Interactive comment on "Alaskan soil carbon stocks: spatial variability and dependence on environmental factors" *by* U. Mishra and W. J. Riley

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Received and published: 20 August 2012

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We thank the reviewer for the detailed review and thoughtful suggestions that we think will improve our manuscript. Looking at the general, specific, and technical comments of the reviewer, we found the following important suggestions:

"Enlarge the discussion section by including the limitations due to small number of

samples in comparison to the study area and various land use types; discuss possible uncertainty due to sampling depth; for fair comparison, include different soil organic carbon (SOC) estimates made for different land use types by Johnson et al. study; and include two more studies for comparison purposes".

For clarity we respond to general, specific, and technical comments of reviewer separately.

General comments: The lack of data in this situation offers little constraint on model parameters as the observation data itself may not detect SOC change, or may erroneously indicate change. This will probably be true for even very sophisticated spatial modeling approaches that try to minimize these types of errors. Before reporting such a change estimate, I would ask the authors this: how confident are we that the equilibrium model would produce the same magnitudes of change if a perfectly unbiased dataset was available for the same analysis? A discussion on this point would be very welcome. It is worth noting, too, that the authors already appear to have tempered the significance of their results somewhat by listing assumptions and limitations (although note below other uncertainties that I believe were not discussed). However, might there be a way to present the estimate of SOC response to temperature in an even more conservative tone if at all?

Response - We agree with the reviewer regarding the limited number of samples available to conduct more robust geospatial analysis such as regression kriging (see also comment by G. Hugelius). The current sample density of 1 sample per 2587 km2 area and their uneven distribution across Alaska is not sufficient to fully characterize SOC dependence on climate, edaphic factors, and land cover types. We believe that the SOC stock estimate will change, and probably increase, from our estimates if more samples are used, particularly from Yedoma (loess deposits), and deltaic (alluvial deposits) soils that are several meters deep and store huge amount of SOC. We note that our estimate of rate of SOC change due to projected warming is within the lower range of the estimates reported in the literature (P16L1-18).

We discussed the limitations of our study in P16L21-P17L4 and we will further enlarge the limitations section in the revised manuscript as suggested by the reviewer. However, we also note in the manuscript that process based modeling results (ESM's) come with their own limitations (P15L13-17). Current ESMs do not have soil forming processes and have unrealistic assumptions regarding SOC dynamics; as a result large uncertainty exists in predicting carbon climate feedback within ESMs (Koven et al., 2011; Schaefer et al., 2011). As the reviewer suggested, if more SOC observation data become available, preferably on a regular grid across Alaska, the estimates of SOC stocks and potential changes in SOC stocks will change; however, this ideal situation is not imminent, and we are not aware of any such ongoing effort.

If an unbiased high-resolution SOC stock dataset were available, and the complete suite of climate, edaphic, land use, fire, geomorphic properties, hydrology, etc., forcings at the same resolution were available, our approach would still lead to uncertainty in the dynamic response of SOC pools across Alaska for the following reasons. Our approach assumed that the current SOC stock distribution is in equilibrium with a large group of forcings. Further, our change prediction over the next 100 years assumes the system comes into equilibrium with the new forcing, thereby imposing further uncertainty on the estimate. Therefore, the type of change analysis presented here should only be viewed as an indication of the direction and potential magnitude of change, and not as a quantitative unbiased estimate. To our knowledge, the best potential alternative approach available is to use prognostic ecosystem models that attempt to explicitly resolve the myriad processes impacting SOC stocks in a dynamic simulation. However, land models that can be used at large scales lack accurate representation of many (some would argue most) of the necessary mechanisms at the appropriate spatial scale to properly represent SOC dynamics at high latitudes (some would argue anywhere (Schmidt et al. 2011)).

Specific comments: 1) Another way to explain why the lack of data is prohibitive to making SOC change estimates is because the models must extrapolate beyond the en-

vironmental bounds of the observations, as opposed to extrapolating within its bounds where data is adequately sampled. Further, validation techniques are probably not accurate for the domain outside the sampled environmental conditions. One of the most problematic areas is Western Interior Alaska where there are simply not enough data to cover the east-west gradients in precipitation and temperature in this portion of the state. Northern Interior Alaska is also sparsely sampled although it is thought that permafrost occurrence is the highest in this part of the region. Other gradients, in contrast, may be adequately covered, and therefore modeled, such as the east-west direction along the northern coast.

Response - As discussed above in our response to General comments, we acknowledge, both here and in the manuscript, that the lack of spatial coverage for the SOC measurements will lead to uncertainty in our current and future SOC stock estimates. We also agree with the reviewer that there is value in providing this type of analysis and SOC stock estimate to the community while carefully indicating the limitations inherent in our approach.

2) The authors do not mention the uncertainty associated with sampled depth. As mentioned, previous estimates have been limited to 1-m, but there were good reasons for this. Those studies' authors may not have felt comfortable that the bottom of the soil profile was reached in the NCCS dataset. My understanding is that often field crews will sample profiles to the top of the C horizon OR about a 1-m depth. Therefore, it is difficult to know whether the whole soil profile to the C horizon was sampled. Another possibility is that field crews may have stopped when permafrost was reached because of the obvious difficulty in excavating further, regardless of whether or not they reached the C horizon. These sampled depth issues potentially add another dimension to the measurement uncertainty. A comment about how these issues were dealt with would be appreciated.

Response - Because of the problems as indicated by the reviewer and to reduce the measurement uncertainty due to soil thickness, we conducted our study on a soil hori-

zon basis. However, if biases in reporting of the horizons sampled exist (as the reviewer suggests), that would lead to uncertainty in our spatial extrapolation of deep SOC. Our dataset showed observed soil thickness to C horizon in Alaska ranged from 10-450 cm depending on the soil types (18 suborders). A detailed description of soil depth and their spatial variability across Alaska is provided in a separate study (Mishra & Riley, 2012). Adopting soil horizon approach allowed us to estimate the active layer and permafrost SOC stocks separately. Out of total 472 SOC profiles used in this study 21 SOC profiles were from the wetlands (Fig. 2). Because of the irregular distribution of soil samples we do not think that different land cover types have been sampled proportionately.

3) There is very little data on the bulk density of frozen horizons. How did the authors address this problem? Were the equations mentioned from Calhoun et al (2001) and Adams (1973) developed to include frozen soils? Is it possible that your estimates were higher because of the method of predicting bulk density? As mentioned, a likely reason for the larger SOC estimates in this study is that they go past 1-m. Many of these soils will likely be wetland soils with very deep organic horizons. Wetlands are generally considered to be poorly sampled, especially in the Interior. For example, Johnson et al., (2011) found only 6 profiles in the Boreal region sampled past 1-m, and none in Southeast Alaska, whereas there were 25 in the Polar region (using the same NCCS dataset). Was any special consideration made to address this gap? My concern is that even though SWI shows a relationship with SOC pools, very deep wetland soils may still be missed. This is important because although wetlands make up a smaller area (and there is more carbon in them), these soils may not respond as strongly to climate change as upland soils. If they are not adequately weighted into the model, then the modeled change could be inflated. I would not expect a novel treatment of this issue, again because of the lack of data, but the author's thoughts about it would be appreciated.

Response - The Bulk density (BD) for each soil horizons was estimated using pe-

dotransfer functions developed by Calhoun et al. (2001) and Adams (1973). These functions were developed using over 200 pedons and 900 soil horizons; and use soil texture, depth, and organic carbon content of a soil horizon to predict the bulk density; and have been widely used in literature to predict the soil bulk density for different soil types across the globe (all soil orders) (Post and Kwon, 2000; Tan et. al., 2005; Minasny et al., 2006; Mishra et al., 2009; Mishra et al., 2010; Minasny et al., 2011). Since these relationships provide a general relationship to predict soil mineral BD from above mentioned soil properties and account for the amount of organic matter contribution in BD these equations can also be used to predict the BD of Gelisols. The BD values predicted by using this approach were always lower than the observed BD values in the dataset. Therefore, higher SOC stock estimates from this study were not the result of higher BD estimates.

4) The comparisons made to the Johnson et al. (2011) should be taken out or modified. It is not true that estimated SOC stocks of the current study for Boreal Alaska are 5.8X higher than the stocks estimated in Johnson et al. The authors took only the estimates made for the Upland conditions in Table 1 of that study, leaving about Lowland, Sandy Lowland, Silty Lowland, and Wetland. If any comparison is to be made, it would have to use an area weighted average, which would be 16.6 kg m2, or 3X difference. The same applies to the Arctic region, but in this case the mistake was even more obvious because Johnson et al. includes the area weighted calculation and compared it to the Ping et al., (2008) paper. The correct difference between the current study's estimate and that of Johnson et al. for the Arctic is 1.9X, not 1.3X.

Response - We thank the reviewer for providing us the area weighted average SOC estimates for fair comparison (this data was not available in Johnson et al. 2011 study). Our revised estimates were 1.5, 3, 1.6, and 1.6 times as large when study area was stratified using the same ecoregions.

5) There was no discussion about the importance of bedrock as a predictor variable. This is very coarsely mapped in Alaska, but can the author's comment on why it was

significant in their model?

Response - Bedrock type, as an indicator of parent material, is often used to infer properties of soil types (Grimm et al., 2008; Mora-Vallejo et al., 2008; Mishra et al., 2010); therefore they were included in our study. Since bedrock types were not supposed to change in future, we focused our discussion only on environmental factors that will change due to anthropogenic or climatic disturbance.

Technical comments: 1) Is there any reason that there is no discussion about or comparison with the Bliss et al. (2010) paper, which also reports SOC stocks for Alaska? A new study might be of interest to the authors for comparison purposes: Yuan, Fengming, Shuhua Yi, A. David McGuire, Kristofer D. Johnson, Jingjing Liang, Jennifer Harden, Eric S. Kasischke, and Werner Kurz. In press. Assessment of Historical Boreal Forest C Dynamics in Yukon River Basin: Relative Roles of Warming and Fire Regime Change. Ecological Applications. http://dx.doi.org/10.1890/11-1957.1

Response - We compared our results with 4 studies and spent several paragraphs (P11L15-P12L17) discussing differences between these previous studies and ours. We will include the reviewer's suggested studies in the revised manuscript.

2) What was the spatial dataset used to delineate continuous, discontinuous, etc.?

Response - The dataset used to delineate continuous, discontinuous, isolated permafrost types were obtained from the permafrost map of Alaska (Ferrians, 1998), we will add this reference in the revised manuscript.

3) Was anything done to account for Geolocation error, i.e. misclassification error from extracting GIS data to the profile locations?

Response - Over 500 SOC profile observations exist for Alaska in national soil characterization database, we used only 422 of them that had geographical coordinates. As discussed in the Methods section, we did not include profile observations that did not provide geographical coordinates.

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