 m, so how are you dealing with different nutrient contents? page 5 line 4 - Actually approx. 3000 miles in between sites. page 7 line 8-17 On top of the differences you also compare soils with and without carbonates? Even under comparable climate you'll have differences in OC storage/stability due to carbonates vs. no-carbonates. page 7 line 18 - How representative were these single soil pits for the area (bulk density, mineralogy, C/N etc.), and thus how representative

to relate these soil types? page 7 line 18-22 What density agent was used? Please briefly describe the procedure. page 7 line 21 - What is your "occluded fraction", a light fraction or a mixed particulate together with minerals fraction? page 8 line 2-5 - On what was the OC input based, field data, assumptions? What are the input rates of the fractions? Wer differences in OM chemistry of the input taken into account? page 8 line 7-4 You are taking a modelling approach from a study that models physical OC transport at profile and landscape scale, to model depth functions of OC stability/mean residence time. The assumptions are based on soils from Iowa, but taken to the continuous permafrost Arctic. How are permafrost table depth, root input etc. related to your model assumptions? page 8 lines 15-20 - You are leaving out the unique features of permafrost soils by neglecting the vast amount of OC stored at depth. This also completely neglects soil erosion and changes in hydrologic conditions with permafrost thaw, which are well known to tremendously affect OC storage/fate/turnover. But those would be the step in between your studied sites. page 9 line 1-2 - This assumption is so far off! There are tremendous degrees of uncertainty already for concepts like "storage potential" but definitely for the fate of OC in permafrost soils. You are modelling your data to 200 cm depth, and obviously hit a cryoturbated pocket in the Gelisol in the 14C data. The other soils were sampled much shallower but you assume something underneath, which is definitely highyl speculative especially given the sight underlain by Tephra. With your approach you could also go a step further and include hot aride loess soils in central China. page 11 line 8-14 This is all based on assumptions! You did not measure a single k for any of the fractions. This might be vaque for one site, but for a comparison and especially as a base for forward modelling, this is far off! page 13 line 12 following - The whole paragraph is only based on assumptions! Where is rooting patterns and biomass data? Were is mineralogy/hydrology data? While the 14C data is based on soil horizons as well as depth layers, which kills comparability already. Okay, not to mention you bring up a story on sites thousands of miles away from each other... page 14 line 9-10 - You don′t have any loss between the two. The only thing you have is three sites with different OC stocks, distribution and composition

and you model this data. So you could maybe "assume" differences in these measures between the analysed soils, but to relate them on a timescale of 300 years - this is not based on data! page 14 line 28 and following - This is all right, you demonstrated differences in the distribution of free vs. occluded POM and mineral-associated OM. But you can not draw a line between these distant soil types with respect to one develops from another.

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