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Interactive comment on "No significant changes in topsoil carbon in the grasslands of northern China between the 1980s and 2000s" *by* Shangshi Liu et al.

Anonymous Referee #1

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General Comments In this manuscript, the authors set out to examine the changes in SOC density in the upper 30 cm of the grasslands of northern China between the 1980s and 2000s, using two computer models algorithms. They utilize soil and atmospheric data from a national database (1980s) and a field campaign (2000s) to compare the net and rate of change in SOCD. The authors conclude that northern grasslands in China remained a neutral SOC sink between the 1980s and 2000s.

Overall I believe that the content of the manuscript is important for the further understanding of the global C system and may prove useful in applying models to predict C behavior in soil systems. However, I have many concerns with the manuscript in its current state. Perhaps the largest concern is that the datasets used to develop

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the models to determine SOCD changes over time are not from the same locations, and differences between the datasets that are due to the spatial differences will result in a bias in the results that cannot be accounted for in the current model. Second, many parameters in the dataset are never discussed nor invoked in the model (or never stated that they are), and may play large roles in the output that are not investigated. Additionally, other absent parameters (e.g. respiration rates) may play critical roles in the modeling output, but are never discussed. These limitations need to critically discussed in the manuscript. Third, a better test of the model would be to utilize the model to predict a SOCD for an area with a known vegetation type, and then directly measure the SOCD in that location to determine the accuracy of the model. Most of the modeling optimization is performed using calculated C values, not direct measurements. For more detailed comments and concerns, see “Specific Comments” below.

Specific Comments 1) Introduction: A sentence or two should be added discussing and quantifying the importance of soil C in northern China. How much C is here? How much is estimated to be fluxing in or out of the grasslands here? Is it small compared to other global fluxes, or is it significant? If it’s small, why should anyone bother with investigating this region? 2) Line 50: “Previous studies”, but no references provided 3) Line 127: Why the top 30 cm? How was this number determined? What is its significance to the study? 4) Line 183: It is reported that the r2 values here as 0.73 and 0.62 for RF and ANN, respectively. These numbers are also provided in Table S1. However, in Figure 3, the reported values are 0.84 and 0.81. What is the difference between the two datasets being presented and compared here? 5) Line 200, Table 2: In the different regions, calculated rates of change values range from -88.9 to 144.65, but the average was determined to be 3.7. How useful will the reported 3.7 value be in a region that exhibits rates closer to 144 or -89? Would it be more useful to account for the extreme spatial heterogeneity, or lump large sections together and utilize their average in global models? In what application are the results the most applicable and useful? 6) Section 3.3: The title of this section suggests that a comparison between soil geochemical parameters and SOCD will be made, but there is no discussion of geochemical parameters here, nor in the discussion section. I recommend simply deleting "and soil geochemistry" from the section title. 7) Figures 6 & 7: C storage and stability is a function of several parameters (e.g. moisture, temperature, vegetation, etc.). Comparing one parameter (e.g. max temperature change) can not thoroughly elucidate information about the behavior and C in the soil, and predict its behavior in a model. Further, using the selected parameters is likely not the most appropriate comparison that can be made, given the dataset available. 8) It is stated that the relationship shown in Figure 9b is significant, with an r2 value of 0.16. Perhaps these data present a general trend, but I would be weary of making big claims about the relationship between MAP and NDVI from this dataset. Further, in lines 298-300, it is stated that Figure (9) demonstrates that alpine grasslands do not show a correlation between SOC and MAP, when Figure 9 shows NDVI (not SOC). Thus, the referenced data do not support the 'suggestion' in this passage. Additionally, the theory of additional precipitation leading to accelerated vegetation growth is valid, but the data in Figure 9 do not support this. 9) In lines 308-310, the authors conclude that a positive feedback was demonstrated in response to climate warming in temperate grasslands. Again, I would be very careful in making large claims based on such weak correlations (r2 = 0.1). 10) It seems odd that the work done in this manuscript is focused on climate change and optimizing models to predict SOCD, but there is little discussion about application of the models and the results. The models use old data/calculations to optimize, calibrate, and validate SOCD output, but for data that already exist and are known. So how can this model then be used to look forward and help "unravel SOC dynamics under climate change", as discussed in the introduction? 11) The results and discussion addressing the third objective of this manuscript, to answer "How these changes are associated with climatic factors and vegetation types", is not satisfying. The 6 different ecological classifications are generically lumped into two throughout the paper, though each of the ecological domains is likely to have a distinct vegetation structure and communities. I would expect that these variables would be discussed and investigated in more detail, and justification be made for the 2-class generalization used throughout the paper if it was appropriate. 12) The supplemental figures are never introduced nor discussed.

Technical Corrections 1) Line 58: Delete period after “soil” 2) Line 66: “nature” should be “natural” 3) Line 67: accounts 4) Line 121: Delete period after “season)” 5) Line 163: Delete “s” from “sites” 6) Line 165: “were” should be “was” 7) Line 166: “extrapolated model predictions” 8) Line 168: Delete “s” from “sites” 9) Line 168: “were” should be “was” 10) Line 169: Use “absence” instead of “lackness” 11) Line 172: “...generated by those sites which had not been...” 12) Line 173: Delete “s” from “sites” 13) Line 235: “an” should be “a” 14) Lines 284, 285: A “,” should be added after “et al.” in both references 15) Line 298: Figure 9? Figure 7 is referenced, but it does not apply. 16) Line 484: Space between “Figure 7”

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