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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 #3

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The present manuscript entitled "Beneath the arctic greening: Will soils lose or gain carbon or perhaps a little of both?" deals with potential responses of soil systems following ecosystem shifts related to climate change. The authors used a space-for-time sequence of soils developed on loess substrates along a cold-to-warm climatic gradient (from soils with permafrost to soils lacking permafrost) with temperatures representative for Interior Alaska in the years 2014, 2100 and 2300. Soil profiles of 2m depth were sampled and analyzed for organic C in bulk soil as well as in density fractions (free light, occluded light and mineral-associated fraction) and 14C data. Using the collected data and modeling, the authors aim at predicting the fate of soil C upon the arctic warming. The major findings are that (i) "depth distributions of organic C were related mainly to depths of rooting and changes in bulk density", (ii) "thawing from the Gelisol to Incep-

tisol in loess parent materials from present to year 2100 resulted in small net gains to soil C, reflecting the net balance between loss of detrital and gain into occluded and mineral associated C" and (iii) "greater warming and shifts from Inceptisol to Mollisol analogous to predicted warming from circa 2100 to 2300 resulted in net losses from both occluded and mineral associated C. . .". The response of C stabilization upon climate change and especially the fate of soil C upon thawing of permafrost is a highly relevant, up to date topic, which will be of interest to a broad readership and it fits into the scope of "SOIL". I personally find the idea underlying this paper nice and the results interesting. However, I have some serious concerns about the used space for time approach – although the authors took care about the parent material (loess deposits) and kept it similar for all three soils investigated there are many assumptions underlying the space-for-time approach that are not discussed or even mentioned at all. In the following, I will list my major concerns / the shortcomings of the approach used which need to be clearly addressed and discussed before the manuscript can be considered for publication in SOIL.

General comments (1) The authors do not provide any data on the parent material except for the fact that it is "loess deposits originating from [. . .] the Matinuska and Knik glaciers (Inceptisols), glacial to post-glacial outwash along the Missouri River and distal loess sources in Nebraska (Mollisols)". The source of the parent material for the Gelisol is not even mentioned at all. Please provide additional information on the soil profiles investigated: (a) where are the profiles located? How far are profiles from each of the three soil types investigated apart from each other? I suggest adding a map of all soil profiles investigated. (b) What about the mineralogical composition of the parent material of each soil profile? What is the texture? Are Fe-/ Al- oxides or hydroxides present? Are carbonates present? All these parameters strongly influence soil C concentrations, amounts of mineral-associated and particulate C as well as C stability in soil. Please give additional information in your table if available. If not available, these factors should at least be clearly taken account of and be discussed! (c) How sure are the authors about the exact development of their soils, i.e. how sure

is the assumption that a Gelisol under Alaskan Black Spruce forest will develop to a Mollisol under a grassland ecosystem? Is it possible that other vegetations/ecosystems would develop (e.g. mixed or deciduous forests) and what would that mean in terms of the C distribution and stability in the soil? (2) The conclusions drawn from the data are a bit too far stretched or let's say formulated too general. E.g. p. 2, l. 8-10 "Thawing from Gelisol to Inceptisol in loess parent materials from present to year 2100 resulted in small net gains to soil C, reflecting. . ." or the sentence thereafter (p. 2, l. 10-12 "Greater warming and shifts from Inceptisol to Mollisol analogous to predicted warming from circa 2100 to 2300 resulted in net losses from both occluded and mineral associated C" – the authors did not observe / measure C gains or losses during soil development, they only PREDICT these upon ASSUMED / PREDICTED soil development during global warming. Therefore, you only ASSUME that this happens. Please phrase your conclusions more carefully.

Minor comments and editorial comments - p. 2, l. 1: The authors write that all profiles developed on similar geologic substrate, i.e. "wind-blown loess deposits". However, on p.5, l. 5-6 they write that Mollisols were formed on "loess deposits originated from glacial to post-glacial outwash along the Missouri River and distal loess sources. . ." - p. 4 "2.1 Study sites": Information on how many profiles per soil type were collected are missing. One can draw the information from the tables, but this is cumbersome – please add here Further, it looks to me that the number of profiles collected differs between soil types (4 profiles for Gelisols, 3 profiles for Mollisols and 14 for Inceptisols – is that correct?) – how was this taken into account statistically / in modelling? - P. 7, l. 18-23: Please provide a bit more information on the density fractionation: (a) what density agent was used? In case it was sodium polytungstate or another salt (b) how did the authors get rid of salt remaining in the sample? Washing? - p. 8, l. 14: different font - p.9, l.8: I suggest writing "The C concentration (C%) decreased with. . ." instead of "C percent decreased with. . ." - p. 9, l. 9: change Molliols to Mollisols - p. 9, l. 8-10: "Parameters of Z* are deeper for Gelisols and Inceptisols than for Mollisols , whereas Zmin parameters are shallower for Gelisols and Inceptisols than Mollisols." First, what

exactly does this mean? A deeper Zmin means that the lowest C concentration was reached in a deeper soil depth? What exactly means a "deeper parameter of Z*"? Second, if I read Table 1 correctly, Z* for Gelisols range from 14.8 to 33.8, Z* for Inceptisols range from 9.8 to 64.3 and Z* for Mollisols range from 35.5 to 56.6. So I would not underline that "parameters for Z* are deeper for Gelisols and Inceptisols than Mollisols"? The same holds true for Zmin. . . Or did I get something wrong here? - p. 13, l. 17: Manuscripts in preparation, i.e. non-accepted, should not be cited. - p. 14, l. 21: write "C" instead of carbon - p. 14, l. 22: delete one dot after ". . .spruce.." - p. 14, l. 23: =0.87 instead of .87 - p. 15, l.23: write "C" instead of carbon - p. 15, l. 26-28: "our space-for-time approach integrates changes in vegetation, climate, and mineral factors to provide. . ." What mineral factors are integrated? Where is the data for that? - Table 1: (A) I would change the order of the soil types from "Gelisol – Mollisol – Inceptisol" to the order of the assumed soil development, i.e. "Gelisol – Inceptisol – Mollisol" (B) I had to search for the meaning of each parameter in the Material and Methods section and this is cumbersome. Please add a short explanation of all parameters listed in Table 1 either in the header or as a footnote (e.g. Cs – surface C, Cmin – minimum C etc.) (C) Additionally, I did not even find an explanation for all of the parameters: What are the adjusted parameters (Zadj, Z*adj, Zmin_adj?) – what did you do to adjust them? This information needs to be added in the Materials and Methods section. (D) Please add units for the parameters given

- Table 2: Please add a column with the soil type here as you did in table 1 - Table S1: Profile HCCN2/3 starts at 24 cm soil depth? What happened to the upper 23 cm?

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