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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.

Jennifer W. Harden et al.

jaodonnell@nps.gov

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Please find below our responses to the general comments from Referee 1.

General Comment 1. 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 C1 SOILD Interactive comment Printer-friendly version Discussion paper 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...

We appreciate Referee 1's concerns about our application of the space-for-time substitution approach to this study. However we disagree with the assertion that state factors are "are just neglected." As we state in the manuscript, we aimed to control for parent material across the chronosequence by only sampling late Pleistocene loess soils. While these three soils fall under different taxa, they are off similar origin. We also conducted particle size analysis on all three soil profiles, which we will include in revisions at the recommendation of Referee 2. These data further support our consideration of parent materials across the chronoequence.

Second, we accounted for relief across the chronosequence, which is another state factor described by Jenny (1941). All three soil profiles were sampled on hillslopes. We will add text to the Methods to better describe topographic position of sampling sites.

Third, we acknowledge in the Methods that vegetation and climate have varied across the chronosequence. Figure 1 shows the close relationship between climate (i.e., soil temperature) and time. More specifically, present-day soil temperatures across the chronosequence closely track projected changes in soil temperature for a permafrost site out to 2100 and 2300. While we were not able to control for vegetation, this is a common issue with soil chronosequence studies, including upland fire chronosequences (O'Donnell at al., 2011), peatland thaw chronosequences (O'Donnell et al. 2012), and deglaciation and especially peatland chronosequences (Trumbore et al, 1997). In our next draft we will add text to better support our approach, identify our assumptions, and highlight possible limitations.

General Comment 2. 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.*

We would prefer to maintain the model in the manuscript for the following reasons.

First, a primary objective of the manuscript was to illustrate one possible application of the data through a very simple modeling framework. Through this simple modeling approach, we were able to highlight dynamics of different soil fractions given variable 14C-based turnover estimates in response to ecosystem changes. In many modeling studies, turnover rates for different soil pools are derived empirically from incubation of bulk soils or ecosystem fluxes, not from observations of different pools. Thus, our study represents a novel approach, including both observational and modeling approaches for specific soil C fractions.

Second, another primary goal of our study was to provide some constraints on possible C changes following thawing of ice-rich Pleistocene loess. This is a globally important C pool, and the fate of this C is poorly constrained both by observations and models, particularly at the decadal and century time scale. Other data-driven modeling approaches have used relatively short-term incubation data (month to annual time scale) to drive decomposition rates to estimate the permafrost-carbon feedback (e.g., Koven et al. 2015). Our radiocarbon-based technique is a more appropriate approach for constraining C dynamics over longer time scales.

Third, outcomes of the modeling work should not be interpreted literally (i.e., a Gelisol might become a Mollisol in 300 years). While our simple model is based on observations, results should be interpreted with caution, given the limitations of our approach. The goal of the model was not necessarily to be predictive, but to better understand dynamics associated with ecosystem change. We will add text to the Discussion to

better articulate what we should AND should not conclude from our modeling results.

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